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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Dr. John New; Pat. Neill, Esq. F.W.S.; Richard Lovell Edgeworth, Esq. F. R. S. and M. R. I. A.; J. G.; W. N.; Mrs. Agnes Ibbetson; Mr. James Thomson; John Gough, Esq.; the Rev. J. Blanchard; Dr. Clarke; Mr. Peter Barlow; Mr. J. Acton; J. S. K.; Mr. Charles Sylvester; Mr. T. Sheldrake; William Saint, Esq.; A Correspondent.

Of Foreign Works, M. Vauquelin; M. Fourcroy; M. Chevreul; M. Bertrand; Professor Link; M. Boullay; Professor Lampadius; L. Cordier; M. Gay-Lussac; M. Thenard; M. Guyton-Morveau; M. Aluau; Professor Mojon; M. Resal; M. Leblanc; B.G. Sage; M. Berthier; M. Gehlen; M. Descotils; M. Bosc; C. L. Berthollet; M. Seguin; Professor Curaudau.

And of British Memoirs abridged or extracted, Thomas Andrew Knight, Esq. F. R. S. &c.; Rev. Jofeph Townshend; Mr. Ezekiel Cleall; W. Bond, Esq.; W. Matthews, Esq.; Mr. Parkinson; J. Christ. Curwen, Esq.; J. Franklen, Esq.; Mr. Samuel Curtis; Mr. Thomas Davis; Mr. Samuel Clegg; Mr. Thomas Saddington; Mr. Wagstaffe; J. Butler, Esq.; Thomas Walford, Esq. F. A. S. and L. S.; Thomas Marsham, Esq. Trea. L. S; George Montagu, Esq. F. L. S.; John Williams, Esq.; Arthur Young, Esq. F. R S.; Captain Kater; Humphry Davy, Esq. Sec. R. S. F. R. S. Ed. and M. R. I. A.; Mr. William Sewell; Thomas Young, Esq. M. D. For. Sec. R. S.

The Engravings consist of 1. Knight's new Method of Training Fruit Trees: 2. Mr. Cleall's Machine for Beating out the Seed of Hemp and Flax: 3. Mr. Bond's Machine for Breaking Hemp: 4. Mr. Samuel Clegg's Apparatus for making Carburetted Hidrogen Gas from Pitcoal: 5. His Lamp for burning the Gas: 6. Different Insects, called Wireworms, that destroy Wheat: 7. The Nasal Membranes of two Species of Horseshoe Bat: 8. Figures to illustrate the Vivification of Seeds, by Mrs. A. Ibbetson: 9. A very sensible Hygrometer, by Lieutenant Henry Kater: 10. An improved Hygrometer, by the fame Gentleman: 11. Mr. Davy's Apparatus for heating Potassium in Gasses, Distilling Potassium, and taking the Voltaic Spark in Sulphur and Phosphorus: 12. Various Figures by Mrs. Agnes Ibbetson, to illustrate the Growth of Leaves, and the Divisions of the Wood in the Stem of Trees: 13. Figures showing the Line of Life in Trees entering into Flower Buds, passing by Leaf Buds, and avoiding an injured Part: 14. Dissections of Seed Vessels: 15. Cryptogamian Plants, that have been mistaken for Perspiration on Leaves: 16. Delineations by the Camera Obscura and Camera Lucida.

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Fruit Trees.

Fig.1.

Fig. 2.

Fig. 3

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MAY, 1809.

ARTICLE I.

On a new Method of training Fruit Trees. By Thomas Andrew Knight, F. R. S. &c*.

FROM the result of experiments I have made to ascertain the influence of gravitation on the descending sap of trees, and the cause of the descent of the radicle, and ascent of the expanding plumule of germinating seeds†, I have been induced to believe, that none of the forms, in which fruit trees are generally trained, are those best calculated to promote an equal distribution of the circulating fluids; by which alone permanent health and vigour, and power to afford a succession of abundant crops, can be given. I have therefore been led to try a method of training, which is, I believe, different from any that has been practised; and as the success of this method has fully an- swered

Usual forms of training trees defective.

A different manner tried with success.

* Trans. of the Horticultural Society, p. 79.

† Phil. Trans. for 1806 and 1807; or Journal, Vol. XIV, p. 409, and XIX, 241.

swered every expectation I had formed, I have thought a concise account of it might not be unacceptable to the Horticultural Society. I confine my account to the peach tree, though, with a little variation, the method of training and pruning, that I recommend, is applicable, even with superior advantages, to the cherry, plum, and pear tree; and I must observe, that when trees are by any means deprived of the motion, which their branches naturally receive from winds, the forms in which they are trained operate more powerfully on their permanent health and vigour, than is generally imagined.

Form of training trees when their branches are deprived of motion, important.

New method described.

My peach trees, which were plants of one year old only, were headed down; as usual, early in the spring, and two shoots only were trained from each stem in opposite directions, and in an elevation of about 5 degrees; and when the two shoots did not grow with equal luxuriance, I depressed the strongest, or gave a greater elevation to the weakest, by which means both were made to acquire and to preserve an equal degree of vigour. These shoots, receiving the whole sap of the plants, grew with much luxuriance, and in the course of the summer each attained about the length of four feet. Many lateral shoots were of course emitted from the young luxuriant branches; but these were pinched off at the first or second leaf, and were in the succeeding winter wholly destroyed; when the plants, after being pruned, appeared as represented in Pl. I, Fig. 1. This form, I shall here observe, might with much advantage be given to trees while in the nursery; and perhaps it is the only form, which can be given without subsequent injury to the tree: it is also a form that can be given with very little trouble or expense to the nurseryman.

Should be commenced in the nursery.

Second year.

In the succeeding season as many branches were suffered to spring from each plant as could be trained conveniently, without shading each other; and by selecting the *strongest* and *earliest* buds towards the points of the year old branches, and the *weakest* and *latest* near their bases, I was enabled to give to each annual shoot nearly an equal degree of vigour; and the plants appeared in the autumn of the second year nearly as represented in Fig. 2. The experienced gardener will here observe, that I exposed a greater surface of leaf

to

to the light, without placing any of the leaves so as to shade others, than can probably be done in any other mode of training; and in consequence of this arrangement, the growth of the trees was so great, that at two years old some of them were fifteen feet wide; and the young wood in every part acquired the most perfect maturity. In the winter, the shoots of the last season were alternately shortened, and left their whole length, and they were then prepared to afford a most abundant and regular blossom in the succeeding spring.

Third year.

In the autumn of the third year the trees were nearly as represented in Fig. 3, the central part of each being formed of very fine bearing wood; and the size and general health of the trees afford evidence of a more regular distribution of the sap, than I have witnessed in any other mode of training.

Necessity of winter pruning should be as much as possible prevented.

In the preceding method of treating peach trees very little use was made of the knife during winter: and I must remark, that the necessity of winter pruning should generally be avoided as much as possible; for by laying in a much larger quantity of wood in the summer and autumn than can be wanted in the succeeding year, the gardener gains no other advantage, than that of having a "great choice of fine bearing wood to fill his walls," and I do not see any advantage in *his having* much more than he wants; on the contrary, the health of the tree always suffers by too much use of the knife through successive seasons.

Remarks on pruning peach-trees.

To enter into the detail of pruning, in the manner in which I think it might be done with most advantage, would of necessity lead me much beyond the intended limits of my present communication; but I shall take this opportunity of offering a few observations on the proper treatment of luxuriant shoots of the peach tree, the origin and office of which, as well as the right mode of pruning them, are not at all understood either by the writers on gardening of this country, or the Continent.

The alburnum a reservoir of sap in winter.

I have shown in the Phil. Trans. for 1805*, that the alburnum, or sap wood of oak trees loses a considerable part of its weight during the period in which its leaves are formed

* See Journal, Vol. XII, p. 233.

 in

in the spring; and that any portion of the alburnum affords less extractive matter after the leaves have been formed than previously. I have also shown, that the aqueous fluid which ascends in the spring in the birch and sycamore becomes specificially heavier as it ascends towards the buds; which, I think, affords sufficient evidence, that the alburnum of trees becomes during winter a reservoir of the sap or blood of the tree, as the bulb of the hyacinth, tulip, and the tuber of the potato, certainly do of the sap or blood of these plants. Now a wall-tree, from the advantageous position of its leaves relative to the light, probably generates much more sap, comparatively with the number of its buds, than a standard tree of the same size; and when it attempts to employ its reserved sap in the spring, the gardener is compelled to destroy (and frequently does so too soon and too abruptly) a very large portion of the small succulent shoots emitted, and the apis too often prevents the growth of those which remain. The sap in consequence stagnates, and appears often to choke the passages through the small branches; which in consequence become incurably unhealthy, and stunted in their growth: and nature then finds means of employing the accumulated sap, which if retained would generate the morbid exudation, gum, in the production of luxuriant shoots: These shoots our gardeners, from Langley to Forsyth, have directed to be shortened in summer, or cut out in the succeeding spring; but I have found great advantages in leaving them wholly unshortened; when they have uniformly produced the finest possible bearing wood for the succeding year; and so far is this practice from having a tendency to render naked the lower, or internal parts of the tree, whence those branches spring, that the strongest shoots they afford invariably issue from the buds near their bases. I have also found, that the laterals that spring from these luxuriant shoots, if stopped at the first leaf, often afford very strong blossoms and fine fruit in the succeeding season. Whenever therefore space can be found to train in a luxuriant shoot, I think it should rarely or never be either cut out, or shortened: it should, however, never be trained perpendicularly, where this can be avoided.

Wall trees generate more than standards.

Luxuriant shoots should not be shortened.

II.

II.

On the Food of Plants, by the Rev. JOSEPH TOWNSHEND, *Rector of Pewsey, Wilts**.

WHAT is the food of plants? Before we can give a satisfactory answer to this question, we must collect facts; we must multiply experiments. For this purpose, in the years 1792 and 1793 I put various seeds to vegetate in different airs; in atmospheric air, in vital air, and in azote. The general result was that neither wheat, oats, nor barley, vegetated in azote; but in vital air vegetation was uniformly rapid. Seeds vegetate rapidly in oxigen; and not at all in nitrogen.

July 12, 1796, I placed eleven cabbage-plants in pots, all healthy plants, and weighing each ¼ ounce apothecaries' weight. The pots stood in pans with water, and remained in them till June 12, 1797, when the plants were taken out of the pots and weighed again. Cabbage plants,

Of these pots four had quartz sand, washed clean, and rendered perfectly free from mixture of either argil or calcareous earth,

No. 1 had nothing but this sand; the plant lived, but did not increase in bulk; when examined, the radical fibres were found numerous and extended, but very small; and when the plant was weighed in January 1797, it had not increased in weight. in pure quartz sand,

No. 2 had the same kind of sand and woollen-rags: the roots shot vigorously, the plant cabbaged, and in January 1797 weighed two ounces. sand and woollen rags,

No. 3 had the same kind of sand, with about ¼ part charcoal in powder; the roots were less vigorous than the former, and in January 1797 the plant weighed ¼ ounce. sand and charcoal,

No. 4 had this sand with about $\frac{1}{30}$ lime. The plant did not increase, yet lived, and in January 1797 weighed only 3 dwts. having lost ⅖ of its original weight. sand & lime,

No. 5 had brickmaker's clay alone; the plant lived, looked fresh, but in January 1797 weighed only ½ ounce. brick clay,

No. 6 had brickmaker's clay, with an equal proportion of the clay and sand,

* Bath Society's Papers, vol. X, p. 1.

the quartz sand. This plant, like the fomer, lived, looked fresh, and in January 1797 weighed ½ ounce.

clay and charcoal, No. 7 had brickmaker's clay, with about ¼ part charcoal in powder. In January 1797 the plant weighed ½ ounce.

clay and rags, No. 8 had brickmaker's clay and woollen rags. This plant cabbaged well, and in January 1797 weighed 4 ounces.

clay and lime, No. 9 had brickmaker's clay, with about $\frac{1}{10}$ lime. The plant lived till December, but never grew.

sand and horse-dung, No. 10 had clean dung from the bowels of a horse, with quartz sand well washed. This plant dropp'd some of its largest leaves during the frost; and yet in January 1797 it weighed 4½ ounces.

peat earth, No. 11 had *peat* earth alone; the plant continued healthy to appearance, and in January 1797 weighed ½ ounce, but the root was rotted off.

& rich mould. No. 12 was planted at the same time in the garden, near the pots, in rich mould: this did not drop any leaves, and in January 1797 weighed 4 ounces.

Such was the result of these experiments on cabbage plants.

Wheat sown, In January 1797, having removed the cabbage plants, I sowed wheat in the same pots; and 25th September of the same year I made the subsequent report.

in sand, No. 1, with quartz sand alone, had two stems, 23 inches long, and the ears 1¼ inch.

sand and rags, No. 2, the sand and rags, had four stems, 28 inches long, and the ears 2½ inches.

sand and charcoal, No. 3, the sand and charcoal, had one stem, 18 inches long, and the ear 1¾ inch.

sand and lime, No. 4, the sand and lime, had two stems, 21 inches long, and the ear 2 inches.

clay, No. 5, the clay alone, had three stems, 27 inches long, and the ears 1¼ inch.

clay and sand, No. 6, the clay and sand, had four stems, 25 inches long, and the ears 2½ inches.

clay and charcoal, No. 7, the clay and charcoal, had four stems, 24 inches, and the ears 2 inches.

clay and rags, No. 8, the clay and rags, had twelve stems, 33 inches long, and the ears 2½ inches.

No.

No. 9, the clay and lime, had one stem very slender, 15 inches, and the ear 1¾ inch. clay and lime,

No. 10, the dung and sand, had sixteen stems, 37 inches long, and the ears 2¼ inches, very strong. dung and sand,

No. 11, the peat earth, had six stems, 35 inches long, and the ears 2¼ inches. peat earth.

Thus, it appears, that in both sets of experiments the results were similar.

Is water the food of plants?

From these facts, compared with other facts with which we are conversant; such as the flowering of bulbous roots in water, and more especially the vast increase of the withy-tree, recorded by Mr. Boyle, our attention is naturally turned in the first place to water, as the supposed nutriment of plants.

Water used with the sand.

In the experiments before us, both the cabbage and the wheat of No. 1 were well supplied with water; but in the space of six months the former had not increased in either weight or bulk; and the latter in eight months produced only two miserable stems.

A sandy soil productive.

In Catalonia, more especially in the vicinity of Barcelona, the soil is principally quartz, from decomposed granite; yet being well watered, and plentifully supplied with light and heat, the crops of every kind are most abundant.

Quality of the air as to heat and moisture important.

Mr. de Saussure remarks, that "we deceive ourselves exceedingly when we imagine, that the fertility of any district depends wholly on the nature of its soil, because abundance and scarcity in crops arise principally from the degree of heat and humidity in the air, with the quantity and quality of the exhalations with which it is charged." He adds, "I have seen, in Sicily and Calabria, rocks and gravel arid and uncultivated, such as in Switzerland would have been altogether barren, which there produced more vigorous plants than are to be seen on the richest and best cultivated lands amongst the Helvetic mountains.*"

Effects of well watering in a warm climate

It is astonishing to see, in a warm climate, the rapid growth of vegetables when they are well supplied with water. The smallest cutting of a vine will in the space of fifteen or sixteen months cover the front of an extensive edifice, or form

* Voyage dans les Alpes, 1319.

a spacious

a spacious harbour, from which the assembled family may gather in abundance of the most luxuriant grapes. In such a situation the seeds of limes, oranges, and lemons, will in four or five years produce a shady grove; and mulberry trees, when wholly stripped of their leaves for the nutriment of silk-worms, will again, in a few days, be covered thick with foliage.

Adanson, in his account of Senegal, informs us, that "when every thing green has been devoured by locusts, not a vestige of their destructive progress after a few days can be discovered."

Water decomposed both in animals and vegetables.

From the consideration of these and other facts similar to them, many distinguished chymists have delivered it as their opinion, that water is decomposed by vegetables. Mr. Chaptal says, "that the decomposition of water is *proved*, not only in vegetables, but in animals also." And for this last he quotes the authority of Rondelet.

But this not yet demonstrated.

That water, as such, enters largely into the composition of vegetables, is evident; but whether or not, and to what extent, it is decomposed, has not, as I apprehend, been yet demonstrated. In water meadows, with a plentiful supply of *running* water, vegetation proceeds even in the depth of winter, and during the severest frosts; but stagnant water is at all times unfriendly to our meadows. Any given quantity may remain upon the surface for weeks or months subject to decomposition; but instead of being in this state beneficial, it is injurious to our crops. In our water meadows we universally observe, that it is not humidity which does good, but a thick sheet of water flowing incessantly, night and day, (for a certain period) over the surface.

Probably it is a vehicle of other substances.

Hence it seems probable, that water is essential to the growth of plants, not merely as such, but as it proves a vehicle of other substances, which are their *proper food*.

Perhaps carbon their chief food.

If we may form a judgment from their analysis, *carbon* may be regarded as the chief pabulum of plants; and this we know can, in a given proportion, be conveyed to them by water. Mr. Chaptal is not only of opinion, that carbonic acid is essential to their growth, but he affirms, that the base of this acid contributes to the formation of the vegetable fibre. In support of this opinion he observes, that in fungi, which

which live in subterraneous places, this acid abounds; but by bringing them from almost perfect darkness gradually to the light, this acid disappears, and the fibres proportionably increase. This opinion is confirmed by some experiments of Mr. Senebier, in which he observes, that "plants abundantly supplied with water, which had been impregnated with carbonic acid, transpired much more oxigen, than when they were supplied with common water."

Some plants have more than others.

Some plants take more *carbon* than others into their composition; as for instance, the *agaricus quercinus, agaricus antiquus, boletus versicolor, boletus igniarius, boletus striatus, boletus perennis, clavaria hypoxylon, clavaria pistillaris*, and many others. All these contain, from the result of analysis, a quantity of carbon, nearly equal to all their other component parts. But the *lichen crispus, pinaster granulatus*, and *lycoperdon tessellatum*, contain a very small portion of carbon.

They do not retain all they receive.

Plants do not however retain all the *carbonaceous* matter they receive: they obtain more in the day when exposed to light, than they naturally require; but by the absence of light they part with this surplus, and therefore yield respirable gas only in the day-time.

Oxigen separated from them by light and hidrogen.

The separation of oxigen from plants by radiant light seems to arise from the chemical affinity between oxigen and light. For this fact we are indebted to Dr. Ingenhousz; but Humboldt was the first who ascertained, that hidrogen gas applied to plants, even when excluded from the light, occasions a separation of their accumulated oxigen.

Oxigen retained with different force, and affects the colour.

Some plants, as for instance, *tremella nostoc*, the *filices, musci*, and *algæ*, retain their oxigen weakly, and part with it readily. And it is remarked by Van Uslar, to whom I am indebted for many of these observations, that such plants as contain much oxigen, and retain it obstinately, are white; as for instance, our endive and celery, when excluded from the light; while such as contain much oxigen, and part with it easily, are generally green.

Plants require vegetable mould.

If the analysis of plants leads us to consider *carbon* as one of the most essential articles in their composition and support, no less does the existence of ages prove to us, that the principal source from which they derive their nutriment,

whatever

whatever it may be, is to be sought for in *vegetable earth*, the produce of animal and vegetable substances decayed. Many plants indeed require little or no earth for their vegetation, such as the numerous *lichens* and *tragacanths*, of which genera the former were discovered by Saussure on the highest of the Alpine granite rocks. In lower situations these form a soil for the geuista, for the cistuses, and more especially for rosemary and lavender, which abound on the most elevated mountains of the Pyrenees. These again, by their decay, form vegetable earth, in which the luxuriant pine trees and the ilex grow.

Valleys.

This vegetable matter, being washed down into the vallies, helps to form and to increase their soil to a considerable depth, and to give them that fertility, which is not readily exhausted.

Soil composed of earths from the hills, & vegetable or animal matter.

When we analyse a soil, we never fail to find it composed of substances derived from a superior level. If the hills are quartsoze, calcareous, argillaceous, or magnesian, so is the soil in all the vallies which communicate with them. But with these earths in a rich soil we find a great proportion of vegetable matter, or of animal exuviæ; and as those are deficient or abound, vegetation languishes, or is exceedingly luxuriant.

Mould.

Good mould abounding with vegetable matters is commonly of a dark colour, pulverises easily, and has therefore what is called a mellow look; but when exhausted or impoverished by frequent crops, the richest soil, such as I have here described, becomes arid, of a lighter colour, compact, and comparatively barren. In a maiden soil, or where every shower of rain brings down from more elevated regions a quantity of vegetable matter, a succession of luxuriant crops may be taken incessantly, without any diminution of fertility. Thus it is in the country newly occupied by the Americans, in Kentucky, on the Ohio, and in the whole extent of territory watered by the Mississippi, or by its tributary streams. Thus also in some parts of Spain, where an extensive plain happens to receive the spoils of rich circumjacent hills, as in the well-watered vale of Orihuela, near Murcia, of which they say, "Let it rain or not rain, corn never fails in Orihuela." Indeed, so productive is wheat in

Some will bear continual crops:

this

this highly-favoured district, that the farmers commonly receive 100 for 1 upon their seed.

In my experiments, No. 10, we see, by the luxuriant growth of the cabbage and the wheat, what vegetable matter can produce. For in neither of these could any kind of nutriment be derived from the quartz sand in which they spread their roots. Vegetable matter.

The same kind of sand, in the vicinity of Barcelona, is by the assistance of a bright sun and copious irrigation rendered exceedingly productive; but then they spread upon the land all the dung they can procure, and not only station children and old women on the highways, with little baskets to collect this manure as it falls from horses or from mules, but like the farmers in the south of France they pick the leaves from the trees in autumn, and this at a considerable expense. Of such importance do they consider vegetable matter as the food of plants. Its importance in sandy soils.

It must be confessed, that we have frequently occasion to observe plants dependant on the nature of the earth in which they are found, and affecting each its peculiar earth, in which they grow spontaneously and thrive. Plants affect peculiar earths:

Thus on chalky and calcareous soils we find *thesium linophyllum*, *anthyllis vulneraria*, *asperula cynanchia*, *lotus corniculatus*, *hippocrepis comosa*, *poa cristata*; and three of the *sedums*, the *s. acre*, *s. album*, and *s. reflexum*; as on the Wiltshire downs and on the hills round Bath. as chalk;

On sand we see *arenaria*, *rumex acetosella*, and all the sorrels; the *plantago maritima*, the *plantago coronopus*, the *onopordum acanthium*, the *sedum anglicum*, and most remarkably the *spartium scoparium*. sand;

On clay, if wet, the *carices*, the *junci*, *schoenus*, *aira cespitosa*, and *aira cœrulea*, *orchis latifolia*, and *orchis conopsea*; wet clay; if dry, the *primula veris*, *orchis mas*, *orchis maculata*, and *poa pratensis*. dry clay;

On bogs, the *equiseta*, *vaccinium uliginosum*, *anagallis tenella*, *scirpus palustris*, *menyanthes trifoliata*, and *drosera* delight to dwell. bogs;

On the sea-shore, and wherever the muriatic salt abounds, as near Alicant in Spain, we find *salicornia Europæa*, four or the sea-shore.

species

species of *salsola*, *chenopodium maritimum*, and two species of *Mesembryanthemum*.

Part of the soil decomposed.

These maritime plants appear to decompose a part of the soil in which they grow; the alkali produced by burning them, or the sal sodæ used in glass and soap, is evidently derived by them from the muriatic salt.

But earths not their food.

But when we see the *lichen parellus* fixing itself on the siliceous rock, or the *lichen immersus* affecting as it does the calcareous rock, in preference to the siliceous; whatever may influence this choice, we cannot suspect, that either of these rocks contribute by its decomposition to the nutrition of these plants; nor as I apprehend, have we reason to imagine, that either chalk, sand, or clay, is in any form the aliment of the plants.

Woollen rags very beneficial.

Woollen rags have been found of great utility as a manure, more especially for *wheat*. And in the experiments before us we may observe, that sand with rags produced a cabbage of two ounces, and four strong years of wheat. In clay with rags our cabbage weighed four ounces, and we had twelve strong years of wheat. But in what manner these rags produced effect it is difficult to say; for in January 1797 they were not visibly decayed; and in the month of September in that year they still retained their texture. The quantity we usually spread upon one acre is not more than four or five cwt.; and yet in the experience of every farmer it is found, that in the first year they nearly double the crop of wheat; and in the two succeeding years they yielded a visible increase. At present, therefore, we can merely record it as a fact, that woollen rags are highly beneficial to the land: but we cannot pretend to say by what process they contribute to the nutriment of plants.

Lime injurious.

Lime in our experiments was clearly detrimental with sand; the cabbage lived, but weighed less in January than when planted in July: the wheat had two slender stems. In clay with lime our cabbage lived till December, but never grew. The wheat had one stem, which was extremely slender, and the ear was diminutive.

Experiments seemingly dis-

These facts appear discordant with the experience of farmers in every quarter of the globe; for lime is found to be an

an excellent manure. In some parts of Wales they have scarcely any other dressing for their wheat. I well remember, that in the parish of Lansamlet, in Glamorganshire, my father, who was very attentive to agriculture, put most of his stable dung on meadow land, and used only lime for wheat. He had two lime-kilns constantly burning for his own use, and with this manure he obtained the most abundant crops; but then his land was principally a dark vegetable mould, and much of it was peat, which before it was drained had been a bog. On this land I have counted sixty grains to an ear, not picked and culled out of many others as being longer than the rest, but taken by handfuls at random.

agree with experience.

In his land, lime as a dressing was particularly apt, because, as we know, it hastens the putrefactive process, and promotes the dissolution of vegetable substances, converting them quickly into vegetable mould.

Attempt to reconcile them.

Now in my experiments there was no vegetable matter to be dissolved, and therefore no benefit according to chymical principles was to be expected from the lime. The trial was however made, and the received opinion as to the effect of lime is thus far confirmed.

But in my experiments the lime appears to have been deleterious. This was not from its causticity, for the plants lived; but from its action as a cement in forming a crust on the surface of the pots impervious to air. For in these pots I remarked, that after rain the water stagnated, and did not readily penetrate as in the other pots.

Injurious by forming a crust on the surface.

Free *access of air* to the *roots of plants* seems to be of vast importance, and almost essential to their growth. With regard to seeds, access of air is absolutely needful to their vegetation. Hence it is that charlock *(sinapis arvensis)* will remain in the earth for centuries, if deposited below the vegetating distance, as we have occasion to observe on Salisbury-plain, where no charlock is ever seen, unless when the downs are broken up. The land is then covered with it; but till then the seeds remain as in *vacuo*, and are therefore not liable to change.

Access of air to roots and seeds necessary.

Seed vegetating

This deposit of seed must have happened in most remote antiquity, either when the hill country, like the low lands, formed

after having lain in the

formed part of an extensive forest; or more probably when these extensive downs were subject to the plough. *ground for ages.*

Being solicitous to know whether these seeds were antediluvian, I took earth from different depths, and soon got below the stratum in which these seeds are found.

The necessity of air for the vegetation of seeds will account for effects which in agriculture are too frequently observed.

Injurious effects of a hardened surface. If soon after wheat or barley has been sown on what is called a running sand there falls a dashing rain, the sand runs together, that is, it forms a crust, which in a great measure is impervious to air, and scarcely a grain of corn will grow; or if on clay land, during a time of drought, a garden plot is watered, and left exposed to the scorching beams of a meridian sun, the ground will *bake*, that is, the surface will be hardened, and being thus rendered impervious to air, vegetation ceases. *Prevention.* But if the surface has been previously covered with fern leaves, as practised by skilful and attentive gardeners, no such effect will be produced. The plot may be watered and vegetation will be rapid.

Advantage of harrowing crops, The admission of air, and its vast importance to the growth of plants, will account for the good effect produced by harrowing our wheat crops in spring, as lately introduced, and now universally adopted by our best farmers. *or hoeing them.* The good effect produced is made apparent by the luxuriant growth of pease, beans, turnips, and cabbages, after they have been hoed; and is at present so well understood, that many agriculturists hoe their turnips twice, and their beans four times, not merely with a view to the destruction of weeds, but because they observe the benefit arising to their crops by a free admission of air into the earth. The palpable advantage of this practice has led many farmers to consider the principles on which the practice has been founded, and to try by experiments how far it can be pushed.

Fallowing rendered unnecessary. In this pursuit, and satisfied of the benefits to be derived from loosening the surface of the ground contiguous to his crops, the Rev. Mr. Close has given up the broad-cast husbandry, keeps the hoe constantly in motion, and now finds that he has never occasion for a fallow.

But

But the most astonishing effect produced by giving free admission of air to the roots of wheat was last year exhibited by Mr. Bartley, secretary to the society of Arts at Bath. In August 1800 he sowed his wheat in rows with three feet intervals, and six inches distance from grain to grain. The proportion of seed was two quarts to an acre. The soil was a deep sandy loam, but out of condition, and filled with couch. This wheat was hoed in autumn, hoed again, and earthed up both at Christmas and spring. When it was in bloom the intervals were dug up, and it was once more earthed up. At harvest this crop yielded sixty-six bushels per acre. Such was its luxuriancy, many of the plants produced 98 perfect ears, many of which, nine inches long, contained each 100 grains. Astonishing affect of admitting air to the roots of wheat.

In the broad-cast husbandry of the hill counties of Wilts and Hants, the produce was formerly three or at most four for one, as it was in the greatest part of France. By the drill, without hoeing, the return would not be near so much; but in Mr. Bartley's crop we see more than 1000 for 1; and some grains yielded nearly ten times as much*.

I shall make but one observation more upon this subject, which is, that an orchard planted on the green sward requires double the time for its maturity as one on cultivated land, that has a more plentiful supply of air admitted to its roots. Orchards.

Thus we see that all the great agents in nature are concerned in the process of vegetation, and may be considered as the food of plants. But to determine in what manner each contributes to nutrition, must be left to the investigation of succeding generations. Conclusion.

* It must ever be with reluctance, that an exception can be taken against any argument of so able a writer as the present, especially in a matter of alleged fact. But in this instance it seems proper to remark, that the argument drawn from the reported success of Mr. Bartley should be received with caution, on account of the peculiarity of the soil. That soil being remarkably deep, fat, and productive, and within the limits of a nursery-man's garden, near a city abounding with manure, are circumstances not common to other situations. Consequently the result of any experiments made in such a spot is not to be considered as applicable to the general practice of agriculture and planting, on a large and common scale of cultivation. With the necessary allowances which the local advantage above-mentioned suggests, the consequences drawn by this gentleman may still be of importance for the consideration of our practical readers. EDITOR.

III.

Description of a Machine for Beating out Hempseed and Flaxseed, likely to be useful in Canada. By Mr. Ezekiel Cleall, *of West Coker**.

SIR,

Machine for thrashing hemp and flax.

I MADE a model of a machine for thrashing out hempseed and flaxseed, in the year 1803; and in the year 1805, I had a real machine made after the plan of the model, by Mr. John Wadman, carpenter and hemp merchant. The said machine has been since tried and approved by many hemp and flax merchants.

I now send the model for the inspection of the Society, and leave the event thereof to their decision. It does not injure the stalk of the hemp so much as the common mode of thrashing out the seed, and consequently leaves it much better for scaling.

I am, Sir, your humble servant,

EZEKIEL CLEALL.

West Coker, near Yeovil, Somerset,
March 22, 1806.

Certificates.

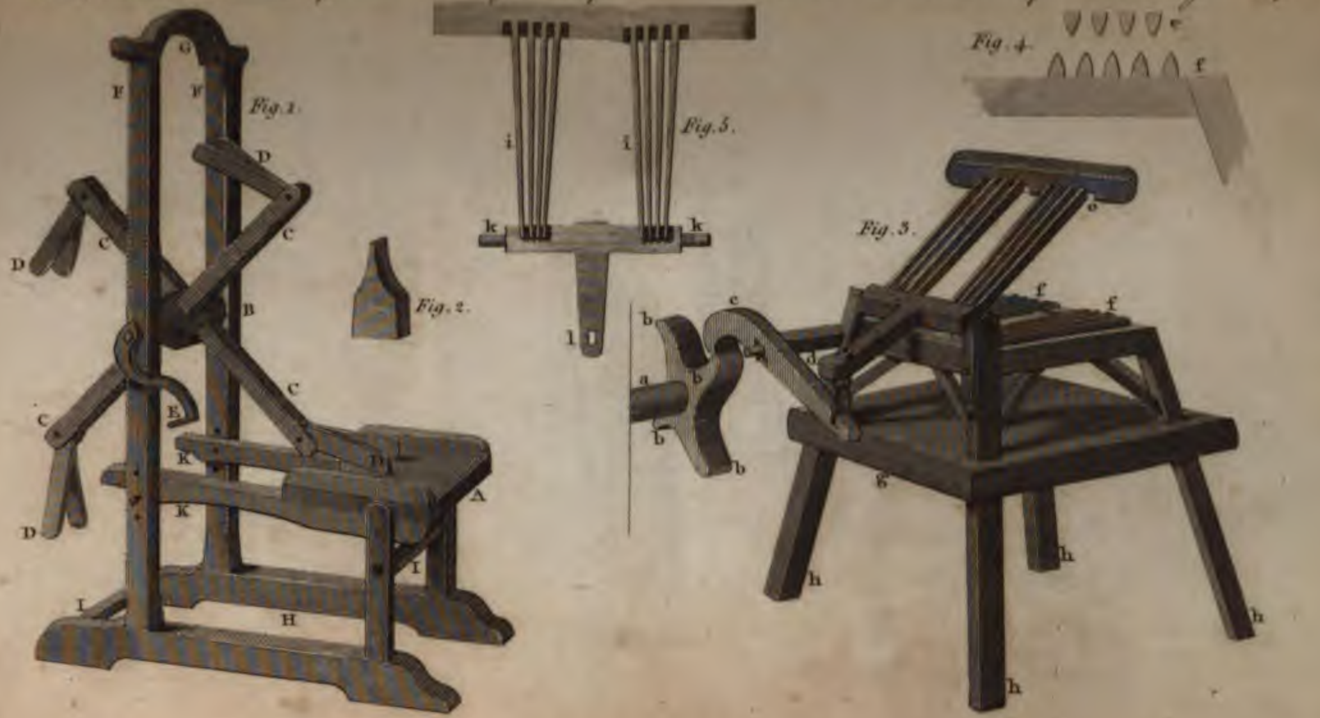
We whose names are hereunto subscribed, do certify, that we well know Mr. Ezekiel Cleall, of West Coker; that we have many times seen his machine at work, in thrashing out hempseed and flaxseed, and think it likely to be of great public utility; inasmuch as two women, whose wages and allowance never exceed one half of what are allowed to two men, will do as much work in any given time as such two men.

That the seeds thrashed by this machine are not so much bruised or injured as by the old or common way, and the hemp and flax are preserved from many injuries which they suffer from the old method.

* Trans. of Soc. of Arts, vol. XXV, p. 143. Twenty guineas were voted to Mr. Cleall for this invention.

In

Mr. Cleall's Machine for thrashing Hemp. Mr. Bond's Machine for breaking Hemp.



In witness whereof, we have hereunto added our signatures.

JOHN WADMAN.
JAMES WADMAN.
JOHN BAKER.
JOHN PINNEY.
JOHN CHAFFEY.

SIR,

THE machine, of which a model was sent to the Society some months ago, must be used with eight flails, two on each arm, for beating out hemp seed. Machine for hemp seed.

When required to be used for beating out flax seed, the above eight flails must be taken out, and four beaters put in their place. For flax seed.

The height of the machine from the floor to the top of the board on which the flax or hemp is laid, is two feet; the breadth, two feet ten inches; the length of the board, four feet four inches; the length of each of the arms, from the axis of the machine, is three feet two inches; the flails for the hemp seed, two feet two inches long; the heights of the uprights, seven feet two inches; the beaters for the flax seeds, are each one foot three inches long, and seven inches broad. Dimensions.

The machine will thrash, in one day, as much hemp as grows on an acre of land, and other crops in proportion; and the work is done with less than half the expense of thrashing in the usual way. Work performed by it.

I am, Sir, your obedient servant,

EZEKIEL CLEALL.

Reference to the Engraving of Mr. Cleall's Machine for beating out Hemp Seeds and Flax Seeds. Pl. II. *Fig.* 1, 2.

Fig. 1. Represents the machine for beating out hemp seeds, in which A is the table or board on which the hemp is to be placed; B the axis in which the four arms CCCC are fixed; D D D D, eight single flails, moving upon four pins near the extremities of the four arms; these flails diverge from the pins on which they move, so that two of them Explanation of the plate.

them united on each arm are nearly in the form of the letter V. E is the winch or handle by which the machine is put in motion; F F, two upright pieces of wood to sustain the axle of the machine; G, an upper cross piece, to secure the uprights firm; H H, the two bottom pieces or sills, in which the two uprights are mortised, also the two smaller uprights which support the board or table A; I I, two lower cross pieces to secure the machine firmly; K K, two levers on which the table A rests, and by which it may be raised or lowered, as thought necessary, by iron pins, at K K, passing through these levers and the two uprights.

Method of using the machine.

When the machine is used, the hemp must be laid on the table A, and moved about in different directions by the person who holds it, whilst another person turns the machine by the handle E; the flails D of the machine fall in succession on the hemp; as the axis moves round they beat out the seeds as different surfaces of the hemp are exposed on the table, and when the seeds are all beaten out from one parcel of hemp, a fresh quantity is applied upon the table.

Flax machine.

Fig. 2. Represents one of the flax beaters, which is made of a solid piece of wood, one of which is attached instead of the two flails, to every arm, when the machine is employed for beating out flax seeds, as they require more force to separate them from the flax plant.

IV.

Observations on the Culture of Hemp, and other useful Information, relative to Improvements in Canada. By WILLIAM BOND, *Esq., of Canada* *.

Observations on the culture of hemp.

Culture of hemp in Canada desirable.

THE culture of hemp in Upper Canada is no doubt one of the most desirable objects with every person of discernment

* Trans. of Soc. of Arts, vol. XXV, p. 147. The silver medal was voted to Mr. Bond for this communication.

ment settled there, and more particularly so with those of this description in our mother country; and though there are so many millions of acres so well calculated to the growth of this highly valuable article, yet I do not expect much progress therein for some time, for the following reasons.

Obstacles to its introduction.

The part of the country the best calculated for the growth of hemp is so lately and in so small a degree occupied, that few have begun to use the plough, but depend upon raising a sufficiency of grain by harrowing only; in this they are not disappointed for two or three crops;—in the mean time they clear away fresh fields from the woods, many of them to a large extent, which take up so much time in fencing and dressing, that few of the farmers have been able to raise more than needful for their own families' consumption, and for the use of their neighbours; indeed they are ignorant as to the growth and management of hemp, and in general so poor, that they cannot afford to raise any thing for sale that will not bring them ready money as soon as brought to market; and grain brings such a high price in cash, that few farmers are inclined to turn their attention to any other article. Another obstacle is, there being no person or persons appointed to buy small quantities of hemp, and pay ready money for the same.

Tract of rich hemp land.

The tract of rich hemp land in Upper Canada is that part west of Yonge Street*, and north of Dundas Street †, and partly enclosed by lakes Ontario, St. Clair, Huron, and Simcoe, and to the east and north-east almost as far as Grand or Ottaway River, and to within a few miles of the south and south-east side of lake Huron. I have not failed to make annually from one to three journeys through this tract; I have crossed it in all directions with Indian guides, great part of which no white man, except myself, has ever set foot in; and I find, that the chief of the interior part consists of a rich deep black soil, which I am well convinced, when well inhabited with farmers, will become one

* A street leading from York, the seat of government, to the navigable waters of Lake Simcoe.

† Leading to the River Thames.

of the finest countries in all his Majesty's territories for the growth of hemp.

But lately begun to be cleared.

It is only about five years since this valuable tract began to be occupied at all, and though by industrious farmers, yet by such as have brought little to the country. A few cows and sheep, a *pair* of plough oxen, one or two horses, a small stock of farming tools, such as two or three axes, as many hoes and iron wedges, one or two ox chains, being the most that a new settler (generally speaking) possesses on his arrival; with these they make a shift to clear away the woods, and divide and fence the land with split timber into fields, and they are greatly encouraged to continue clearing away the forest, in consequence of the high price given for the ashes by the potash makers: this eventually will be vastly in their favour, in future, when hemp becomes the object, as it gives time for the roots and stumps of trees to rot, and their stock of horses and oxen to increase, which is essentially necessary before the farmer can expect to be successful in the growth of hemp. It is in this progressive manner, that this fine country will be settled; the nature of things demands the pursuit; and the first settlers are in a situation capable of putting the same in practice; their stock of horses and oxen are sufficiently strong to work the ground a second time over, tear up the stumps and roots, plough and pulverize the soil; and until the ground is brought to this state, it is not fit for hemp, as hemp, in its nature, depends chiefly upon a tap root, and when this root is interrupted in its progress downwards, it will throw out horizontal ones, which produce horizontal branches also, and the open spaces round the stumps of the trees admitting so much air, permits these branches to grow to such a length and strength as greatly to injure the bark or hemp of the stem. Such hemp, when it comes to the hackle, breaks off, and drags away at the knobs of the branches, so as to leave it short, and make a very great waste. Notwithstanding, if there was a sure market for as small a quantity as 50lb., there are few farmers but would try the experiment; and if one was more successful than the rest, his neighbour would endeavour to find out the reasons why it was so. Thus, step by step, the knowledge in the manage-

Hemp requires a soil well pulverized.

ment

ment of hemp would be greatly extended, the farmer would generally be in possession of fresh seed, and when grain becomes less an object, he would feel no fear in turning his attention to the culture of hemp upon a large scale: and, in order to encourage the farmer, it would prove highly advantageous to take in any quantity, great or small, of sound hemp, assorted perhaps into four or five qualities, according to its length, which will vary for some years to come, for the reasons before given.

High price of labour and of grain.

The high price of labour, owing in some measure to the high price of grain, is such, that hemp, agreeable to the present regulations, is not an object with the farmer; if an addition of about a third of the present price was given, it would be an inducement for the farmers to cultivate their old fields in a more spirited manner; which bounty might be taken off again, when grain becomes less an object than it is at present, which will soon be the case in time of peace, and no doubt will affect the price of hemp in proportion in the English market.

Contrivances for diminishing labour.

In all new countries where labourers are scarce, we find many contrivances calculated for the purpose of reducing labour, more for the sake of expedition than ease; such, for instance, as the saw mill, the hoe ploughs, scythe and *cradle for cutting* and gathering grain, the wooden machine (drawn round by one horse) for thrashing grain, the iron shod shovel, drawn by oxen, and held by two handles, as a plough, for the purpose of levelling the roads, &c. Nor are the Americans, or other settlers in this country, fond of any work that needs violent exercise of the body; which

Disadvantage of breaking hemp in the old way.

the breaking of hemp in the old way certainly occasions, in consequence of requiring a cross motion of the arm, which makes the breakers complain of a pain about the short ribs on the side they hold the hemp; and on the opposite side a little under the shoulders, so that breaking of hemp in the old way is a great obstacle to its increased culture. To render labour, therefore, somewhat more easy and expeditious, is an object worthy the first attention, and I consider it practicable at a small expense, and have sent to the Society a model of a machine for this purpose.

Dash wheels

I have observed among the clothiers' and fullers' machinery,

erected across streams.

chinery, great power and rapid motion proceeding what is commonly called a dash wheel, erected acr stream of rapid water, the flies or float boards of whic fixed in the octangular axis, from fifteen to twenty feet in length, and from three and a half in depth, fly. I have seen many corn mills in Upper Canada, no other water wheels than such as the above descr which save a vast expense in raising dams, &c.

Well calculated for Canada.

There are a number of streams in that part of Ca which I have endeavoured to describe, (as to the prac bility of the various ways of cultivation) that are wel culated for such wheels; and where these streams or are not too wide, the axis of the wheel might be exte across so as to reach the land on each side, where I pr the breakers to be fixed to go by a tilt the same as a hammer. Such a simple piece of machinery would not more than 70 or 80 dollars, as little iron would be wa and timber we have for nothing; and when in motion w employ four breakers and two servers, from whom I sh expect as much good work as fifteen or sixteen pe could possibly do in the old way, and that without n bodily labour.

Mills for breaking hemp.

Mills for breaking hemp, on the very same princip that of a saw mill, as to motion only an addition of an crank, so as to run with two cranks instead of one, something of a larger sweep than that of a saw mill, w be of vast utility in a neighbourhood of a large growt hemp, and would not cost more than a common saw As the brakes of the frame continue in motion the as that of a saw mill, twenty men might be employed, would do as much as fifty or sixty could do in the old way, with much more ease and pleasure to themselves; and

Some collateral advantages.

is not the only advantage that would result from such m it would cause something of a social meeting, which youth would be particularly fond of. At such meeting the defects respecting the culture and management of h would be examined into, and those who raised the would become ambitious, and try to excel each other; we might reasonably expect, that Upper Canada woul

ex

exceed all other countries in the world for the growth of good hemp.

Reference to the Engraving of Mr. Bond's Machine for breaking Hemp. Pl. II, Fig. 3, 4, 5.

Fig. 3. *a*. Represents the axis of a water wheel, on which is fixed a trunnion of four lifters *b b b b*, each of which lifters raises in succession a lever *c*, which, by means of a chain connected with it, pulls down another lever *d*, and thereby raises the upper part of the double brake *e*. As each lifter of the trunnion passes the lever *c*, it allows the upper part of the brake to fall upon the hemp placed on the lower part of the brake *f f*; and by its weight, and teeth intersecting the teeth of the lower brake *f f*, the woody parts of the hemp plant are separated by repeated strokes from the filaments or fibres of the hemp proper for use. This completes the first operation necessary in the preparation of hemp. *g* is a table on which the woody parts of the hemp fall, and which gives security and strength to the frame; *h h h h* are the four legs or supports of the frame. Description of the machine.

Fig. 4 shows a section of the teeth of one half of the double brake abovementioned: it is betwixt the upper and lower rows of these teeth that the breaking of the hemp takes place, by the repeated rise and fall of the upper part of the brake upon it.

Fig. 5 shows the upper part of the brake, in which *i i* show the two rows of teeth, *k k* the two pins on which it is moved, *l* the part to which the chain which raises the upper part of the brake is attached. After the breaking of the hemp, it is wholly finished for use by scutching or swingling, an operation which may be either performed by the hand or machinery, and is easily executed by either mode.

The machinery for breaking hemp should be removed from the rivers previous to the beginning of the frosts.

On the breeding of rabbits.

To include the interest of the colonists and the mother country also in one and the same pursuit, is not only laudable, Interests of colonies and the mother country

try should go together. able, but most likely to succeed; especially where only a trifle of property of the individuals or of the public is wanted to set the bountiful hand of Nature to work in a country where animal subsistence and a suitable climate call for the industrious husbandman, who may in various ways be useful to himself and his country.

Warren rabbit. In my travels through America, I have often been surprised, that no attempt has been made to introduce, for the purpose of propagation, that useful little animal, the warren rabbit, of such vast importance to the hat manufactory of England. It is chiefly owing to the fur of this animal, that the English hats are so much esteemed abroad. It is a fact well known amongst the hatters, that a hat composed of one half of rabbit wool, one sixth old coat beaver, one sixth pelt beaver, and one sixth Vigonia wool, will wear far preferable to one made of all beaver, as it will keep its shape better, feel more firm, and wear bright and black much longer.

Its fur essential to good hats.

Importance of rabbits to this country. The value of the rabbit wool, the produce of the United Kingdom only, is not less, I will venture to say, than £250000 per annum; but the quantity is much diminished, owing to the banishment and persecution they meet with on every side, and so many small warrens taken in for grain land; in consequence of which it is time, that some protection should be afforded, if possible, to that important branch of British manufactory (in which rabbit wool is used) from suffering any inconvenience in the want of so essential an article, and the accomplishment of this grand object I conceive perfectly easy.

The warren rabbit only of value. *General Observations.*—When I speak of the warren rabbit, I have to observe, that there are in England, as well as most parts of Europe, three other kinds, viz. the tame rabbit, of various colours, the fur of which is of little value, except the white; the shock rabbit, which has a long shaggy fur of little value; the bush rabbit, like those of America, which commonly sits as a hare, and the fur of each is of a rotten inferior quality.

Two sorts. To return to the warren rabbit.—There are two sorts in respect to colour, that is, the common gray, and the silver gray, but little or no difference in respect to the strength and

and felting qualities of the fur. The nature of this animal is to burrow deep in sandy ground, and there live in families, nor will they suffer one from a neighbouring family to come amongst them without a severe contest, in which the intruders are generally glad to retire with the loss of part of their coats, unless when pursued by an enemy, when they find protection. Manners.

It is scarcely worth while for me to mention a thing so generally known, viz. that rabbits, particularly those of the warren, are the most prolific of all other four-footed animals in the world; nor do I apprehend any difficulty would attend the exporting this little quadruped with safety to any distance, provided it was kept dry, and regularly supplied with clean, sweet food, and a due regard to the cleanliness of the boxes or places of confinement. Prolific, and easily exported.

Twelve or fifteen pair of these valuable animals taken to Upper Canada, and there enclosed within a small space of ground suitable to their nature, but furnished with a few artificial burrows at the first, by way of a nursery; and spread over those now useless plains, islands, and peninsulas, so well calculated to their nature; would, I will make bold to say, the eighth year after their introduction, furnish the British market with a valuable raw material, amounting to a large sum, increasing every year with astonishing rapidity, so as to become, in a few years, one amongst the first of national objects. Would become highly profitable.

It may be supposed by some, that the above project is magnified beyond possibility, or even probability; but the serious attention I have paid to the subject, these many years past, as to all points for and against, leaves me no room to accuse myself of being too sanguine; for if properly managed a few years at the first, I cannot find a single thing likely to interrupt their progress.

Some idea of the astonishing increase of the rabbit may be had from the following facts:— Increase of a pair in one year.

An old doe rabbit will bring forth young nine times in one year, and from 4 to 10 each time; but to allow for casualties, state the number at 5 each litter.

In

In nine months	45
The females of the first litter will bring forth five times the proportion, of which is 2½ female's produce	62
Those of the second litter 4 times produce	50
Ditto of third ditto 3 ditto	37
Ditto of second ditto 2 ditto	25
Total in one year from one pair	219

The third female race of the old dam, and the second of the first litter, seldom breed the first year, but are early breeders in the spring following, when we might expect an increase of the whole in proportion to the first pair, if properly attended to and protected.

Hares. It is generally allowed, that hares are not more than one fourth as prolific as rabbits, notwithstanding, agreeable to an experiment tried by Lord Ribblesdale, who enclosed a pair of hares for one year, the offspring was (as I have been credibly informed) 68: these animals, could they be exported to Upper Canada with safety, and there protected within enclosures for a few years, would soon after spread over a large extent of country: the fur is nearly as valuable as that of the rabbit.

Climate of Upper Canada. In that part of Upper Canada within 45 degrees of north latitude, and the southern and western boundaries, the climate is nearly the same as that of England, a little hotter a few days in summer, and a little colder a few days in winter, according to Fahrenheit's thermometer, which I have paid great attention to for some years, comparing the same with the observations of the English.

Animals increase fast in America. The increase of most animals appears much greater in proportion in America than in England, mankind not excepted. That of sheep is very apparent to those that pay attention to their breeding stock, which gives me hopes, that in a few years we shall be able to pay for our woollen cloths in wool. Finding the effect of soil and climate so salutary to sheep, &c., it may be reasonably supposed, that rabbits will answer the most sanguine expectations; as I understand the wool of the sheep retains all its nature the same as in England,

England, particularly its strength, and felting qualities among the hatters; which assures me, that rabbits' wool from those bred in Upper Canada will do the same; and there are some millions of acres, within the latitude and boundaries which I have before described, suited to the nature of the warren rabbit; nor do I apprehend that the wolves, foxes, &c. of Upper Canada will be half so destructive as the poachers in England.

The Guanaco,

or camel sheep of South America, no doubt will be a national object at some future period. This is a tame, domestic animal, very hardy, and used with much cruelty by the natives in travelling over the mountains with their burthens. It shears a fleece of wool of from 2lb. to 3lb., which is of a dusky red on the back, on the sides inclined to white, and under the belly quite white; its texture is very fine, yet strong; its felting qualities are very powerful; and it is worth, when ready for use, from five to fifteen shillings per lb. This animal would no doubt thrive, and do well in England, Upper Canada, and in particular I should suppose in New Holland.

The guanaco.

The Beaver

might be propagated to great advantage in Scotland, Ireland, and the northern parts of England. It is an animal, when tamed, very familiar, and will eat bread and milk, willow sticks, elm bark, &c., and no doubt might be imported with safety; but as these two last mentioned animals are not likely to be attended to immediately, I shall say no more respecting them for the present.

The beaver might be introduced into Britain & Ireland.

Pine Timber.

There are many thousands of large pine trees on the borders of the lakes, rivers, &c., in Upper Canada, which might be marked and secured for naval purposes, and which might be floated down to Montreal and Quebec with great ease, and which no doubt would be of great benefit

Pines for masts.

nefit in furnishing a large supply of good masts for the navy of this empire.

I am, Gentlemen, with respect,

Your obedient servant,

WILLIAM BOND.

V.

*Remarks on sundry important Uses of the Potato**.

On the use of the potato

THE potato has, though deservedly, occupied so much of the attention of different writers, and of this Society, that it may seem almost *necessary* to bring forward some new and important discoveries concerning it, if we attempt to say more on its qualities. It is not however, a singular opinion, that so important is this vegetable, and so applicable to economical uses, as human food; that it will remain for posterity fully to appreciate its positive and comparative value. But as no new and promising experiment, however imperfectly conducted, should be suffered to escape general notice, it will be acceptable to our readers to receive a general statement of certain trials made by a very respectable *British merchant*, who is also a member of the Society, with a view to ascertain the value of the potato for *sea provision* and other *stores*. His diffidence about having done justice to the subject, which he doubts of finding leisure to prosecute, prevents his allowing his name to appear as to a finished Essay of his own, for this volume; but certain statements laudably reported by him to the Society, are deemed too important to be lost, as they may lead to farther discoveries and facts. The statements then are in substance as follow:

for sea stores.

Cheap methods of preserving potatoes have not been sought after.

"The ease with which this root is prepared by boiling and for immediate consumption, either in its separate form, or mixed in bread; the little trouble there is in preserving it

* Bath Society's Papers, vol. X, p. 293.

it through the winter months; and the short period between the time of planting, and the return of the crop; have most probably been the causes, why *less pains* have been taken to find out cheap methods of preserving potatoes, as a store for future sustenance, than would otherwise have been the case.

Experiments in drying them.

"The large quantity of potatoes produced in the last season, and the reputed scarcity of bread corn, induced me a few weeks since to make some small experiments on the means of drying potatoes, either in substance or in flour; either for future consumption at home, or for the supply of our seamen on long voyages.

This may easily be done.

"The *ease* with which I found this might be done, and the probable benefit which I think may be derived to the public from a farther *pursuit* of the subject, induces me to submit to the inspection of the Society a small quantity of the flour of potato sent herewith.

Potato flour.

"The potatoes were boiled with their skin on, dried on a kiln, and the whole ground in a steel corn mill: none of the skin has been separated by dressing.

It will keep longer than wheat flour.

"By experiments that have been before made on fine dried flour of potatoes, it is known, that it will keep longer than the flour of wheat, without spoiling; that it is used as a substitute for sago, and makes good biscuits without admixture. And I have every reason to believe it will mix and make good bread, in a much larger proportion with wheat flour, than has hitherto been employed of the boiled root, in the common mode of using it.

Washing the chief labour.

"The expense of preparing the flour from the root in large quantities, I am not prepared to speak to. The chief labour is washing the potatoes from the mould which adheres to the eyes, particularly in those sorts, the eyes of which are much depressed. Drying them will be considerably expensive; but I think may be reduced much below what at first it will be estimated at. Grinding will not cost more than corn.

Boiling not necessary,

"From what I believe were accurate experiments, I find that one hundred pounds of washed potatoes will produce full twenty-five pounds of flour (such as the sample). The difference in weight will be very little, whether the potatoes

are

are boiled, or only ground in an apple mill, and the juice suffered slowly to drain from them before they are dried. It might seem therefore at first view, that the boiling might be omitted; my trials however have shown me, that the colour of the flour is much fairer when boiled, and the taste more pleasant; and that the expense of boiling in steam is very little. With the greatest care even some of the starch (the most nutritive part of the root) will seperate with the juice; above three pounds of fine starch (weighed after it was dried) passed off with the water from 100lbs. of potatoes.

but advantageous.

"Other persons will, I trust, ascertain such facts with more accuracy; I myself hope soon to ascertain more satisfactory particulars. In the mean time permit me to make an estimate of the probable produce of an acre of potatoes in quantity, when reduced to the state of flour.

Quantity of flour from an acre of potatoes.

"The average produce of an acre managed with care, estimated at about eighty sacks of 240lbs. each.

"According to my experiments (as before) 100lbs. of washed potatoes will produce 25lbs. of dry flour; or each sack 60lbs.; or one acre, two tons and upwards.

"I am not qualified at present to carry these calculations farther—if quantity alone be the question, I need not.

"*Note.* The potatoes used in the foregoing trials were the red apple potato.

Peeling.

"The steel mill has not ground this flour so fine as I believe a stone mill would have done. Some of these had their skins stripped off after boiling. Should an expeditious method be found of stripping off the skins, it will perhaps be less troublesome than washing so carefully as must otherwise be practised."

After giving a numerical account of the samples of flour of potato prepared for exhibition; this gentleman gives also samples of bread and biscuit made from different sorts of potato flour, mixed with different proportions of wheat flour of different degrees of finenes; but these would be unintelligible in this place, in the absence of such samples.

Maufacture of the flour,

"The potato flour used in the bread and biscuit is made of the *whole* of the potato, washed, steamed, bruised slightly

slightly after steaming, dried on a malt kiln, and ground in a common corn mill, no alteration whatever having been made in the set of the stones, from what they were as used for grinding wheat; it may reasonably be supposed however, that a miller, accustomed to grind this article, would make better work and finer flour.

"Nothing was taken from the flour except some large pieces that were not ground, and a little large bran in the proportion of the sample sent herewith.

"The potatoes of which this flour was made were certainly over dried; and having lain in a heap after steaming upwards of two days before they were put upon the kiln, some degree of fermentation had begun to take place, but which was thought so little as to have been perfectly corrected by the drying. In the bread, however, it is certainly distinguishable. The baker considers, that it is from this cause that the bread is not so light as it otherwise would have been. It rose well in the oven, but fell when the door was opened. He thinks that when mixed with the flour of dry wheat, the potato-meal will have exactly the same effect as the mixture of a certain portion of cone wheat flour, and that it will answer as well in about the same proportion. He has no doubt, but that even with this flour he shall succeed better in the second attempt. With potato meal *well made, he* believes that bread of the best quality may be produced.

The potatoes should be dried without delay after boiling, & not over dried.

Similar to cone wheat flour.

"The chief precautions necessary in making potato flour seem to be, to prevent any fermentation taking place in the boiled potatoes, previously to their being dried, and to avoid giving them too great a heat in drying. With this view it seems advisable to construct the apparatus for preparing it, so as that the steaming tubs and kiln should be heated by the same fire, without loss of time or labour; the potatoes may then be immediately removed from the steam to the kiln, and means should be used to regulate the heat of the kiln, so that it should not much exceed 90°.

Precautions.

"For the common purposes of bread, it seems evident, from the samples, that taking off the rind or skin is by no means necessary; to wash the potatoes carefully before boiling seems, therefore, the only precaution required.

Peeling not necessary.

"From

More potatoes may be used in making bread this way than raw or boiled.

"From experiments as before stated, the produce of dry meal is to the raw potato, as 26 or 27 to 100, but let it be estimated at 25 or 1 qr. of the whole. The greatest quantity of *raw* potatoes said to be used as a mixture with wheat flour in bread is one third; not much above the same quantity of boiled potato has usually been employed. The proportion of flour in boiled potato exceeds that in raw potato by about 1 qr. As a rough ground for calculation, we may take 33 per cent as the proportion of flour in any given quantity of boiled potato.

"The proportion therefore which the potato meal makes of the whole mixture in this bread, above that in which one third raw potato has been used, is four times: that is, the actual quantity of potato flour in this bread is as great, as if 24lbs. of raw potato had been mixed with 12lbs. of wheaten flour; and compared with *boiled* potatoes, it is as great as if 18lbs. of potato had been mixed with 12lbs. of wheat flour."

Practical application.

From the foregoing statements, it is not presumed that much farther information is imparted, than may have been gathered from some former accounts of bread making from a mixture of such flours, except as to the mode of preparing the potato flour. Neither is it at present supposed, that for common use, when corn is not dear, the potato will supersede the use of neat wheaten flour for family bread. But in very dear times, when it may be used in some places to great advantage, the most economical mode of doing it is important; and the process of steaming, kiln drying, grinding, and dressing, seems excellent. If *equal* quantities of wheat and potato flour are found to make very good bread, and the potato to have the effect of *cone flour* in the mixture; this may be set down as a sufficient regulation, and a valuable fact.

Potato flour almost imperishable.

But what is of great consequence to be known and fully noticed is, that the flour of the potatoes so prepared, if barrelled up, and kept in any common dry place, will retain its virtues longer either on land or at sea, than the other sort of flour made from grain: in short, from frequent appearances and well attested facts, the flour of this vegetable, prepared

prepared as aforesaid, seems to possess the singular quality of being almost *imperishable*. In addition to this quality, the power of preserving potatoes in barrels, after being kiln dried, either when whole or cut into parts, for the use of the table in long voyages, is very important; and it is found, that, after being so preserved, they are capable of being again boiled soft, and served up as a vegetable at table, retaining much of their original flavour, consistence, and other qualities.

EDITOR.

☞ For two valuable papers on the fecula of potatoes, and its uses, by Mr. W. Skrimshire, jun., see Journal, vol. XXI, p. 71 and 182.

VI.

On the Dissimilarity between the Creatures of the present and former World, and on the Fossil Alcyonia. From Parkinson's Organic Remains.

SOME of the extraordinary circumstances which have arrested our attention, whilst examining into the nature of *fossil corals*, now demand a few general remarks. You cannot but have observed how completely I was foiled, in my attempt to preserve a parallel between the fossil corals which I have particularised, and the several corals which are enumerated in the *Systema Naturæ* of Linnæus. Indeed, so little could this parallel be preserved, so little agreement could be traced between the recent and the fossil corals, that I find myself under the necessity of acknowledging, that I am not certain of the existence of the recent analogue of any really mineralized coral.

Great dissimilarity between recent and fossil corals.

This dissimilarity between the creatures of this and the creatures of the former world, is a circumstance which appears to be so inexplicable, that I can only admit it, without attempting to account for it. It however furnishes us, I think, with a strong argument against that theory, which supposes the changes which this planet has undergone are all attributable to the constant, regular, and gradual processes

This inexplicable.

The present state of our world not the effect of regular workings of nature:

cesses of nature, which have been acting from an indefinite period of time, aided by the occasional heavings of strata, effected by subterraneous heat. By this system—by the gradual interchange of situation between land and water, we might account for the mountains of fossil coral which are found at considerable distances from the sea, were it not that so little agreement is observable between the fossil and the recent coral. Had the coral of the mountain and the coral of the sea been constantly the same, it would, indeed, have furnished a powerful evidence of the gradual change of relative place in the strata, which were once covered by the ocean, but which are now thousands of feet above its surface: the gradual receding of the sea would have sufficed for the explanation.

But of some great catastrophe.

But how, according to this theory, shall we explain the disagreement between the coral of the mountain and the coral of the sea? I see no explanation which can be thus obtained: every thing being supposed to have proceeded in its regular course, the animals of the first creation must then have exactly resembled those of the present hour. Some vast change, of powerful and even universal influence, must be sought for, to explain this wonderful circumstance: and such, doubtless, can only be found in the deſtruction of a former world. Thus, indeed, we shall be enabled to account for the existence of various animals, in a mineral state, whose analogues are unknown; but it must be admitted, that even this circumstance is not sufficient to account for the existence of animals at the present period, of which no traces can be found in the ruins of that former world.

Fossils of animal origin resembling vegetables.

We now arrive at the examination of that class of bodies, of which it was remarked, in the former volume, that although they were decidedly animal substances of marine origin, yet, from the resemblance which they bore to terrestrial fruits, their animal origin had been doubted, and they had been considered as petrified oranges, figs, funguses, nutmegs, &c.

There is no substance which has attracted our attention, during the prosecution of these inquiries, which can yield so many subjects for investigation as these bodies. For whether

whether we consider the peculiar forms with which they are endowed, the original modes of their exiftence, or the extraordinary changes which they have undergone, a variety of subjects of inquiry, of the most curious nature, will necessarily arise.

That many terrestrial fruits and seed-vessels, containing the ligneous matter, have been found in a petrified state, has been already shewn: of these, of course, it is not intended here to speak. But substances have been repeatedly met with, the general appearances of which have so much accorded with those of some terrestrial fruits, as to have led several learned and ingenious men to place them among these substances. Thus Volkmann was deceived, and figured and described one of these bodies as *nux moschata fructu rotundo*, Casp. Bauhin *. Scheuchzer, on the authority of Volkmann, adopted the same figure and description. Nor will this errour be considered as without excuse, when the great resemblance of many of these substances to terrestrial fruits is shewn. Indeed, I much suspect that, after all the circumstances have been examined, some persons will be found who will not be readily disposed to consider fubftances, bearing such appearances, as subjects of the animal kingdom. The propriety however of doing this will perhaps appear, when other bodies will be shewn passing, through almoft insensible gradations, from these bodies, which so closely approximate, in their general appearances, to the subjects of the vegetable kingdom, up to others, whose characters are sufficiently marked, to leave no doubt whatever in the mind as to their animal origin. Many have been deceived by the closeness of the resemblance.

No one I believe has been more industrious, or more successful in their inquiries, respecting these bodies than M. Guettard, as appears by his very ingenious Essay, *Sur quelques Corps Fossiles peu connus*, in the Memoirs of the Academy of Sciences at Paris for the year 1757. M. Guettard observes, that at Verest, near Tours and Saumur, and at Montrichard, in Touraine, there are found, at some depth in the earth, numerous bodies, which from their very close resemblance, in figure, to figs, pears, oranges, Guettard very successful in his inquiries into them.

* Silesiæ Subterraneæ. Tab. XXII. Fig. 6.

and other fruits, are there considered as fruits, which, having fallen from their trees, have been buried in the earth, where they have undergone the process of petrifaction. These bodies, it appears, not only differ very much from each other, in their forms, but also in their ſtructure: and in Mons. Guettard's judgment are divisible into two kinds; those which possess somewhat of a globular form, and those which are conical or funnel-formed.

Two kinds of them.

The former, he observes, may be divided into the body or globular part, and the pedicle or elongated part. In the centre of the superior part of the body is a circular opening, which, in some of the specimens, is closed by extraneous matter, derived from the matrix in which they lie. This opening, which is larger in its upper part than it is downwards, is continued almost to the pedicle, and in some specimens appears even to penetrate it. This is however very difficultly ascertained, since the opening is in general loaded with the extraneous matter. From the circumference of this opening lines may be traced, which not only pass over the whole of the spherical part, and inosculating, are continued to the elongated part, where they form striæ more or less plain; but they are also found to penetrate into the substance, both of the body and of the pedicle. These bodies have, in general, but one of these openings, but some have more; and Mons. Guettard found one with three distinct openings. In this specimen, the lines or striæ just mentioned were seen to collect around the circumference of each of the openings, and after inosculating, to pass into the pedicle, in nearly the same manner as in the others.

The pedicle varies greatly.

A great disproportion, it appears, is frequently observable between the size of the globular part of these bodies, and their pedicle; sometimes the pedicle appearing very large, and sometimes very small in proportion to the body: this difference is however frequently the consequence of the pedicle having been broken off; a circumſtance which indeed so often occurs, that a perfect specimen is very rarely to be met with: numerous fragments of the pedicles being dispersed about in the places where these bodies are found.

The

The pedicles are in general of a conical form, and not unfrequently flattened.

Texture of both parts similar.

By grinding the globular part as well as the pedicle on a stone, he discovered that their texture appeared to be similar, and that by the frequent ramifications of the fibres, of which their substance was composed, a net work was formed, not much unlike the parenchyma of vegetables. We therefore perceive that a loose resemblance, sufficient to excuse the vulgar opinion of their origin, is observable between these bodies and the terrestrial fruits. These bodies, like fruits, appear to have been formed chiefly of a parenchymatous substance; their pedicle seems to answer to the stalk; whilst the opening on their superior part agrees with what is termed the eye of fruits. But a little attention shews that, unlike to the parenchyma of fruits, which is formed of vessels terminating in minute points, the substance of these bodies is formed of a species of net-work, which, as M. Guettard observes, if all the matter contained within the meshes could be removed, would resemble a skain of thread, of which one part, answering to the pedicle, is pinched together, and the other, answering to the body, is spread out without being cut. Again, the eye, in fruits, is not pervious, as is that part which answers to it in *these fossils*; nor does the pedicle at all agree with the stalk of fruits, either in proportionate size, or in figure.

Its difference from that of fruits.

Fossil supposed to be a sea-fig.

Scheuchzer describing a fossil of this kind refers it to the *alcyonium stupposum Imperati* *; but of the identity of these substances Mons. Guettard, with much propriety, doubts; although he allows that the external form, and particularly the opening in the upper part, might readily lead to this supposition. This doubt arose in the mind of M. Guettard, from comparing the structure of one of the alcyonium stupposum of Imperatus with the description of its structure as given by John Bauhin and by Count Marsilli; the result of his comparison being, that both the descriptions were in some respects erroneous. Taught by careful examination, he states it to be composed of fibres, more or less fine, intersecting each other, without order or

* Lithograp. Helvet. P. 15.

regularity,

regularity, and anastomosing together by their ramifications, by which they form irregular meshes of various figures and quite empty. By this contexture a spongy mass is formed, which is covered by a thin pellicle, constituted in the same manner, excepting that the texture is more close and compact, and extended into a membrane-like substance, which may be detached and easily raised from the body, and which, when examined by a lens, appears to be a mass of fine fibres forming very small meshes, similar to the large ones of which the body is composed. The foot stalk, which spreads out and is a species of basement by which the fig is attached to the body on which it grows, does not seem to differ from the general substance in its conformation. Hence M. Guettard concludes the sea-fig to be merely a sponge, differing from other sponges only in form, and possessing like them the property of imbibing water and losing it by compression.

The sea-fig a sponge.

Difference between this and the fossil.

On comparing the structure of the sea-figs with that of these fossils, M. Guettard points out differences which are undoubtedly very essential. In the pedicles of the fossils, he observes that circular points may be seen, which will be found to be continued into the spherical part of these bodies; so that by different transverse sections they may be traced, passing on like so many veſſels, from the pedicle into the substance, and even on to the surface of the fossil; whereas, in the sea-fig, the fibres have no such regularity of disposition, nor are they thus continued like tubes from the pedicle into the subſtance of the fig.

Fungites, or supposed petrified mushrooms.

M. Guettard next describes the other kind of fossil, which belongs to the class of fungites, and which, like the ficoid fossils just treated of, are open at their superior and wider part, and in general possess somewhat of a conical form: and from their varying in length, width, and size, frequently bear a resemblance to cups, glasses, funnels, cones, &c., whilst others are longer, cylindrical, and even fusiform. This variety of figure is frequently dependant on the circumstances of the fractures which they have suffered; these fossils, like the former, being rarely found in a perfect state. M. Guettard appears to have been entirely foiled in the attempt to discover any recent zoophyte, which might be considered as bearing any analogy with these fossils.

He

He first was disposed to consider them as being similar to the *spongia elegans* of Clusius, or the *spongia dura* of Sloane, but this opinion he found reason to relinquish, and was then induced to believe that they bore a nearer resemblance, in their general characters, to some species of madrepores than to any of the sponges. In several of these fossils he discovered an outer layer, which appeared to differ from the general substance of the fossil; and his opinion, he thought, derived support from this circumstance, for, on examining the interior lamina of these fossils, he conceived that it much resembled the hard smooth part which forms the corresponding parts in madrepores, &c. Madrepores and corals, he observes, are covered by a substance which has been distinguished as their cortical part, and immediately beneath this, there is a smooth substance of very close and compact texture, in which there are no striæ nor traces of any fibres. With this latter substance, he thinks, the external layer of these fossils exactly agrees: and he is confirmed in the supposition that it originally belonged to them, and was not derived from the matrix in which they lay, by observing that, in one specimen, several little flat shells of oysters were adhering to this surface.

Nothing, he thinks, in the fossil kingdom approaches so near to these fossils, as the single-starred corals of the Baltic, described by Fougt. The only difference, M. Guettard remarks, is that the corals described by Fougt have ſtriæ which extend from the centre of the coral to the edge, in such a manner as to form a star. This difference is however sufficient to remove all idea of similarity between the two bodies: since, as we have already seen, the star constitutes the genus Madrepora, to which those corals belong, whilst in the fossil bodies now under consideration, there exist none of the characters which mark any of the species of zoophytes, which we have hitherto examined.

Single starred corals of the Baltic.

Many of these fossil bodies, it will be seen, differ so much from any known recent zoophyte, that were it not that vaſt numbers of these muſt be concealed from us, in the numerous recesses of the ocean, they would be concluded to possess not the leaſt resemblance with any animal substance now exiſting: indeed, so considerable is that difference,

Many fossils apparently of unknown genera.

difference, that some substances will be placed before you, which, not only cannot be referred to any particular known species, but which would almost authorize the formation of new genera for their reception.

We shall proceed, however, as nearly as possible, according to the generally accepted systematic classification; and shall derive what aid can be obtained, from the examinations which have been made of living substances apparently of a similar nature. It is intended, therefore, to endeavour to comprise, under the genus *alcyonium* or *spongia*, the substances so accurately inquired into by M. Guettard, as well as several others which have not been spoken of by him, but are evidently of the same kind.

Difficult to distinguish alcyonia from sponges in the recent state.

With respect to the classification of these bodies, a difficulty almost insuperable presents itself; since the characteristic marks by which the substances belonging to these two genera are distinguished, in a recent state, are frequently not to be discovered after they have sustained the change of petrifaction. Previously, however, to proceeding further in an inquiry on this subject, it will be proper to consider the nature of both alcyonium and of sponge, and to ascertain what are the distinctive characters of each.

Characters of the alcyonia.

The alcyonium is an animal which assumes a vegetable form, and which is either of a fleshy, gelatinous, spongy, or leathery substance, having an outward skin full of cells, with openings possessed by oviparous tentaculated hydra; the whole substance being fixed to some other body by a seeming trunk or root.

Count Marsilli, who carefully examined not only the physical, but the chemical properties of these bodies, observes that they are all surrounded by a porous leather-like bark; and that the interior substance is, in some, a jelly-like matter, and in others, a mass of light ash coloured acicular spines, which prick the hands on being handled, in the same manner as do the spines of the plant called the Indian fig.

More minutely examined by Donati.

Donati, in his Essay on the Natural History of the Adriatic Sea, has made, in some respects, a more minute examination of the structure of two different species of alcyonia

onia than even that of Count Marsilli, and was able to ascertain by the aid of a magnifying glass, the peculiar forms assumed by the spines of which these animals are in a great measure composed. Of these we shall soon have occasion to speak more particularly.

The forms in which these animals exist are very numerous; this depending not merely on the number of species, but on the different irregular forms which the same species under different circumstances may assume. Thus Marsilli observes the same alcyonium, which sometimes grows flat, and thus covers large pieces of rocks, is at other times found in a rounded form. Exist in various forms.

From the different colours as well as forms which some of the species of these substances possess, they have obtained names expressive of their resemblance to certain fruits. Thus the *alcyonium lyncurium*, being of a globose form, of a fibrous internal structure, of a tubercular surface, and of a yellow colour, has been termed the sea-orange: the *a. bursa* being of a sub-globose form, of a pulpy substance, and of a green colour, has been termed the green sea-orange or sea-apple: the *a. cydonium*, which is of a roundish form, and of a yellow colour, has been distinguished as the sea-quince: and the *a. ficus*, from a very close resemblance to the fig in its form, has been called the sea-fig. Named from their resemblance to fruits.

The sponge is a fixed, flexible animal, very torpid, varying in its figure, and composed either of recticulated fibres, or masses of small spiculæ interwoven together, which are clothed with a living gelatinous flesh, full of small mouths or holes on its surface, by which it sucks in and throws out the water. Characters of sponges.

The vitality of sponges had been suspected by the ancients, even in the time of Aristotle; they having perceived a particular motion in their substance, as if from shrinking, when they tore them off the rocks. This opinion of their possessing a degree of animal life was also entertained in the time of Pliny. Count Marsilli * confirmed this opinion by observing, on their being taken out of the Their animal nature suspected by the ancients, and confirmed by the moderns.

* Histoire Physique de la Mer. p. 53.

sea,

sea, a systolic and diastolic motion, in certain little round holes, which lasted until the water they had contained was quite dissipated. Mons. Peysonell supposed sponges to have been formed by certain worms, which inhabited the labyrinthean windings of the sponge; and believed, that whatever life was found in these substances, existed in these worms; and not in the substance of the sponge, which he was convinced, was an inanimate body. This point was, however, determined by Mr. Ellis, who, in a letter to Dr. Solander*, relates the observations which he had made; by which he ascertained, that these worms, which he found in the sponge in great numbers, were a very small kind of *nereis*, or sea scolopendra; and that they were not the fabricators of the sponge, but had pierced their way into its soft substance, and made it only their place of retreat and security. Upon examining, in sea water, a variety of the crumb of bread sponge, the tops of which were full of tubular cavities or papillæ, he could plainly observe these little tubes to receive and pass the water to and fro; so that he inferred, that the sponge is an animal *sui generis*, whose mouths are so many holes or ends of branched tubes, opening on its surface; with these, he supposes, it receives its nourishment, and discharges, like the polypes, its excrements.

Worms in them adventitious.

Texture of sponges different.

Mr. Ellis also discovered, that the texture is very different in different species of sponge: some being composed wholly of interwoven reticulated fibres, whilst others are composed of little masses of straight fibres of different sizes, from the most minute spiculæ to strong elastic shining spines, like small needles of one third of an inch long; beside these, he observes, there is an intermediate sort, between the reticulated and the finer fasciculated kinds, which seem to partake of both sorts.

Distinction between alcyonia and sponges.

In the substances considered as alcyonia by Donati, as well as in some of those which have been described by Count Marsilli, similar large bundles of elastic fibres like needles were discovered. These had been reckoned alcyonia by most authors, but in Mr. Ellis's opinion they should not be

* Phil. Trans. vol. LV. p. 280.

be ſo reckoned, since neither Donati nor Marsilli mentions any polype suckers extending out of their pores; he considering the exiſtence of theſe as the distinguishing character of the genus alcyonium, as much as the pores without the polypes in these elastic fibrous bodies is the character of the sponges *.

It is evident that these needle-like spiculæ cannot be considered as belonging to the genus spongia only; since among the alcyonia some are admitted to be formed of a spongy substance, into the composition of which these spicules may of course be expected to enter: on the presence or absence therefore of polypes in the cells of the substance must alone depend the necessary distinction.

Most difficult in the fossil state.

But when the difficulty of distinguishing between the alcyonia and the sponges, even in a recent state, is considered, the oryctologist will easily find an excuse for his inability, to make a similar diſtinction between these substances, after they have undergone the lapidifying process; when their pores have become filled; and their colour and their subſtance, and, in fact, their whole nature has been changed. Indeed, the assumed generic difference between the alcyonia and sponges is such as muſt be entirely loſt in moſt of these substances which have undergone the change of petrifaction. Whether the pores, which are discoverable in a fossil, were the dwelling of the polypous hydræ or not, can no longer be ascertained; since their radiation, which is supposed to characterize the openings in which these minute animals exist, and which is frequently so faint in the recent alcyonium as hardly to be detected, is very likely, in the fossil substance, to be still more difficult to be made out. Indeed, from this indistinctness of the radiation, much difficulty appears to have arisen in making the necessary distinction between even the recent sponges and alcyonia; the graduation from the perfectly radiated opening of the alcyonium, to the plain opening of the sponge, being so gradual and imperceptible, as to render it a difficult task, even where the substances are in a recent state, to draw the line where alcyonium ceases and sponge begins.

* The Natural History of Zoophytes, &c. p. 183.

But

Farther difficulty from their possessing other characters, and differing from all known species.

But here is not the whole of the difficulty: several of the fossils, which will be presently described, possess some of the characters of acidia and actinia, with those of the sponge or alcyonium; thereby rendering their distinct and correct classification almoſt hopeless. Hence, although I shall in general speak of these bodies as alcyonia; I am aware, that when their histories have been elucidated by the inspection of more illuſtrative specimens, several of them may claim other designations.

The consideration of another circumſtance leads to the neceſsity of giving up every idea of distinguishing the alcyonia from the sponges, whilst in a mineralized state. Among the fossil zoophytes which claim a situation under one or other of these genera, by far the greater number are such as are so totally different from any known species of either alcyonium or sponge, as to render it almost impossible to determine under which genus they ought to be placed. Under these circumstances, you must perceive that the attempt to separate these fossils, by specific distinctions, at present, would be hopeless: it can only be effected when, by additional observations, their nature and forms are more perfectly known.

When it is recollected what very considerable variations in form, are found to take place in the recent individuals, of the several species into which these subſtances are divided; and when it is considered, that whilst passing into a mineralized state, their figure and appearance may be also much changed, it may be suspected that hardly any opportunity of fair comparison could be found, between the recent and fossil alcyonia.

Their change of form, when converted to stone, wonderfully little.

This however is very far from being the case; and indeed when we reflect on the transmutation which has taken place; that a soft, gelatinous, or spongy substance, has become a hard and ponderous stone, we cannot but be affected with a high degree of astonishment; especially on perceiving, that this great and extraordinary change of substance has been accompanied by so little change of form. In consequence of this I trust I shall be able to place before you many bodies, even in a silicified state, which will immediately appear to have been animals of this description,

belonging

belonging to a former world. So great indeed will be the variety of these bodies, and so perfectly well preserved will they appear, as to render it necessary for me to say a few words, respecting the state of preservation in which they are found.

This is rendered necessary; since the comparatively frequent appearance of these bodies, in a fossil state, appears to contradict a position laid down in the former volume, whilst speaking of fruits, that substances possessing a pulpy consistence were not likely to be found in a fossil state; since their decomposition would most probably take place with too much rapidity, to allow of that change being effected, on which their mineralization would depend. But a peculiarity of structure exists in these animals, which exempts them from the influence of this law. It appears, as we have seen from the observations of Marsilli and Donati, that these animals have blended, with their gelatinous and carneous substance, innumerable minute spiculæ, which may be considered as the bones of the animal. These manifest themselves by the prickling sensation they occasion, on being handled, which has obtained for some of these animals the name of the sea nettle. That these spiculæ, formed of a hard and durable matter, may, in some, and especially that the spongy fibres and coriaceous covering may, in others, keep up the form of the animal, for a sufficient time to admit of the petrifactive process being accomplished, seems to be not improbable; and indeed appears to afford a satisfactory mode of explaining this curious fact. Attempt to account for this.

That the bodies now about to be more particularly described are the remains of animals of a former world, seems to require no stronger proof, than the circumstance of these inhabitants of the sea being found in their changed state, in mountains much elevated above the level of the sea, and at a considerable distance from the situations which it now possesses. Whilst treating of the fossil corals, many were pointed out, whose recent analogues were positively not as yet known, and which were therefore conjectured to be the remains of certain species which might be now extinct. Any opinion of this kind with respect to these animals appears to They must have belonged to a former world.

to be hardly admissible; since from the innumerable recesses in which they lurk, and still more from the comparatively small degree of eagerness with which they have been sought, we are totally unable to form any conjecture, as to the number of those which may have hitherto entirely escaped observation. Analogy indeed may lead us to conclude, that by far the greater part of these fossil bodies are actually the remains of extinct species; but where evidence of a stronger kind cannot be also obtained, the fact must be considered as undetermined.

Fossil alcyonia described. Having made these few prefatory remarks, I shall now proceed to a more particular examination of such fossils of this description, in my possession, as are most illustrative of the history of these extraordinary animals.

Ramified. Those which are of a ramified form seem to be most rarely found in a mineralized state. The specimen however which is figured, Plate VII, fig. 12*, and which was found in Berkshire, is undoubtedly the fossil remains of one of these species; although it is impossible to say to what particular ramified species it belongs, or whether indeed it is at all referable to any known species.

Silex & chalk. An examination of the substance of this fossil, now a mixture of silex and carbonate of lime, affords us internal evidence of its origin; since its texture is such, as I have found almost constantly to characterise the fossil remains of any individual of this genus, which had been composed of a sponge-like substance. This substance has evidently, like sponge, been of a reticular texture; but the disposition of the meshes, if so they may be called, is in the spongy alcyonium much more uniform and determinate than in ordinary sponge, and though not to be described in words, the texture is so peculiar and characteristic, as directly to be known by those, who have been in the habit of examining these and similar substances, by the aid of magnifying glasses.

Digitated. The fossil represented Plate VII, fig. 6, and which is also from Berkshire, appears to bear a tolerably close resem-

* The references here and elsewhere are to the figures of the original work.

blance

blance to *alcyonium digitatum* of Linnæus; or the *dead man's hand*, or *dead man's toes* of Ellis. Its texture evidently appears to be of that kind, being finely reticulated, which would correspond with the carneous spongy substance, of which the recent zoophyte is formed. Its surface also, thickly beset with minute openings, bearing somewhat of a stellated appearance to the naked eye, serves to confirm the resemblance. This fossil is now a carbonate of lime moderately hard, but friable. Chalk.

In the elegant work of Mr. Knorr, Mr. Walsh describes several fossil elongated alcyonia, by the silly term which the ancients had adopted, of *priapolithi*. One of these from Touraine is figured, Plate VII, fig. 1. It had at its superior termination that opening, observable in many of these animals, which served for the reception of the sea-water, from which, it is probable, they derived their support. Priapolithi.

On rubbing down this substance on a sandstone, at this termination, for the purpose of examining its structure, its hardness and the partial polish it obtained, proved, that it had suffered an impregnation with silica: and an examination of this surface with a lens plainly showed, that the flinty part was regularly distributed in continuous meandering lines, bearing the peculiar and characteristic form of the spongy part of alcyonia, whilst the intervening spaces appeared to be filled by a softer substance, a carbonate of lime. The substance was therefore partly immersed in dilute muriatic acid, by which the calcareous part was speedily removed, with effervescence, and the siliceous part left, possessing the fine retiform texture of the spongy alcyonium, surrounding the central opening already mentioned, as may be seen in the upper part of the figure. A retiform texture of silex, and the interstices filled with chalk.

The fossil represented Plate VII, fig. 9, approaches the nearest, in its general form and appearance, to the *alcyonium cydonium* Linnæi, the *alcyonium primum* of Discorides, or rather to the representation of this animal as given by Donati. It must however be, I believe, considered, as differing from any known animal of this genus.

This fossil is of a roundish form, rendered unequal by shallow depressions about the width of a finger, which pass from

from the superior to the inferior part of the fossil, and are separated from each other by tuberculated ridges. At the upper part has been a circular opening more than half an inch in diameter; and, at the lower part, is a rugged spot as though the pedicle had been here separated: a circumstance indeed which renders its affinity to the alcyonium described by Donati rather more doubtful. The substance of this fossil appears to be a limestone, which, probably from some tinge of iron, has obtained a reddish brown colour. It is not of a very close texture, apparently from the superadded calcareous matter not having accurately filled all the interstices between the fibres. Hence numerous small openings are, even in its present state, observable on its surface, which on close inspection are seen to be such as would result from a loose or spongy texture.

Limestone tinged with iron.

Spines mentioned by Donati.

Whilst treating of the alcyonium, of the species to which this seems to approach, Donati particularly describes and delineates the curiously formed spiculæ, which constitute a part of its substance. The body, as well as the cortical part, he remarks, is formed of two substances: the one of which is fleshy, and the other osseous. The latter, he adds, is formed into spines; which, near the cortical part, are in great number, and closely intermingled; being about the length of two lines, and even longer. They are either of a fusiform figure, or are finely pointed at one end, and then gradually enlarge towards the middle: then, diminishing as they lengthen, they divide into three sharp conical points, around which are fixed numerous minute globular bodies, which are chiefly found in the cortical part.

Strictures on Donati by Plancus.

A very strict examination, with a lens, of the surface of numerous fossil alcyonia, did not however discover any appearance of similar spines, and almost induced me to a ready concurrence with Plancus, who relates, that he has dissected various bodies of this kind, and has seen the osseous fibres disposed in a radiated form; but as to the wonderful bark, the structure of which is so floridly described by Donati, he says, I have not seen it, and observes that the same thing has happened to him, with respect to the greater part of the figures in Donati's book, which, he says, are embellishments of the designer, drawn by the rule and compass,

compass, rather than in agreement with the truth and simplicity of nature*.

Being in possession of another specimen of this kind, formed of a much harder and closer stone, and which from its appearance I supposed to be invested with its cortical part, I resolved to sacrifice it to a more rigorous search for the spines described by Donati, concluding that, since all agreed as to their differing in their bony hardness from the other parts of this animal, I should at least discover some traces of them, although I might not be able to make out their form. A specimen examined

This fossil was therefore subjected to the only modes of dissection which I could employ with substances possessing a stony hardness. A polished section of it was obtained on different parts of it, and at different depths; by which the peculiar spongeous structure, already noticed as belonging to these bodies, was perceived; but no appearance of spines could be detected. by cutting,

The specimen was then immersed in dilute muriatic acid, and examined at different periods, to ascertain whether the new surfaces thus obtained displayed any particular appearance. After rather more than a quarter of an inch of its substance was thus removed, I was pleased to find, with a lens of moderate power, several cruciform spines, formed, as it were, by two fusiform bodies, not an eighth of an inch in length, crossing each other at right angles, and terminating at each end in a very sharp point. and digestion in muriatic acid, which exhibited the spines.

When these bodies were first discovered, the specimen was still wet with the water, with which the acid had been removed. In this state they possessed a considerable degree of transparency, which they rapidly lost, as the water evaporated: so that when dry, they were completely opaque, and of a chalky whiteness. From their possessing this hydrophanous quality, and from their having withstood the action of the muriatic acid, there appears to be the greatest reason for supposing, that these bodies, which were originally the spines of the animal, are now formed of an hy- These an hydrophanous chalcedony imbedded in chalk.

* De Conchis minus notis. App. II, page 115.

drophanous chalcedony, and imbedded in a matrix of carbonate of lime, which has pervaded or has supplied the place of the soft spongeous part. This and the preceding fossil alcyonia are from Switzerland.

Alcyonium resembling the sea-fig

Alcyonium ficus Linn. accurately depicted in the *Metallotheca* of Mercatus* as *Alcyonium quintum antiquorum*, and particularly described by Marsilli as *Figue de substance d'eponge, & d'alçion* †, resembles much, in form, the brown silicious fossil, Plate IX, fig. 4. The recent alcyonium, according to the Count, is of the form of a fig, being attached to the rocks by branches proceeding from its smaller end; its upper part being a little flattened, with a hole in the middle. Its colour, he says, resembles that of tobacco, and its parenchymatous substance, he thinks, cannot be compared to any thing better than to nutgalls, when well dried. In all these respects, a very exact agreement seems to exist between the recent and fossil substances. Still, however, the fibres running over its surface, and penetrating its substance, with the grooves which appear to have been formed by other fibres, which are now removed, distinguish it, not only from this, but, I believe, from all known alcyonia. This fossil is from Wiltshire, and appears to be formed entirely of flint.

but different.

Wholly silex.

The fossil, Plate IX, fig. 3, from Mount Randenberg, near Schafhousen, in Switzerland, possesses evident marks of its alcyonic origin. This fossil, like those of the ramose kind, figured in Plate VII, has that reticular texture, which appears to be peculiar to the spongy alcyonia. In this specimen also, as well as in those, the reticular fibres are impregnated with silica, and have their interstices filled with calcareous matter. In this, as in the fossil last described, the remains of the pedicle, the organ, by which its attachment to its appropriate spot was accomplished, are observable; as well as the superior opening, which passes into the substance of the fossil.

Reticular texture of flint filled with chalk.

Another similar.

The fossil represented Plate IX, fig. 5, and which is from the neighbourhood of Saumur, being a very perfect fossil

* Arm. 6. C. 6. p. 102. † Histoire Physique de la Mer, p. 87.

of

of the kind described by Mons. Guettard, agrees, in its general characters, as well as in its texture, with that one which has been juſt described. In this specimen, at its superior surface, there are, as Mons. Guettard observes is sometimes the case, four openings; and the pedicles, as well as its lateral processes, which appear like roots, seems to have been formed with a great degree of luxuriance.

A very perfect fossil of this kind, and similar in its substance and texture to the alcyonia, which have been just described, but of a dark red colour, where it is not invested with its cortical part, which is of a grey colour, pervaded by a slight tinge of red, is represented Plate IX. fig. 8. The pedicle, and the opening at the superior part, are here very perfect. Slight traces of lines, passing from the pedicle to the opening, are discoverable on this specimen, and, doubtlessly point out the arrangement of fibres, by which the animal was enabled to draw in and eject the water which supplied it with food. This fossil, I have reason to believe, is English.

A very perfect one of the same texture.

Fibres for drawing in and ejecting water.

VIII.

An Account of Improvements in the Culture of Vegetables, by John Christian Curwen, *Esq., M. P. of Workington Hall, Cumberland* *.

SIR,

I AM fearful you should suppose, that I am become indolent, and that the favours so liberally bestowed on me by the Society had ceased to operate as a ſtimulus to the farther exertions of my humble endeavours to assist those objects, which by the fostering hand of the Society, have been so essentially promoted. You will excuse me for wishing to assure you that I am not idle, and to inform you that the

Objects of importance in agriculture.

* Trans. of the Society of Arts, vol, XXVI, p. 79. The gold medal of the Society was voted to Mr. Curwen for these communications.

 objects

objects which at present employ me are, I conceive, of great importance to agriculture.

The first is by experiments to ascertain the best and most productive mode of applying manure. The second is to determine, whether the distances between the stitches in drill husbandry may not be greatly enlarged, without any diminution of crop.

Best mode of applying manure.

I am strongly inclined to believe, that, where the ground is laid dry, manure can scarcely be deposited too deep; by so doing the evaporation is retarded, and consequently the manure continues for a greater length of time to furnish nourishment to the crop.

Distance of the stitches in drill husbandry.

The increase of the distances between the stitches permits the power of continuing the operations of turning up the soil to a more extended period, which not only improves the tilth, but furnishes a greater degree of moisture by exhalation, than can be yielded from ground in that state of hardness it soon acquires when undisturbed in summer. This evaporation is prodigious, though not perceptible to the eye: it is, however, fully demonstrated by a very ingenious experiment of the Bishop of Llandaff; and I am anxiously expecting to form such conclusions from trials I am engaged in respecting its effects on vegetation, as may deserve the consideration of the Society.

Feeding cattle and horses with potatoes.

My former objects of feeding cattle with potatoes, supplying milk to the poor *, &c., are pursued with increased success. The use of potatoes as a food for horses and cattle increases daily.

I am, dear sir,

Your faithful and obedient servant,

J. C. CURWEN.

DEAR SIR,

Benefits resulting from the Soc. of Arts.

IT is with great satisfaction, that I have the honour of again submitting the result of my farming operations to the consideration of the Society of Arts. Deeply impressed with a sense of the many favours conferred upon me by them, I have found myself impelled, both by gratitude

* See Journal, Vol. XVI, p. 190.

and

and inclination, to proceed with redoubled exertion, as the best return in my power.

The liberal patronage and encouragement bestowed on agriculture by the Society has powerfully contributed to awaken the country to a just estimation of its importance, as the basis of individual happiness and national prosperity; and at this moment the empire owes its preservation and security to it. Agriculture.

I submit with great deference the result of my recent operations. I am disposed to flatter myself, that they may lead to important consequences and discoveries, highly beneficial to agriculture. The experiments I have made tend to establish the double advantage of well cleaning and working the ground. First, as it frees the land from weeds; and secondly, as it conduces to the growth of the crop. It affords likewise a very strong demonstration in favour of using manure in its freshest state, by which not only the great usual expense of making dunghills will be saved, but the manure made to extend to the improvement of a third more land. Advantages of well clearing and working ground. Manure.

Most of the farm I occupy was in that state of foulness as to require, according to general practice and opinion, a succession of fallows to clean it. Being unwilling to adopt a system, which is attended with such loss, I determined to attempt to clean a part of it by green crops, and for such purpose to allow a much greater distance between the stitches, than had ever been in practice. My first experiment on this plan was made on a crop of cabbages; they were planted in a quincunx form, allowing four feet and a half between each plant, in order to allow room for the plough to work in all directions. I adopted this plan of field husbandry, as affording the greatest facility in cleaning the crop, though I believe it never was before so practised. Two thousand three hundred and fifty plants were set per acre (eight thousand is not unusual in the common method), and each plant had, by computation, an allowance of a stone of manure, or less than fourteen tons per acre; though the common quantity is generally from thirty to forty tons per acre. The manure was deposited as deep as the Foul ground cleaned by green crops. Cabbages. Manure laid deep.

the plough could penetrate, drawn by four horses, and the plant set directly above it.

Ploughed and harrowed constantly between the rows.

The plough and harrow, constructed to work betwixt the rows, were constantly employed during the summer, and the ground was as completely freed from weeds, as it could have been by a naked fallow. The very surprising weight of my crop, which in October was thirty-five tons and a half per acre, and many of the cabbages fifty-five pounds each, were matters of surprise to all who saw them, as well as to me, and I could assign no satisfactory reason for the fact. The quality of the land was very indifferent, being a poor cold clay,—the manure was very deficient of the usual quantity,—the plants when set by no means good,—in short there was nothing to justify the expectation of even a tolerable crop. I did not find any thing in the accounts from cultivators of cabbages to afford me a solution of my difficulties, or any clew to explain it. By mere accident I met with the Bishop of Llandaff's experiment ascertaining the great evaporation from the earth, as related in his admirable Treatise on Chemistry; fingular as it may appear, this very interesting experiment had remained for thirty years without any practical inferences being drawn from it applicable to agriculture. It appeared to me highly probable, that the rapid advance in growth made after the hoeing of drilled grain was attributable to the absorption of the evaporation produced from the earth, and was the cause of the growth of my cabbages. With great impatience and anxiety, as I had the honour to inform you last year, I looked forward to the ensuing season, to afford me an opportunity of continuing my experiments. I had long been a strenuous advocate for deep burying of manure, though my sentiments rested chiefly on opinion; this appeared to open a field for incontestible proofs of its advantage. My cabbages were last year planted on the same plan as the former year. Fortunately I extended the same principle to my potatoes, which I was obliged to set on wet strong ground, from want of a choice of land. My annual quantity of potato ground is from sixty to seventy acres. They were set in beds three feet long and two feet broad, leaving four feet and a half between

Great produce.

Evaporation from the earth, absorbed by the plants.

Potatoes set in beds with wide intervals.

between each bed lengthways, and three feet endways. On each acre there were 1230 beds, and 6150 sets, or five to each bed, viz. one at each corner, and one in the middle. The sets of potatoes, when planted according to the usual most approved practice, in three feet stitches, and nine inches apart, amount to about twenty thousand. In the present, and indeed in all seasons when potatoes are scarce, the saving in planting is a considerable object. A great advantage also arises in being able to keep the potatoes and manure from wet. In the late uncommonly wet season I sustained little or no loss in my mode, which was not the case in many of the driest grounds. This plan unites hand hoeing with horse culture, and will be found serviceable in wet soils. Advantages.

The lateness of planting, together with the premature frosts, prevented my forming a fair judgment as to the quantity per acre, which might be obtained by this method. My view in fixing upon this plan was, to enable me to judge of the effects of evaporation, by being able to continue my operations for a longer period. I have no doubt but that in common seasons, notwithstanding the increased distance, the whole ground would be covered.

My experiments on cabbages this season commenced by planting them early in April. From the rain which fell subsequently, and continued till the beginning of May, succeeded by severe east winds, the earth became so hard and baked, that the plants had made very little progress. Cabbages.

In the first week in June the ploughs were set to work: as they started, Mr. Ponsonby of Hail Hall was present, and saw the crop; it was with difficulty, that the ground was first broken, but by the end of the week it was brought into fine tilth. Notwithstanding the whole week had been dry, with a strong sun and severe east wind, yet such was the progress in growth of the cabbages, that when seen again by that gentleman on the Saturday, he could scarce be persuaded they were the same plants. Striking benefit of ploughing the intervals.

During these operations I had been making constant experiments with glasses, contrived for the purpose, to ascertain the quantity of evaporation from the land, which I found to amount, on the fresh ploughed ground, to nine hundred Evaporation from the ground.

hundred and fifty pounds per hour on the surface of a statute acre: whilst on the ground unbroken, though the glass stood repeatedly for two hours at a time, there was not the least cloud upon it; which proved, that no moisture then arose from the earth.

The evaporation from the ploughed land was found to decrease rapidly after the firft and second day, and ceased after five or six days, depending on the wind and sun. These experiments were carried on for many months. After July the evaporation decreased, which proves that though the heat of the atmosphere be equal, the air is not so dense. The evaporation, after the most abundant rains, was not advanced beyond what the earth afforded on being fresh turned up. The rapid growth of my potatoes corresponded perfectly with the previous experiments; and their growth in dry weather visibly exceeded that of other crops where the earth was not stirred. The component parts of the matter evaporated remain yet to be ascertained; the beneficial effects arising from it to vegetation cannot be doubted or denied, but whether they proceed from one or more causes, is a question of much curiosity and importance.

Evidently beneficial to vegetation.

May not a similar process here take place, as when water is exposed to the action of the air in irrigation? Is it too much to suppose some natural operation to take place in the earth, which may decompose the oxigen contained in air from the hidrogen, during the absence of the sun, which on the sun's reappearance may be again given out in a state highly propitious to vegetation? Oxigen is found to contain carbon; and may not the growing plants imbibe it from the air, and may we not thereby account for its forming a constituent part of all vegetables?

Does not the air assist the action of the water, as in irrigation?

The investigation of these objects presents a wide field for inquiry, and may lead to very important discoveries. From more or less oxigen contained in the earth, may not its proportions account for the fertility of one soil above another? May not the advantages supposed to be derived from loosening the soil, proceed from its being thus rendered in a fit state to imbibe the air? Fallows soon become so hard upon the surface, as to be capable neither of absorption

Objects of inquiry.

absorption nor evaporation. One very important result is placed before the eyes, and within the reach of every practical agriculturist to ascertain, namely, that the evaporation from dung is five times as much as from earth, and is equal on the surface of an acre to 5000 pounds per hour. By making use of dung in its freshest state, the farmer may extend his cropping to one third more land with the same quantity of manure. It is with regret that I have viewed in many parts of the kingdom the quantity of manure which is exposed on the surface, and tends to no good. I am strongly of opinion, that in all light soils, if the manure was buried in trenches as I propose, and the turnips sowed above it, more abundant crops would be procured. By cleaning with the plough, great advantage would be derived to the crop, from the evaporation yielded by the earth. Hot manure might also be used. By fermentation dung is reduced to one half its bulk, and its quality reduced in a much greater proportion. The manure now commonly taken for one acre of broad cast would, if deposited whilst hot in drills, answer for four acres, and the crop produced be much more.

Great evaporation from dung.

Dung should be used fresh and buried deep.

If the Society of Arts extend their sanction and patronage to my exertions, I shall feel bound to proceed, and to endeavour to bring the experiments to a regular system. The glasses I used for determining the quantity of evaporation were of a bell form, and placed with the open part upon the earth; a quantity of tow was first weighed, ready to wipe off the moisture collected from evaporation within the glass, which tow was then again weighed as exactly as I could after the glass had stood for a given time, and been wiped dry with the tow; and from knowing the contents of the glass I made my calculations. Mr. Robert Wood, watch maker, of Workington, attended to the experiments made with the glasses.

Experiments will be pursued.

I have the honour to be, with great respect,

Dear Sir,

Your obedient humble servant,

J. C. CURWEN.

DEAR

DEAR SIR,

Opinion that the evaporation greatly influences the produce.

IT is with great pleasure and satisfaction, that I learnt yesterday from Mr. Arthur Young, the Secretary of the Board of Agriculture, that he has adopted my idea of the great importance of evaporation, and that he has actually ordered Mr. Blunt, optician of Cornhill, to construct him an instrument for ascertaining the evaporation, which instrument I shall request Mr. Blunt to show to the Society. Mr. Young intends in the course of the summer to make a variety of experiments on the quantity of evaporation produced from different soils, agreeing with me, that the greater or less degree of it influences most materially the luxuriance or growth of the crop.

In all the valuable tracts which Mr. Young has given to the world, he has never adverted to this, and the first knowledge of it as a principle for promoting the growth of crops was obtained from my account of the Schoose Farm, in the report of the Workington Agricultural Society, of which he is a member.

A great saving of manure.

Being unable to account for the surprising weight of my first crop of cabbages, with only one third of the manure usually given, I was led to make the experiments I have laid before the Society; and I believe I am not only the first person in Lancashire, but even in Great Britain, who ever thought of ploughing the ground upon the principle I have executed, for promoting the growth of the crops.

Hence more waste land might be inclosed.

I flatter myself, that my experiments on the economical application of manure will lead in a high degree to facilitate a more extended cultivation, and obviate the objections, which have been started by some persons against the enclosure of waste lands, from their supposition, that manure could not be furnished for more than the land at present cultivated.

I remain, dear Sir,

Your obedient servant,

J. C. CURWEN.

CERTIFICATES.

Certificates.

A certificate from Miles Ponsonby, Esq., of Hail Hall, testified, that he had seen Mr. Curwen's statement of the rapid progress made by his cabbages in the month of June 1807; that he perfectly recollects viewing them on the Monday, and again on Saturday in the same week; that the improvement in the appearance of the plants was so great, that he imagined the land had been replanted, till Mr. Curwen explained the cause, which had produced so great a change.

Certificates of the benefits accruing from Mr. Curwen's plan.

That he considers Mr. Curwen's plan of managing his potatoes and cabbages as very good garden husbandry, and the best calculated for keeping the land clean, improving the plant, and at the same time enriching the ground, of any that he had observed; and though the mode is entirely new there, he has no doubt but it will be found beneficial, and that it will in a few years be much attended to.

A Certificate from Mr. D. Campbell, Secretary to the Kendal Agricultural Society, stated, that he had attended to the cultivation of potatoes in most parts of Lancashire, and could speak with the greatest precision respecting it in that *part of the* country which is north of Lancaster.

That whether they were planted in the lazybed way, by the dibble, or with the plough, they were always set in *rows* from *one end of a field*, or *piece of ground*, to *the other end or side*, with narrower or wider intervals, as the cultivator might deem best suited to the kind of potato he was raising. That he never before saw or heard of their being cultivated in beds, in the manner practised and described by Mr. Curwen, and that being more particularly desirous to ascertain whether any such method was pursued in the great potato district which lies south-west from Lancaster, including Pilling, the Fylde, Rufford, and the neighbourhood of Preston, he applied to George Clayton, Esq., of Lostock Hall, and Robert Hesketh, Esq., of Warrington Hall, gentlemen upon whose accuracy the utmost dependance may be placed, and who informed him, that neither from their own knowledge, nor from inquiries they have made, can they learn that

that the method of cultivating potatoes alluded to has been seen or heard of in a tract of country, where more are raised for the market than in any other of the same extent perhaps in the kingdom.

Advantage of Mr. Curwen's mode of planting cabbages.

Mr. Campbell further stated, that Mr. Curwen's cabbages were planted at a much greater distance than any he had ever before seen, and their size far exceeded, as a general crop, any that had fallen under his observation; that the ground was perfectly clear from weeds, and from having been frequently turned over by the plough in the intervals, the mould appeared to be in fine order for a subsequent crop, and he conceived that in the two essential points of freedom from weeds, and of the land being in a fine tilth, no garden could exceed it.

Further certificates.

Other certificates respecting the novelty of the method of planting potatoes, as practised by Mr. Curwen, were received from the following gentlemen:

William Knott, Summerhill.
Mr. Sunderland, Ulverston.
J. Penny Marshall, Bolton Oak.

Further certificates, stating the method to be new as practised by Mr. Curwen, for planting both potatoes and cabbages, were received from the following gentlemen:

Walter Gardner, Crooks.
William Harrison, Ulverston.
A. Benson, Reading.
Henry Richmond Gale, Bardsee Hall.
Jos. Penny, Budgefield.
Edward Barrow, Allithwaite Lodge.
Charles Gibson, President of the Lancaster Agricultural Society.
Rev. J. Barns, Pennybridge.
Rev. E. Ellerton, Colton.
Jos. Yorker, Ulverston.
Michael Knott, Thurstonville.
Rev. Joseph Brooks, Ulverston.
Thomas Machell, Aynsome.

Also

Also from the following farmers, resident in the neighbourhood of Lancaster:

THOMAS TART.
WILLIAM ARMSTEAD.
WILLIAM STALLER.
ANTHONY EIDSFORTH.
CHRISTOPHER ATKINSON.
ROBERT EDMONDSON.

DEAR SIR,

Mr. Curwen having informed me, that a question would probably arise in the Society of Arts &c. relative to the degree of exhalation of water from the earth, and it appearing to me to be intimately connected with various matters in agriculture, I think you will not be displeased at my mentioning a few circumstances, to prove, that the object much deserves attention. I conceive that it bears upon the point of showing the great depth, to which dung may be ploughed with safety; for when we find, as I have done, that from two to three thousand gallons of moisture are exhaled in a day from an acre of land, and that the quantity varies greatly according to the state of tillage, it should appear, that such a vertical stream of vapour must remove all apprehensions of *burying* dung. I also think it goes to the point of hoeing and horse-hoeing such plants as demand much moisture. I have found, that the dung in a farmyard, laid three feet deep and hard trodden by cattle all the winter, has exhaled in the proportion of above four thousand gallons per acre in ten hours; hence a practical conclusion may be surely drawn. I could much extend these observations, but they are sufficient to convince so enlightened a mind as yours of the propriety of a very extensive pursuit of this inquiry.

Subject of evaporation of much importance.

I have the honour to be,
With much regard, dear Sir,
Your faithful and very humble servant,
ARTHUR YOUNG.

IX.

IX.

*Electrical Experiments on Glass considered as a Leyden Phial, and on coated Panes; by Mr. *** *.*

Experiments militate against the doctrine of plus and minus electricity.

CHANCE having thrown in my way two papers written in Dutch by Mr. Lugt, I was surprised on reading them to find, that this gentleman could admit the theory of plus and minus electricity, while almost all his experiments concur in proving, that there is an actual passage through the pores of the glass, when it has a communication on one side with the prime conductor of an electrical machine in action, and on the other with conducting bodies communicating with the ground: and that to obtain this passage it is not necessary for the glass to be coated on both sides, as it is sufficient for that in contact with the machine to be so, and to touch at a single point some substance that is but an imperfect conductor, as the wood of a table, or the like, which has sufficient force to communicate the attraction of the Earth through its pores. Thus I have always suspected the charge of the cascade is effected, in the 5th experiment of my first letter to Mr. van Mons: but as the phial seems to retain in its pores a portion of the electric fluid, and collect on the surface communicating with the ground a large quantity of fluid sensible both to the touch and sight, when we charge highly a phial not coated on that side; I have thought the force of attraction of glass for this fluid was so powerful, that Abbe Nollet had reason to suspect it attracted electricity from the Earth, which however did not happen in the experiments of Mr. Lugt, as for instance the following, which is the second of his first essay.

Glass has a powerful affinity for the electric fluid.

Insulated phial charged by an insulated machine.

He procured an apparatus completely insulated by means of four glass feet. Thus he could at pleasure leave the whole insulated; or form a communication between the ground and the conductor, or the ground and the rubbers, which were united together by a semicircle of metal placed about a foot from the insulated plate. Rods were contrived to

* Journal de Physique, vol. LXIV, p. 371.

to be fixed occasionally to the conductor or the rubbers. In this experiment he fastened one of these rods to the rubbers, and made it communicate with the outer or inner coating, it did not signify which, of a phial placed on an insulating stand; the other coating of the phial communicating with a similar rod fixed to the conductor. The communication was made by means of a wire in contact with each coating, and terminating at the other end in a knob, which might be brought near or removed from the other rods at will. This phial, thus completely insulated, was charged by an equally insulated machine. Hence the author infers, that the ground does not contribute to the charge of the phial; and that, when the apparatus is not insulated, the wood of the table, and that which supports the stand, are the invisible conductors of the fluid from the surface that parts with it towards the point where the fluid is excited on the plate: that in his insulated experiment the use of the rods supplies the place of the ground, and conducts the fluid: &c.

I cannot admit the theory of taking the fluid from the surface of an impenetrable substance, as Dr. Franklin asserts glass to be; because it is a fundamental law of chemistry and physics, that no movement can take place without a previous impulse, and consequently without immediate action on the substance to be deprived of the fluid. Besides, what substance is there, that the igneous matter cannot penetrate? and no one will deny, that the igneous matter forms a part of the electric fluid. Accordingly I deduce an opposite inference from this experiment.

Glass not impenetrable to the electric fluid.

Mr. Lugt then recites several very ingenious experiments, among others the following on the electrophorus, by which he would go on to prove this singular deduction; but which in reality prove nothing, except that the attraction of the igneous fluid, developed at the disk, is strong enough to supply the place of the attraction of the ground: in fact, that in uninsulated and insulated experiments glass has such an elective attraction for the fluid, as to retain the same quantity in both situations of the phial. It is still to be accounted for, like all chemical and physical phenomena, by the theory of elective attraction.

Affinity of glass for the electric fluid shown by

He

an experiment with the electrophorus.

He takes an electrophorus, places it on an insulating stand, and insulates himself before he rubs it. In this state of complete separation from the ground he excites it by friction, touches the two coatings, and obtains sparks as strong as if both he and the electrophorus had a communication with the ground. Hence he concludes still, that the double contact, necessary as he says, establishes a complete circulation, as in his experiment with the phial.

The experiment made in another way.

There is a more simple mode of making this experiment with a small curved exciter with a glass handle. I take an electrophorus completely insulated; I rub it in a state of insulation like the Dutch philosopher; I quit the insulating stool and take the exciter, the two coatings of which I touch at once with its knobs; and I not only obtain a spark, but taking the exciter, leaving one of its knobs on the external coating, and raising the other four or six inches so as to lift the cap to it in the air, a real discharge takes place. On laying down the cap without a fresh contact, scarcely does it give a very feeble spark. The beautiful experiments of Mr. Libes, in which he obtains electric fluid by the mere contact of different metals, evidently prove to me, that here, where the action is triple, or between two metals and rubbed resin, there is a real generation of igneous matter, if I may so express myself, which is renewed at every double contact. The following experiment is calculated to support my conclusion.

Electricity from the contact of different metals.

Sparks from the mouldings of a chest of drawers when one taken from an electrophorus upon it.

I had seen in the Electrical Phenomena of Mr. Sigaud de la Fond, that some gentleman observed the gilt mouldings of a chest of drawers to emit sparks every time he drew one from the cap of an electrophorus accidentally placed on it *. This fact led me to make the experiment with an insulated electrophorus, by the side of which I placed a copper ball having a rod that communicated with the ground. This ball was about a line from the outer coating; and I stood on an insulating stool when I took a spark. In this state, to prove that it is no circulation that occasions the discharge, but an attraction of the ground, which becomes divellent at the moment when the fluid retained in

* Phénomènes Electriques, p. 678, § 174.

the

the metal of the cap acts no longer in competition with the glass to fix it in the metal of the inferior coating, I raised the cap three or four inches, and held it thus a few seconds without seeing the least spark pass between the inferior coating and the knob of the exciter; but the moment I drew electricity from the cap, a strong spark was emitted toward the ground. This fact gave me the more pleasure, as it still more confirmed the theory of elective attraction, on which all my deductions are founded. I know not whether this experiment be new, but I do not find it in Libes, Haüy, or the French translation of Fischer, which has lately appeared with notes by Biot; and it appears to me to merit attention, as it throws light on the theory of thunderstorms. Here the column of air interposed between the cap and the glass prolongs the retaining power of the glass to six, eight, or even fifteen or sixteen inches in dry weather: there I figure to myself a large plate of air between those clouds that traverse the atmosphere in opposite directions, the electric fluid of which remains insulated till the moment when the elective attraction surpasses the retaining action of the stratum of air, &c. This experiment also shows the reason why the new doubler of electricity, invented a few years ago in England, charges its plates on approaching and separating them repeatedly, and acquires through the stratum of air that separates them so intense a change, that the plates discharge themselves spontaneously *.

Thunder-storms.

Doubles.

The glass electrophorus, mentioned by Mr. Lugt as well as Sigaud de la Fond, but the effects of which, as it appears to me, have not been compared with those of the Leyden phial, has lately engaged my attention. The following are the experiments I have been led to make, and in my mind they render still more probable the complete saturation of the Leyden phial by the retaining affinity of the substance of the glass itself.

Glass electrophorus.

I take a square of German sheet glass [*verre blanc de Bohême*] twenty or two and twenty inches wide, and place it on an insulating stand seven or eight inches in dia-

Experiment.

* See Journal, vol. IX, p. 19. It is for September, 1804, not 1805, as misquoted by the writer in the Journ. de Physique.

meter,

meter, gilt or silvered all over, with its edges well rounded off, and supported by a glass foot at such a height, that the balls of the two curved tubes may rest on a little metallic circle of three or four inches diameter cemented to the centre of the upper side of the glass. Below I place a knobbed exciter against the edge of the gilt top of the insulating stand, leaving about a line distance between them, as in the preceding experiment. In this state I begin to charge. At the first turns of the plate it frequently happens, that we see round the little upper coating some flashes of electric light; but if the glass be thin, they will soon disappear, and though you continue to turn the plate a thousand and a thousand times, the square will be charged to the whole capacity of the coated glass, but will afterward yield a continual passage to the fluid. By this experiment in the dark I have been convinced of the reality of the passage of the fluid through the pores of glass as through a filter of capillary tubes. This experiment was repeated several times in the presence of the friend, who suggested to me the idea of the oxidation of the metallic coatings, comparing them with those, which probably take place in the great in marble quarries. He is inclined to consider this as an *experimentum crucis* with respect to this passage of the fluid. It is thus he is equally convinced, that the electric fluid oxides the most tenacious metals partially in its passage, before it destroys them at the instant of the developement of the gasses, which takes place in my metallic cylinders. He is an excellent pneumatic chemist, and frequently repeats to me, that caloric penetrates all bodies, that all consequently have pores, and that the penetration of the electric matter through those of glass is in no way inconsistent with the true principles; but that the pretended removal of it from one side of the glass, which receives a superabundance of it on the other, is contrary to the axiom of his master, Lavoisier: there is no motion, no sensation, unless the impulse acts through the thickness: and hence, if we grant this expulsive action, we must admit a capacity of penetration in the fluid.

Passage of the fluid through the pores of the glass.

Electricity destroys metals by oxiding them.

In his presence I repeated the experiment with common glass. This yields a passage to the fluid with less ease, but on

Common glass less easily pervaded but

on the contrary saturates itself infinitely more quickly: in a little time it discharges on itself, notwithstanding the little extent of the coatings. We ascribe the anomaly of these two different kinds of glass to different fluxes. The German glass contains more metallic oxide, the common more saline matter. If this inference be just, the English flint glass should be like a sponge to the fluid; and if it were possible to find large squares coloured with metals, these perhaps would furnish us with other facts. sooner saturated with the electric fluid. Flint glass.

It must be observed, that, notwithstanding the German glass admits this passage, a large mass will not pass, unless it be attracted in the manner related in a former letter. This is why we see a reflux toward the machine. The following experiment will in some degree account for this. German glass.

I charge a glass electrophorus, placed on an insulating stand, the lower coating of which is as extensive within an inch as the glass, and stop the machine the moment the sparks announce an approaching spontaneous discharge: if in this state I cut off the communication with the ground, and take the cap from the upper surface, the whole charge will remain adhering to the glass; and on touching it a prickling sensation will be felt, and something like an igneous vapour. On extinguishing the light it is visible, particularly if you approach the edge; but the fluid becomes absolutely luminous, if you blow lightly on the surface: then a wave of fire traverses the glass, to join the fluid accumulated on the other side between the glass and the metallic coating. What is particularly remarkable, two colours may be distinguished in the fluid, the lower being whiter and more vivid. This phenomenon takes place if the communication be suffered to remain: the wave of fire, which flows from the part blown upon toward the lower surface is stronger, but it does not continue so long. This experiment gives rise to the question, whether all the ingredients pass through the substance of the glass, or whether the difference of action is to be ascribed to the state of the glass alone. I believe it is this modification, which the electric matter itself appears to undergo, that constitutes the opposite states, which every natural philosopher endeavours to explain according to the mode in which he views Experiment. The fluid may be blown toward in a luminous wave of two colours. The electric fluid or compound.

them;

them; Franklin by plus and minus; du Fay by two fluids neutralized in bodies, the particles of which repel and attract each other; &c.

Is not the fluid retained by the attraction between the two surfaces of the glass?

Does not this experiment demonstrate, that the attractions, which act here between the surfaces of the glass reciprocally, retain the fluid on the upper side notwithstanding we take off the cap? while, if the opposite surface be not insulated, the cap takes it off at a distance of three or four lines above it, if we touch the cap with a metallic body communicating with the ground without establishing a complete circuit; because then the ground wholly absorbs that which is accumulated on the opposite surface. To verify this fact, I have repeated the transvasation of water, in the three following manners.

Water poured from a charged into an uninsulated bottle,

I charge a bottle filled with water, and pour the water into another bottle standing on a plate of lead, that has a communication with the ground. Whether I be insulated or not, when I do this, the two bottles divide the charge between them. But to retain the charge in that which has lost its water, I must place myself on an insulating stool when I pour into it fresh water, unless it be from a glass vessel; otherwise, as the electric fluid may escape both by my body, and by the metal on which its outer surface rests, and which can conduct the opposite electricity into the ground, the bottle will discharge itself entirely on one side by my body, and on the other toward the ground; in the same manner as a charged bottle touched by the hand, while there is a communication between the ground and its opposite side.

and into an insulated bottle.

On the contrary, if I charge a bottle highly, and pour its water into an insulated bottle, the water will convey away nothing, and the whole charge remains in the bottle; because there is no attraction of any substance to act on the electric fluid, the glass, which I suppose to be saturated during its fusion, having no longer any affinity to attract it. It is like a full sponge, which takes up no more water, unless it can part with some of what it contains to another body. It is not in the coatings then, that the fluid is retained, but in the glass itself, and on its two sides. If, as I have remarked above, I make the transvasation into a bottle

tle communicating with the ground by its external coating, while I stand on an insulating stool, it neither loses nor acquires more of the electric fluid. Must we not hence conclude, that the outside, when once charged, neither attracts any thing more from the ground, nor gives off any thing to it?

The following experiment with the electrophorus throws still more light on all these facts.

I charge an electrophorus of glass and resin; I touch it on both sides; I raise the cap, and place it again on the electrophorus; the moment I touch with my hand either the external coating or the cap, I perceive a spark almost as strong as that which issues from the cap taken off. But if, before I replace the cap, I touch the inferior coating, I take from it its superfluous electricity; and when I touch it afterward the spark is almost nothing: a sign, as it seems to me, that the fingers in touching the two surfaces only establish a communication between the two coatings, which serves as a divellent intermedium, if I may use the expression, to develope the fluid that is disengaged.

Experiment with the electrophorus.

I offer these views to the natural philosopher, not to create a new theory, but as an inquiry whether the igneous phenomena of magnetism, galvanism, electricity, and detonations, be not subordinate to the general law of affinities. The fine experiments with which Libes and Ermann have enriched the fields of science concur in support of the hypothesis, that there is but one igneous matter, which forms light, the magnetic, galvanic, and electric fluids, &c., and is modified in them by different ingredients. In a letter which I wrote to Mr. Delamétherie about six months ago I called these *semigravitating*, because I see them always take a centrifugal force, and accompany this matter when it is disengaged from combustible bodies: one of these fluids takes it, like that of ether, at a certain degree of heat; another only at the strongest heat of a burning glass, unknown before Homberg, and even in his time, which is necessary to volatize gold; and so on. The experiments of Mr. Ermann demonstrate, that the flame of alcohol contains different ingredients from that of sulphur, or that of phosphorus.

Does not the matter of fire, combined with different ingredients by chemical affinity, produce the phenomena of magnetism, electricity, &c.?

phosphorus*. Examine the gasses, which the same acids evolve from different metals, or the different colours of artificial fireworks; do not all these modifications demonstrate, that the calorie of the air, added to the ingredients latent in combustibles, carries off various particles, the number of which will ever remain unknown to us? Of the nature of carbon, nitrogen, hidrogen, oxigen, abundant as they are, we are still ignorant. Are they simples? or are they compounds? How many varieties do these four bases afford merely by the proportions in which they are combined? Why does the new inflammable mixture that alarmed Proust, and prevented him from pursuing his experiments, appear still more terrible than fulminating silver? Before my experiments, if I had spoken of the combined action of water, lead, and the electric fluid on the most tenacious metals, as solders and iron, should I have ventured to say, that the igneous expansion in them might at length become sufficiently powerful to burst a cylinder of the best iron of ten lines in diameter, and two lines aperture, consequently four lines thick; as well as a large cartridge of an alloy of nine parts copper and one tin similar to the former, which so long resisted a force of about forty feet, and was burst by one of a hundred and forty in ten explosions? That of iron exhibited undulations at the ninth explosion, but was not actually cracked till the fortieth. I could wish, that some one would try two cylinders of similar materials, to find the proportion of the resistance, which is not in the ratio of the square of the thickness, as I had imagined. The progress of the resistance is greater on doubling the thickness of the iron; for a cylinder of iron of half the thickness was cracked at the fourth explosion, and at the seventh the cracks were wider than at the fortieth in the thicker cylinder. I cannot but be persuaded, that the igneous action tends to decompose the metals subjected to it.

Action of water, lead, and the electric fluid, will burst any metals.

* Journal de Physique, February 1807: or our Journal, vol. XVII, p. 246.

X.

IX.

On the Identity of the Base of Charcoal with Hidrogen, or its Base. In a Letter from Dr. John New.

To Mr. NICHOLSON.

Stapleton, near Bristol, April 24th, 1809.

SIR,

IN the 18th volume of your valuable Journal, p. 43, is inserted a paper, entitled "Report on a Memoir of Mr. "Berthollet, jun. entitled, Inquiries concerning the reciprocal Action of Sulphur and Charcoal; by Messrs. "Fourcroy, Deyeux, and Vauquelin." Reciprocal action of sulphur and charcoal.

The general conclusions from the experiments are,

"1st. That charcoal contains hidrogen, which the most "intense heat we can produce will not completely expel." Charcoal contains hidrogen.

"2d. That sulphur at a red heat acts upon hidrogen, "and forms compounds in very different proportions, on "which their properties depend." Sulphur forms a compound with hidrogen,

"3rd. That charcoal deprived of hidrogen, or at least "nearly so, forms with sulphur a solid compound, into "which the sulphur enters in a small proportion." with charcoal deprived of hidrogen,

"4th. That at a high temperature sulphur, carbon, and "hidrogen unite into a compound, which assumes the state "of gas." or with both;

"5th. And lastly, That sulphur contains hidrogen." and contains hidrogen.

The perusal of this important paper furnishes me with an opportunity of communicating an opinion, which I have, for some years, entertained: *That charcoal and hidrogen are modifications of one and the same substance,* or *that hidrogen is the base of charcoal.* Hidrogen the base of charcoal, or a modification of its base.

My opinion was formed from the result of various experiments and obervations, made at a time when experimental chemistry was a favourite amusement; but which very different pursuits have obliged me reluctantly to relinquish. This opinion founded on experiment.

Should this opinion be confirmed by accurate experiments, (and it appears to me to have been nearly proved by Berthollet in the Memoir above quoted, at least by analysis)

lysis) what an important and extensive field will be opened to the scientific world!

The carbon of plants from water.

The pabulum of plants, and the origin of that immense quantity of carbonaceous matter annually produced in the vegetable kingdom, will be easily and satisfactorily accounted for, as originating from *water* alone.

Different appearance of hidrogen and charcoal no argument.

Although the two substances hidrogen and charcoal differ so much in appearance, yet, it may be a question whether the *diamond* and *charcoal*, or *steam*, in its greatest degree of rarity, and *ice* or *snow*, do not differ quite as much.

This intended only as a hint for inquiry.

I do not mean by this communication to lay claim to any priority of discovery, but only to furnish a hint to others, which, if improved by those who have leisure and ability to pursue the inquiry, might lead to the discovery.

I took no notes of the experiments to which I have alluded, and certainly cannot, at this distant period, narrate them from memory; and, if I could, it is by no means improbable, that they might be explained in a different manner.

I am, Sir, your obedient servant,

JOHN NEW, M.D.

X.

Extract of a Letter from a Gentleman in Jersey to his Friend in Glamorganshire, on the Use of Vraic as a Manure. Communicated by J. FRANKLEN, Esq*.

Seaweed good manure for light soils; its ashes on strong.

VRAIC, or its ashes, we esteem here good for all manner of soil, whether deep and heavy, shallow or light; for we use it on all our lands. I think ashes agree best in the strong soil, as they lighten it, and open its pores; and the vraic in the light or shallow soil, for it keeps it moist in the summer: yet our people use both together on all lands. The ground receives no benefit from the vraic but for the year

* Bath Society's Papers, Vol. X, p. 258.

year in which it is laid on; but does from the ashes for several years.

Our time of gathering it in summer is always the first or second spring-tide after Midsummer: the Court fixes the day to begin to cut it. There are but six or seven days allowed to do it. It is done with a small hook, partly cut and partly torn from the rocks. It is brought ashore just above high-water mark, and there spread and dried in the same manner as hay. Three or four days of fine weather are enough, (for it must not be too dry.) It is put in large cocks, and carried home at leisure, and housed. If there be no convenient place they make a rick, and a certain quantity is brought within at a time. A small bundle of brambles, or a little faggot, is put in the chimney, and twice or thrice as much vraic as a man can take in his arms placed over it. It makes a good fire, and as it burns must be supplied with fresh vraic. The ashes must be drawn aside in a corner of the chimney every now and then, for it must not be burned too much, otherwise it would lose the best part of its virtue. The ashes are carried away every morning to a place under cover. Before I leave this article, I must observe to you, that it may be gathered with you, as there is no restraint, any time in the summer.

Collecting and curing it.

Burning for ashes.

The winter vraic is begun to be gathered about the middle of February, and continues till about the latter end of March. That with large broad leaves, which usually grows in deep water, is the best to be used green. It is carried as soon as possible on the land for which it is intended, and spread on it, if rainy weather. If very dry weather, it is left on the ground in little heaps till moist weather.

Spring gathering.

This is the method by which we gather our vraic here. Now I will describe how we use it. After our land has lain fallow three or four months, about December or January we give a light ploughing, just to turn the turf. Some spread their ashes before it is turned; others after. I believe it is no great matter which. We allow forty-eight bushels to a vergee, (two vergees and a quarter make an English acre) the green vraic is brought, as before-mentioned, and spread in such a manner as that the leaves almost

Method of using it.

almost touch one another. We generally allow two cart-loads, or sixteen horse-loads, to a vergee.

Crops. In the latter end of March, or beginning of April, this ground is ploughed deep, and sown generally with barley. Some sow a sort of wheat which we call *fremé*, which must be sown the beginning of March; others sow the common red wheat in the beginning of December, allowing the same quantity of ashes; but instead of vraic they put dung. This is the way of our ploughing the first year. The second year the soil is manured and ploughed as the first, but always sown with barley, at the season before-mentioned. The third year there is no manure used, nor the following years. All the ground is either dug with a spade, or turned with two ploughs, one following the other in the same furrow, that the ground may be turned deep. In January and February beans are planted in ridges, and parsnips sown all over the ground; the weeding and digging of which is very expensive; but nothing that I know answers better than parsnips to fatten hogs or black cattle.

This ground that has been dug deep, stirred in the weeding, and again dug to get the parsnips, is finely prepared to sow wheat the fourth year, which is done in December and January. I generally sow clover seed in it in the beginning of April, which I think better than taking oats the fifth year; for it impoverishes the soil, and its produce is not answerable. However, most people sow oats after their wheat and clover seed.

Produce. Now as to the produce. This cannot be exactly ascertained, as it depends on the nature of the soil, goodness of the season, &c. So I will fix it as near as I can at a medium. Of barley, we have sixteen bushels per vergee, each bushel fourteen gallons; of beans, about eight bushels (same measure) per vergee; and five cart-loads of parsnips. The produce of wheat is about fourteen bushels, of ten gallons each, per vergee. We have about the same number of bushels of oats, at fourteen gallons each.

XI.

Account of an extensive Orchard planted at Bradwell in Essex, by Mr. SAMUEL CURTIS, *of Walworth**.

SIR,

I Take the liberty of sending you an account of an undertaking, for which I hope I shall be entitled to some notice from the Society of Arts, &c. I do not know whether they have offered premiums or medals for planting fruit trees, nor do I suppose it is always requisite, as I understand the Society confer their favours without such offers for matters they think deserving of them.

Two years ago I took a small farm in Essex, (a county where fruit is scarce,) consisting of near fifty acres. As the soil appeared proper, and the aspect favourable, I converted the whole into an orchard, by planting one hundred trees on each acre, in the following manner, viz. The fruit trees are placed in rows one rod asunder; between the trees in each row is a space of two rods; the plants are cherries, and apples or pears alternately, so that one half of the plantation consists of cherry trees. In about twenty or thirty years the apple and pear trees will require the whole of the ground; the cherry trees are then to be cut out, leaving the apple and pear trees uniformly two rods asunder each way, and in straight lines.

Farm of fifty acres converted into an orchard.

The orchard is now completed with the best kinds known or produced in the nurseries, in the whole nearly five thousand standard trees. They are well staked, and have been properly pruned twice a year. Farming crops have been since produced on the same ground as good as formerly, the plough being allowed to go within two feet of the trees each way, so that for many years to come the land will pay the expenses, and yield a profit exclusive of the fruit. I have in one part planted medlars, quinces, plums, walnuts, and other trees, to make the fruit collection as complete as possible, and I have spared no expense which could tend to improve the whole.

Farming crops produced on the ground as before.

* Trans. of the Society of Arts, vol. XXVI, p. 123. The silver medal of the Society was voted to Mr. Curtis for this communication.

I shall

Destruction of insects.

I shall make it an object to destroy the coccus, an insect which is at present damaging all our orchards. I know the application of spirits of turpentine will do it, without injuring the trees; it is by far the most easy and expeditious method for that purpose.

I am, Sir, your obedient Servant,

SAMUEL CURTIS.

Testimonies.

Certificates from M. P. Carter, D. D. Rector of Bradwell, and Mr. Thomas Fairhead, Churchwarden, confirmed, that Mr. S. Curtis had planted about four thousand standard fruit trees on about forty-eight acres of land, and that the same were, on the 7th of April, 1808, in a thriving condition.

SIR,

THE certificate I sent you relative to my orchard stated the number of trees to be about four thousand, but the real number is 4620 trees. I am sorry to have occasion to notice to you a disease in pear trees, almost as destructive, although not so frequent as that I mentioned to be produced from the insect on apple trees. This upon pear trees appears as a dry rotten scab, which keeps increasing until it penetrates even the hard wood, and as it proceeds, surrounds the limb entirely. The following spring the limb dies from the diseased part upwards. I have not found any insect to be concerned in this disease, which frequently takes place upon the trees of most luxurious growth. Its commencement seems to be from the thick rind of the tree becoming spongy; it then begins to crack and look scabby, the inner bark becomes dark coloured, and the disease proceeds until the destruction of the limb takes place. Some particular sorts of pear trees are with me much more liable to this disease than others: Windsor, autumn, bergamots, Catharine pears, &c. I suspect the disease to arise in a great measure from the soil.

Disease in pear trees.

My new orchard is situate at Glazen Wood, near Coggeshall, in Essex. I think myself highly honoured by the inquiries of her Serene Highness the Margravine of Anspach concerning it. As her Highness has attended to the pruning of fruit trees both in England and on the Continent, doubtless she is aware of the existing diseases in apple and pear

Remedy for this, and that of apple trees wanting.

pear trees,—any easy remedy for them would be of immense consequence; and if her Highness can furnish any discovery relative thereto, it would confer a great service on the public, and be esteemed by me a very particular obligation and honour.

I remain, Sir, your obedient servant,

SAMUEL CURTIS.

XII.

On the Management of Marsh Lands, Irrigation, &c. in a Letter to a Friend. By Mr. THOMAS DAVIS*.

SIR,

WITH respect to the management of Marsh Lands *after draining*, the great desideratum is to make them perfectly dry—to get rid of the coarse aquatic grasses, and to replace them with the finest and best grasses; and as the latter root is much shallower than the former, they cannot be made to thrive, unless the land is firm and close round their roots. There are but few instances where land of this description does not contain plenty of the best grasses, but in such a weak and starved state, that you can scarcely see them until the land is drained, and made so firm in its surface as to discourage all the coarser, and encourage the finer grasses, by bringing vegetation near the surface, and affording a proper nidus for the small shallow root of the latter. Management of marsh lands after draining.

But between the decay of the coarser grasses and the establishment of a better kind, there will be an *interregnum*, in which the land will be worked very little; *in some instances less than before it was drained at all.* The enclosures and drainage of the marsh lands (called moors) in Somersetshire, and the fens in Lincolnshire, have shown this clearly, and the same cause must produce the same effect every where. At first generally appears worse.

For the first three or four years after the drainage, the land has generally grown gradually worse; for two more, it has been stationary; and then, if well managed, and particularly by the help of a dry summer, it has improved rapidly, and will never, unless shamefully neglected, revert to the former state. Afterward improves.

* Bath Society's Papers, vol. X, p. 324.

But

This improvement may be accelerated.

But this *interregnum* may be much shortened, by reflecting on its cause, and acting accordingly. If the coarse grasses are to be destroyed, they must not be suffered to seed. If the shallow-rooting fine grasses are to be encouraged, the earth must be trodden into contact with their roots; of course mowing should be avoided, and feeding *in dry weather, as hard as possible, encouraged*; and the stock should be that of the *cow kind*. Horses eat very unfairly, and are continually running about and poaching the ground; and sheep will pick out all the fine grasses, and leave the coarse. But the surface water must mostly be drained off; and feeding in wet weather, particularly in the winter, avoided as much as possible.

The under water must be complete.

I am supposing all this while, that the land has been completely drained of its *under-water*, or else it is useless to attempt any thing towards its improvement. Manure may as well be thrown into the water, as put upon land, which (though not always under water) *is full of water* every winter. Besides, the *under-water* of marsh land, particularly under the hills which contain veins of blue lyas stone, as in Lincolnshire and Somerset, is frequently so impregnated with sulphur, as to be injurious to vegetation; and the land never improves much, till this water is completely drained and kept out of it.

Suitable manure requisite.

When land of this description is recovered, and well stocked with good grasses as above described; these grasses should be encouraged by such manures as suit the soil, such as wood-ashes, peat-ashes, soot, and other top-dressings in the spring, till the grasses are completely established; and then lime, chalk, marl, clay, sand, or whatever suits the land best, may be used in large quantities as *alterative manures*, but not until there is *a good coat of grass on the land*. In the choice of these manures, local manures will be useful; theory, on the soundest principles, is sometimes fallacious. But the golden rule of agriculture—to use such

Principles of manuring.

manures as will make heavy land lighter, and light land heavier; cold land hotter, and hot land colder—must never be lost sight of. He that knows and follows this rule, and he only, is a farmer.

If any of your land be capable of irrigation, and you have *water*

water enough to do it properly, (the great errour has been in attempting too much land with a given quantity of water) no improvement can be so great. But the land must not only be first drained of its under-water, but must be by nature, or made by art, capable of draining itself, and that speedily, from the water to be brought on by irrigation, or the attempt should not be made; and marsh land is seldom in this shape, unless *a river runs through it*, and there is of course a natural fall in the land: where you have this advantage, embrace it by all means; if you have not, be shy of attemping any thing on a large scale, until you have consulted some one who perfectly understands the subject. With all the improvements to be derived from irrigation, (and it certainly is the greatest improvement in agriculture) local prejudices, in countries where it is but little known, are strong against it. Every thing may look favourably, and yet the water may not agree with the land, or the land with the water; and the owner may be put to a great expence, and not only be disappointed, but what is to the full as vexatious, be laughed at by all his neighbours. Begin therefore with a little, and do that little well. You must not pretend to undertake irrigation by any written instructions, which I or any one else can give you. You must get a man who understands the subject practically, and who will undertake it at a fixed price per acre. But even then I would do but little at first, then wait a year, and see the effects, before I would go farther. And by the by, it is absolutely necessary, that your own workmen should see the effects, and understand the subject, and be fond of it; for every farmer, let him profess what he will, is governed by his own workmen; and whatever he may attempt to do will never fully succeed, unless he can get them to like it as well as himself.

Irrigation.

Necessary to convince the minds of workmen.

I am, &c.

Horsingham, Oct. 1805. THOMAS DAVIS.

METEOROLOGICAL JOURNAL

For APRIL, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

MAR. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
26	42	44	48	38	29·17	Fair	Fair
27	44	46	52	40	29·30	Ditto	Ditto
28	45	42	50	39	29·45	Rain	Rain
29	42	40	45	35	29·72	Fair	Fair
30	38	42	44	38	29·83	Ditto	Ditto
31	42	40	49	36	29·79	Ditto	Cloudy *
APRIL							
1	40	39	45	31	29·73	Ditto	Fair
2	38	36	43	29	29·80	Hail †	Ditto
3	34	37	43	28	29·90	Ditto	Ditto
4	35	28	40	32	30·05	Ditto	Ditto
5	34	36	42	30	30·26	Ditto	Ditto
6	36	36	44	30	30·27	Rain	Rain
7	38	39	46	28	30·16	Cloudy	Cloudy
8	40	42	50	37	30·33	Fair	Ditto ‡
9	42	46	52	44	30·14	Ditto	Rain
10	46	50	56	45	29·91	Rain	Cloudy
11	48	42	53	32	29·56	Ditto	Ditto
12	40	43	48	40	29·80	Fair	Rain §
13	42	44	52	38	29·32	Rain	Cloudy ‖
14	42	45	46	40	29·09	Ditto ¶	Ditto
15	43	46	50	40	29·47	Fair	Ditto
16	46	47	53	41	29·08	Ditto	Rain
17	41	40	43	34	29·13	Rain	Ditto
18	36	37	42	32	29·57	Hail	Fair
19	34	39	43	35	29·77	Fair	Ditto
20	40	38	42	35	29·73	Snow	Rain **
21	38	40	42	39	29·58	Ditto	Cloudy
22	44	46	47	45	29·67	Rain	Fair
23	43	41	46	40	30·01	Ditto	Cloudy
24	41	43	48	38	29·28	Fair	Ditto

* With very cold wind.

† From 9 A. M. to 1 P. M. the thermometer rose 4°, and during the heavy storm of hail fell to 38, and afterwards rose to 43.

‡ Venus and Mars visible at times.

§ Very high wind at 11 P. M. ‖ Lightning at 9 P. M.

¶ Thunder at half past 6 A. M.; again, with hail and lightning, at 1 P. M.

** Heavy snow at 7 P. M.; again, during the night of the 21st.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1809.

ARTICLE I.

Observations on the Natural History of the Divers. In a Letter from PATRICK NEILL, *Esq., Secretary to the Wernerian Natural History Society.*

To Mr. NICHOLSON.

DEAR SIR,

HAVING paid some attention to the natural history of the Divers, I have subjoined some remarks in answer to your correspondent's inquiries concerning the *Ember-goose*. And am, with esteem,

Yours,

Edinburgh, March 17, 1809. PAT. NEILL.

The Danish clergyman, whose account is quoted by your correspondent, is said to affirm, that the ember-goose "lives constantly on *dry land;* and although it has been often seen with grown up young, no person has ever found its

Blunder in the Danish clergyman's account of the immer diver

its nest." There is here, in my opinion, a palpable blunder, which must have arisen either from a mistranslation, or from the accidental omission of some words. If the original were consulted, I should not be surprised to find it run thus: "The ember lives constantly at sea, and is never seen on the dry land," &c. That this must be the import, seems evident from the rest of the account. If the ember lived constantly on the dry land, in the narrow and confined islands of Feroe, the nest and young of so large and remarkable a bird must have been familiar to the natives; yet we are told, that, "although it has been often seen with grown up young, no person has ever yet found its nest." It is indeed added, "As it has a large hole under each wing, many have imagined, that it there hatches its eggs."

probably a mistranslation.

Improbability of the account.

Supposing that the eggs were really hatched in hollows under the wings, (which is too extravagant a notion to be granted without complete proof) we cannot for a moment believe, that the young could remain there till they were "grown up." But further, if the ember lived constantly on dry land, there would evidently be no occasion at all for this singularity in the manner of hatching its eggs; which, on the other hand, might seem commodious, on the supposition that the bird lived constantly at sea. And the opinion, that it does live constantly at sea, has procured it sometimes the striking appellation of the "herdsman of the sea."

Opinion of the Orkney and Shetland islanders.

If any confirmation be wanted, I may state, that, by the correction I have suggested, the Feroe account of the ember is brought to agree perfectly with the opinion entertained at this day by the common people in the Orkney and Shetland islands. These, it will be recollected, were formerly subject to the Crown of Denmark, and ultimately connected with the Feroes. That the vulgar notions, therefore, prevalent in our own northern islands and in the Feroes should coincide, is extremely natural: that they should be directly contrary to each other, seems exceedingly unnatural and improbable.

Inquiries in these islands.

In the course of visiting many of these islands in the summer of 1804, I made frequent inquiries concerning the habits

habits of the ember-goose, both of the best informed gentlemen, and of the fishermen and common people.

The common people believe it hatches its eggs under the wing.

By the latter class I was uniformly assured, that the ember continues constantly at sea, without ever touching the land; and that it hatches its eggs in holes under its wings. This last opinion I found was adopted, because, though the ember is never seen on land, nor have its nest or eggs ever been discovered in the islands, yet the old ember is frequently observed in the friths and bays, attended by a couple of young ones. I remarked that, both in the Orkney and the Shetland islands, the common people in general made no distinction between the true immer and the northern diver, but included both under the name of embergeese: some fishermen, however, denominated the northern diver, the great emmer, or ember; but the hatching of the egg under the wing was supposed to be equally characteristic of both.

Account given by the better informed.

From the gentlemen resident in both sets of islands, who were sportsmen, or had been sportsmen in their youth, I learned, that both the true immer, colymbus immer, and the northern diver, colymbus glacialis, frequent the friths and bays during the whole year, and very much resemble each other in their habits; only the northern diver is observed to be more common in winter than in summer, while the emmer is equally common all the year round. On this account some gentlemen were of opinion, that this last might probably breed in some of the unfrequented *holms* *; but they acknowledged, that its nest had never been found: indeed neither species had ever been seen to go ashore; far less been known to breed. I was told, that when pursued by a boat, both kinds swim with astonishing velocity; when approached, they dive very rapidly; and occasionally changing their course under water, rise to the surface at a great distance, and in a quarter altogether unexpected; thus baffling the efforts of their pursuers. When suddenly surprised, or very much teazed, they sometimes, though but rarely, run along the water, beating it violently with their wings, and uttering cries not unlike the howlings of some

* A *holm* is a small uninhabited island, used only for pasture.

small dogs; but they have never been observed to get full on wing, or even to attempt an elevated flight. The young ones, which are seen accompanying them, are always, I learned, of sufficient size, to render it possible that they may have come from a great distance, perhaps Iceland, Norway, or Greenland: this is an important remark, and the testimony was uniform.

Perhaps breeds far to the north.

There is no hole, or remarkable hollow under the wing.

In regard to the alleged hole under the wing, I can assure your correspondent, that no such hole exists. I affirm this, not only from having myself examined prepared specimens of the immer, in which no trace of such a cavity existed; but on the authority of those who have shot the bird, or caught it, as sometimes happens, on a baited hook on a sunk line; and who declared, that on examination they found no greater hollow under the wing of the ember than may be seen under the wing of the common goose. The same thing may be affirmed of the northern diver. I have at different times procured large and full grown specimens of this beautiful bird, which were found entangled in nets set in the Frith of Forth for thornback and skate, in the months of April and May; and in none of these were there any remarkable hollows under the wing.

Accounts given by various authors.

I shall close these remarks (which have already, perhaps extended to too great a length) with some slight notice of the accounts to be found in books.

Wallace, sen.

The elder Wallace, in his History of Orkney, 1693 gravely states, that the emmer "has its nest and hatches its eggs under the water." Brand, a visitor sent to the islands by the General Assembly of the Church of Scotland in his Description, published 1701, repeats the same story with equal solemnity: "It hath its nest wherein it hatcheth its eggs, one or two at once, under the water at the foot of a rock, as they informed me hath been found." Sir Robert Sibbald, rather incautiously following these authors, gives a similar account. The other notion, of its hatching its eggs under the wing, is countenanced by Pontoppidan, in his History of Norway, 1751.

Brand.

Sir R. Sibbald.

Pontoppidan.

Horrebow.

Horrebow, however, in his Natural History of Iceland 1758, gives a much more natural and rational account "The *lom*," he says, "is unmolested; for the people give themselves

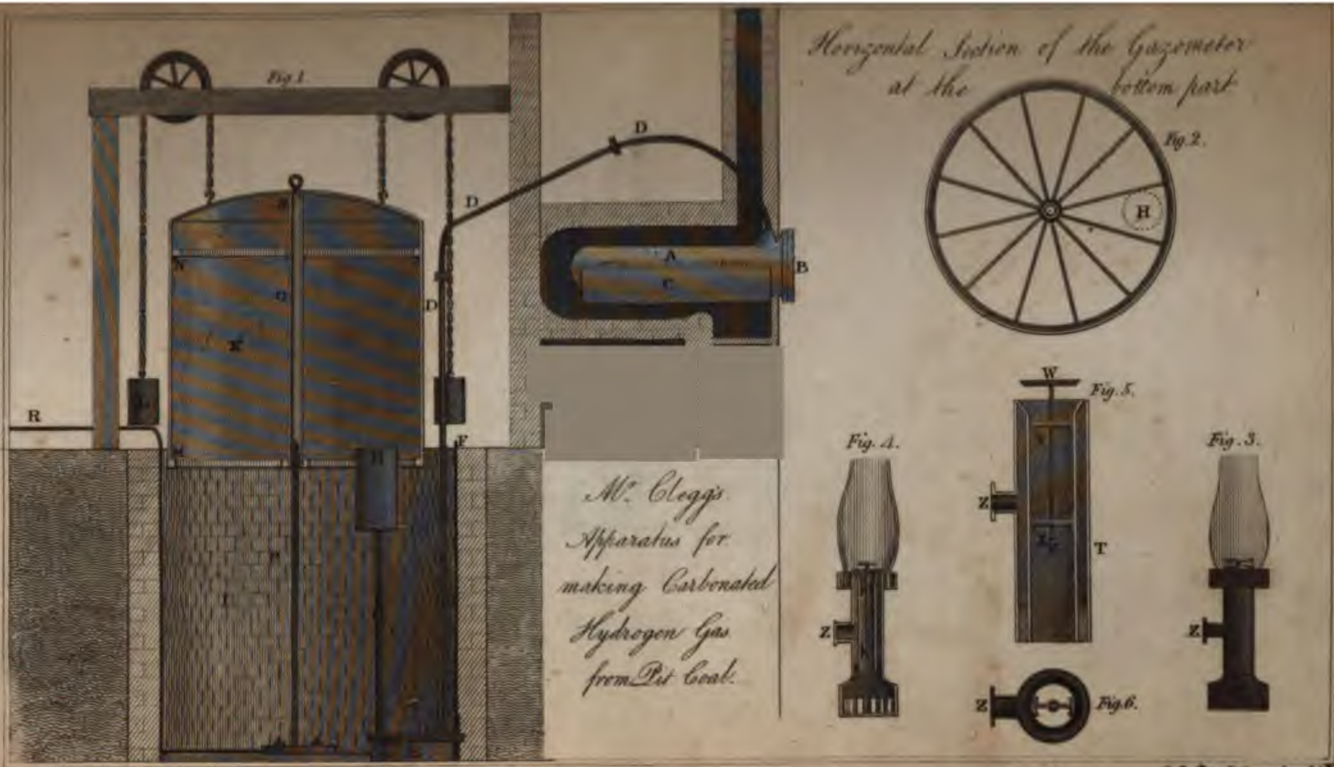


Fig. 1.
R
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A
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C
Horizontal Section of the Gazometer at the bottom part
Fig. 2.
H
Mr. Clegg's Apparatus for making Carbonated Hydrogen Gas from Pit Coal.
Fig. 4.
Z
W
Fig. 5.
Z
T
Fig. 3.
Z
Z
Fig. 6.

themselves no trouble to look after its nest or brood, neither their flesh nor eggs being fit to be eaten. They build in remote places near fresh water;" so near, as we learn elsewhere, that the bird may almost slide into the water. It is not perhaps easy to determine, whether, by the term *lom*, Horrebow here means the proper immer, or the northern diver; but it matters not. As the habits of both are in other respects much alike, and as the breeding of the northern diver is held to be of the same mysterious nature as that of the immer, we may reasonably conclude, that both perform the offices of incubation in places of the same sort, and in a manner somewhat similar.

Upon consulting Colonel Montagu's Ornithological Dictionary, (2 vols, 8vo, 1802) a work in general of the greatest accuracy, I find, that in regard to the immer, without taking notice of any of the fabulous reports above detailed, he merely states, that "it makes a nest on the water, placed amongst the reeds and flags," in fresh water lakes. He does not, however, mention any authorities. As to the northern diver, he observes, that "it is not uncommon in Iceland and Greenland, where it breeds in the fresh waters, and is said to lay two large eggs, of a pale brown colour, in the month of June." He mentions that this bird seldom leaves the water; but that, in the spring of 1797, one was taken near Penzance in Cornwall, at some distance from water. It appeared incapable of raising itself from the ground, yet did not seem to have any defect. It lived for six weeks in a pond, eating fish thrown to it.

Col. Montagu.

A northern diver taken some distance from water.

II.

Description of an Apparatus for making carburetted Hidrogen Gas from Pitcoal, and lighting Manufactories with it. By Mr. SAMUEL CLEGG, *of Manchester**.

DEAR SIR,

WHEN your son was in Manchester, he called to see my nephew, Samuel Clegg's, improved gas lights, and was

Mr Clegg.

* Trans. of Soc. of Arts, vol. XXVI, p. 202. The silver medal was voted to Mr. Clegg for this communication.

desirous

desirous to have a plan of his method, which my nephew promised him, and I undertook to get it conveyed to you. I have, accordingly, taken the opportunity of sending to the Society of Arts a plan and explanation of his apparatus.

used gas lights some years ago, and freed them from offensive smell.

He lighted a large manufactory in Yorkshire some years ago upon this principle, and has since lighted some buildings in this neighbourhood, and I believe he is the first person, who succeeded in rendering these lights free from the offensive smell which generally accompanies them. My nephew served an apprenticeship to Messrs. Boulton and Watt, of Birmingham, in the steam engine business, in which he is now engaged here on his own account, and has made considerable improvements in their construction.

I remain, dear Sir,

Your most obedient servant,

ASHWORTH CLEGG.

Manchester, May 18, 1808.

SIR,

Cost of the apparatus.

Your esteemed favour I have received, and, according to your request, have sent you a fuller explanation of the gasometer and lamp, accompanied with farther drawings.

A gasometer, containing seven hundred cubical feet of gas, weighs about twenty hundred weight, and costs about two pounds ten shillings the hundred weight.

The whole of an apparatus complete, capable of supporting forty lamps for four hours, each lamp affording light equal to ten candles of eight in the pound, will cost about two hundred and fifty pounds. Each lamp consumes six cubical feet of gas per hour, I am happy to find, that the Society have honoured my communications with their attention, and I remain, with great respect,

SIR,

Your most obedient servant,

S. CLEGG.

Manchester, Aug. 12, 1808.

Reference

Reference to Mr. S. Clegg's improved Apparatus for extracting Carburetted Hidrogen Gas from Pit Coal. See Plate III, figs. 1, 2, 3, 4, 5, *and* 6.

In fig. 1, *A* shows the cast iron retort, into which are put the coals intended to be decomposed by means of a fire underneath it, the heat of which surrounds every part of it, excepting the mouth or part by which the coals are introduced. The lid or iron plate *B*, which covers the mouth of the retort, is ground on air tight, and fastened by means of a screw in the centre; *C* is a shield or saddle of cast iron, to preserve the retort from being injured by the intensity of the fire underneath it, and to cause it to be heated more uniformly. *D D D* represents the cast iron pipe which conveys all the volatile products of the coal to the refrigeratory of cast iron *E*, in which the tar, &c., extracted from the coal is deposited, and whence they can be pumped out by means of the copper pipe *F*. *G* is the pipe which conveys the gas to the top of the cylindrical vessel or receiver *H*; this receiver is air tight at the top, and consequently the gas displaces the water in the vessel *H*, to a level with the small holes, where the gas is suffered to escape and rise through the water of the well *I*, into the large gasometer *K*. The use of the vessel *H* is pointed out as follows, viz. If the pipe *G* reached all through the water, without passing into the vessel *H*, the gas would not be rendered pure or washed; and if part of the pipe did not rise above the water, the water would have free communication with the tar, besides exposing the retort *A* to a very great pressure, so as to endanger its bursting when red hot. This vessel or receiver *H*, in a large apparatus, is about eighteen inches diameter, and two feet long; the quantity of gas, therefore, which it contains, is sufficient to fill the pipes and retort when cool, prevent the pipe *G* from acting as a siphon, and expose the gas to the water without endangering the retort.

Description of the apparatus.

When the operation begins, the upper part of the cylindrical gasometer *K*, fig. 1, made of wrought iron plates, is sunk down nearly to a level with the top of the circular well *I*, and is consequently nearly filled with water, but it rises gradually

Description of the apparatus.

gradually as the gas enters it and displaces the water; the two weights *L L* suspended over pullies by chains keep it steady and prevent its turning round, otherwise the lower stays *M* of the gasometer would come into contact with the vessel *H*. There are two sets of these stays, one shown at *M*, and the other at *N*.

There is also an iron pipe *O*, made fast in the centre of the gasometer by means of the stays, which slides over the upright pipe *P*, by which contrivance the gasometer is kept firm and steady, when out of the well; it likewise prevents the gas from getting into the cast iron pipe *P*, and the copper pipe *R*, any where but through small holes made in the pipe *O* at *S* at the top of the gasometer, where the gas is perfectly transparent and fit for use.

The pure gas enters the tube *O* at the small holes made in its top at *S*, and passes on through the tubes *P* and *R* to the lamps, where it is consumed and burnt.

The seams of the gasometer are luted to make them air tight, and the whole well painted inside and out, to preserve it from rust.

Fig. 2 shows a horizontal section of the lower hoop of the gasometer *K* at the part *M*, with its stays or arms, and the manner in which the iron pipe *O*, before described in fig. 1, sliding on the tube *P*, passes through the ring in the centre of the hoop. A horizontal section of the receiver *H* appears therein.

Lamps for burning the gas.

Fig. 5 shows a section of one of the gas lamps. The space between the outer tube *T* and the inner tube *V*, is to be filled with gas supplied by the pipe *R*, shown in fig. 1, where a stop cock is inserted for adjusting the flame, which gas passes through a number of small holes made in the outer edge of a circular plate shown at fig. 6, which unites the tubes *T* and *V* at their tops. *V* is the inner tube which conveys the atmospheric air into the centre of the flame; the upper part of this tube is made conical, or widening outwards, to join a circular plate with holes in it, a horizontal view of which is shown at fig. 6. *W* is a button, which can be placed at a small distance above the mouth of the lamp, and its use is to convey, in an expanded manner, all the air which rises through this tube to the inner surface

of

of the flame, which assists the combustion very much; this button may be set at any convenient distance above the tubes of the lamp, as it slides in the cross bars *XX*, by which it is supported in the inner tube.

A current of air also passes between the glass tube or chimney and the outer tube *T*, through holes made in the bottom of the glass holder, as in Argand's lamps; this surrounds the flame, and completes its combustion, as explained by the view, fig. 3, and section, fig. 4, which have a glass upon each. *ZZZZ*, figs. 3, 4, 5, and 6, show the tube through which the lamp is supplied with gas from the pipe *R*, fig. 1.

III.

A Cheap Method of Preserving Fruit without Sugar, for Domestic Uses or Sea Stores. By Mr. THOMAS SADDINGTON, *No. 73, Lower Thames Street* *.

SIR,

I SHALL be much obliged to you to lay before the Society of Arts &c. the enclosed communication, and a box containing the following fruits in bottles, preserved without sugar, namely, apricots, gooseberries, currants, raspberries, cherries, Orleans plums, egg plums, green gages, damsons, and Siberian crabs. I have also sent some fresh English rhubarb plant, preserved in a similar manner. The same mode is applicable to other English fruits, as cranberries, barberries, and many more. This manner of preserving fruit will be found particularly useful on ship-board for sea stores, as the fruit is not likely to be injured by the motion of the ship, when the bottles are laid down on their sides, and the corks kept moist by the liquor, but on the contrary will keep well even in hot climates.

Fruits preserved by the new mode.

Applicable to others.

Particularly useful for sea.

* Trans. of the Soc. of Arts, vol. XXVI, p. 145. Five guineas were voted to Mr. Saddington for this invention.

The

and cheap. The cheapness of the process will render it deserving of the attention of all families from the highest to the lowest ranks of society. If the instructions I have sent are well attended to, I have no doubt, that whoever tries my method will find it to answer his expectation.

I am, Sir,

Your most obedient humble servant,

THOMAS SADDINGTON.

A new Method to preserve various Sorts of English Garden and Orchard Fruits, without Sugar.

Fruit generally useful, The general utility, as well as luxurious benefit, arising from the fruit produced by our gardens and orchards, is well known and acknowledged at the festive board of every family; nor is this utility and benefit less manifested by a desire of many persons to preserve them for culinary purposes in the more unbountiful season of the year; and I am well persuaded, that this commendable desire would be greatly extended in most families, was it not attended with so much expense as is generally the case by preserving fruit in the common mode with sugar, this article chiefly constituting the basis by which it is effected. In addition to the expense of sugar, which is frequently urged as a reason for not preserving, there are other objections to that method, and what I am about to mention cannot be considered as the least, namely, the great uncertainty of success, occasioned by the strong fermentable qualities contained in many sorts of fruit. It may be said by some, that fruit may be preserved for a length of time without sugar by the ordinary mode of baking or boiling, and being closely stopped up, to which assertion I freely assent; but even this method is frequently attended with uncertainty, for if the cork or other means used for keeping the external air out of the vessel becomes dry, or from any other cause the atmospheric air exchanges place with what is impregnated by the fruit, it soon becomes mouldy and unfit for use.

but preserving it expensive,

and the sugar apt to ferment,

or the fruit grow mouldy.

These disadvantages removed. From these considerations, and a desire of preserving fruits at a trifling expense, I have made various and successful

cessful experiments of doing it without sugar, and at the same time with a certainty of their retaining all those agreeable flavours which they naturally possess; and it is highly probable, that they will keep perfectly good for two or three years, or even a longer period, in any hot climate, by which it appears to become a valuable store for shipping or exportation, as I have exposed them to the action of the meridian sun in an upper room, during the whole of the summer, after they have been so preserved (being done in 1806). I have now the pleasure of laying before the Society specimens of the fruit alluded to.

Process for preserving Fruit.

The bottles I chiefly use for small fruit, such as gooseberries, currants, cherries, and raspberries, are selected from the widest necked of those used for wine, or porter, as they are procured at a much cheaper rate than what are generally called gooseberry bottles. Having got them properly cleaned, and the fruit ready picked, (which should not be too ripe,) fill such of them as you intend doing at one time, as full as they will hold, so as to admit the cork going in, frequently shaking the fruit down whilst filling. When done, fit the corks to each bottle, and stick them lightly in, so as to be easily taken out when the fruit is sufficiently scalded, which may be done either in a copper, or large kettle, or saucepan over the fire, first putting a coarse cloth of any sort at the bottom to prevent the heat of the fire from cracking the bottles: then fill the copper, or kettle, with cold water sufficiently high for the bottles to be nearly up to the top in it: put them in sideways to expel the air contained in the cavity under the bottom of the bottle; then light the fire if the copper is used, taking care that the bottles do not touch the bottom, or sides, which will endanger their bursting; and increase the heat gradually until it comes to about one hundred and sixty, or one hundred and seventy degrees, by a brewing thermometer, which generally requires about three quarters of an hour. For want of such an instrument it may be very well managed by judging of the degree of heat by the finger, which may be known by the water feeling very hot, but not so as Process described.

to

to scald it. If the water should be too hot, a little cold may be added to keep it of a proper temperature, or the fire may be slackened. When it arrives at a sufficient degree of heat, it must be kept at the same for about half an hour longer, which will at all times be quite enough, as a longer time, or greater heat, will crack the fruit.

During the time the bottles are increasing in heat, a tea kettle full of water must be got ready to boil as soon as the fruit is sufficiently done. If one fire only is used, the kettle containing the bottles must be removed half off the fire, when it is at the full heat required, to make room for boiling the water in the tea kettle. As soon as the fruit is properly scalded, and the water boiling, take the bottles out of the water one at a time, and fill them within an inch of the cork with the boiling water out of the tea kettle. Cork them down immediately, doing it gently, but very tight, by squeezing the cork in, but you must not shake them by driving the cork, as that will endanger the bursting of the bottles with the hot water; when they are corked, lay them down on their side, as by this means the cork keeps swelled, and prevents the air escaping out: let them lie until cold, when they may be removed to any convenient place of keeping, always observing to let them lie on their side until wanted for use. During the first month or two, after they are bottled, it will be necessary to turn the bottles a little round, once or twice in a week, to prevent the fermentation that will arise on some fruits from forming into a crust, by which proper attention, the fruit will be kept moist with the water, and no mould will ever take place. It will also be proper to turn the bottles a little round once or twice in a month afterwards.

Recapitulation. Having laid down the method of preserving fruit without sugar, in as clear and concise a manner as possible, I will recapitulate the whole in a few words, which may be easily remembered by any person. Fill the bottles quite full with fruit. Put the corks in loosely. Set them in a copper, or kettle of water. Increase the heat to scalding for about three quarters of an hour; when of a proper degree, keep at the same half an hour longer. Fill up with boiling water.

ter. Cork down tight. Lay them on their side until wanted for use.

Bottles.

It may be said as an additional reason, as well as cheapness, for using wine, or porter bottles, instead of gooseberry, that there is a difficulty of obtaining them, even at any price, in some parts of the country; and indeed they are equally useful for small fruit, and answer the purpose quite as well, excepting the little inconvenience of getting the fruit out when wanted for use, which may be easily done by first pouring all the liquor out into a bason, or any other vessel, and then with a bit of bent wire, or small iron meat skewer, the fruit may be raked out. Some of the liquor first poured off serves to put into the pies, tarts, or puddings, instead of water, as it is strongly impregnated with the virtues of the fruit, and the remainder may be boiled up with a little sugar, which makes a very rich and agreeable syrup.

Specimens.

In confirmation of the foregoing assertions, I now produce twenty-four bottles as samples, containing twelve different sorts of fruit, viz. apricots, rhubarb, gooseberries, currants, raspberries, cherries, plums, Orleans plums, egg plums, damsons, Siberian crabs*, and green gages—which have all been preserved in the manner above described.

The heat must not be too great, or too long continued.

In order to diversify the degree of heat, and time of continuance over the fire, I have done some in one hundred and ninety degrees, and continued them in it for three quarters of an hour; from which experiments it is evident, that the heat is too powerful, and the time too long, as the fruit by this degree and continuance is rendered nearly to a pulp†.

Cost.

In the summer of 1807 I preserved ninety-five bottles of fruit, the expense of which, (exclusive of bottles and corks) was £1 9*s.* 5½*d.*; but having some fruit left, it will not be right to judge them at a higher rate than £1 9*s.*; and allowing 5*s.* for the extra coals consumed in consequence of

* Apples and pears may be done for shipping, &c.

† Some of these samples of 1807, were done in 180 and 190 degrees.

my

my not having a conveniency of doing more than seven or eight at a time, and this being done at fourteen different times, it will amount to £1 14s.; the average cost of which is nearly 4½d. per bottle, exclusive of the trouble of attending them. But if we estimate their value in the winter season at 1s. the bottle, this being in general as low or lower than the market price, they will produce £4 15s.; but losing one bottle by accident, reduces it to £4 14s., leaving a net profit of £3 on ninety-four bottles, being a clear gain of nearly two hundred per cent.

Profit.

For ship's stores.

Another great advantage resulting from this statement will appear by making it an article of store for shipping, or exportation; and I shall submit a few ideas tending to promote such a beneficial object by doing it in large quantities; for which purpose sufficiently extensive premises must be fitted up, with a proper number of shelves, one above another, at a distance of about five inches.

Method of doing it on a large scale.

The vessel for scalding the fruit in should be a long wooden trough of six, eight, or ten feet in length, two or three in breadth, and one in depth, fitted with laths across to keep the bottles upright, and from falling against one another; this trough of water to have the heat communicated to it by steam, through a pipe from a closed boiler at a little distance. The boiling water, wanted to fill the bottles with, may be conveyed through a pipe and cock over the trough, by which arrangement, many hundreds of bottles might be done in a short time. It may be prudent to observe, that this idea is only speculative, not having been actually practised, but at the same time seems to carry with it a great probability of success, and worthy the experiment.

It remains now, that I state some reason or object for troubling the Society, whom I have taken the liberty to address with these communications. The first is a desire of publicity, sanctioned by their investigation of the experiments made for preserving fruit without sugar, thereby lessening the expense attending an object of so much public benefit and utility. The second arises from a personal or private consideration; but on this subject I shall only observe, that I wish to throw myself entirely on that protection

lection which has ever characterised the liberality of the Society; and that I shall feel highly honoured, if they conceive what I have communicated deserving any mark of their favour.

I am, Gentlemen,

Your most obedient humble servant,

THOMAS SADDINGTON.

IV.

On Reclaiming Waste Lands. By *Mr.* WAGSTAFFE *.

GENTLEMEN, *Norwich, June 27,* 1801.

AS your influence for the enclosure of Waste Land is confessed, and, I conceive, extending within the scope of your Society, and it should now seem on the eve of a Parliamentary encouragement; I ask leave to recite an experiment I made on a portion of land, of as obvious sterility as perhaps any present waste within the Western counties. Waste land

This was an acclivity, which had not been cultivated within memory; and at the foot of it a various tract, gravelly and moory, broken into hollow spaces, in which waters rested during the summer months, which waters were covered with most of the aquatic plants native to stagnant pools. My predecessor in possession of these watery wastes, during a summer drought, fed their interstices with sheep, which became diseased, and many of them rotten. described.

The mode I pursued was as much as might be to extract the weeds, roots, and sediment; lay them in heaps as a preparation of manure measurably to replace and fertilize the barren sands and gravel, brought from the heights to fill up these hollows. I then opened ditches, raised their sides with sand and gravel, and on them planted large cuttings of poplars and willows. The ditching drained the soil, and the materials from the heights raised this swamp Steps taken to improve it.

* Bath Papers, vol. X, p. 18.

Fence. to the proper condition of meadow. The upland I enclosed with thorns on a willow ley *, and within the banks inlaid them with seedling trees and forest; divers of the former have been taken down for use, and some of the aquatic cuttings are grown to a timber measure; while the several subdivisions, meadow and upland, have been cultivated, and borne every species of grain and herbage, confessedly upon an equality with the long tillaged circumjacent fields.

The process applicable to great extent. By a process thus pursued, of which I have presumed to adduce this example, the numerous millions of waste acres, which yet disfigure our nation, may and will become, the seasons favouring, under your and your compatriots' encouragement, a widely extended garden, replete with every useful production congenial to our climate; and the boundary of its fields fenced with faster thriving trees, and more abundant in number than the present large tracts of

Timber. forest produce, provide for generations yet to come an increase of those necessary timbers, that have given this island an intercourse with the inhabitants of every maritime clime, and an acknowledged superiority in the commercial world, which probably it would not have obtained but from the indigenous growth of these not sufficiently valued timbers. Although your extended encouragements have much increased them by multiplied plantations, yet their growth may be indefinitely enlarged by an encouragement for their acorn seed to be placed in every raised bank, or their seedlings planted in every new formed hedge-row; which most efficaciously might be enforced by Parliament as a conditional obligation on all to whom they are assigned, under the statute of a national enclosure. But as every seminary of oaks must be referable to a distant posterity, it becomes worthy of every present planter in the interior of his hedge-rows to have large cuttings of poplar † and willow,

* A willow fence in this situation has the appearance of improbability, but it is yet improving.

Different poplars. † Of poplars, the *nigra*, *alba*, and *hybridum*; this latter hath not, I conceive, found its way into any systematical arrangement of plants, and in course has not received any specific character. The name assigned

willow *, and an intermixture with young trees of the resinous tribe. Those I have already known may be taken down as timber during the life of the planter, and as early as the inlays are grown to afford shelter and shade to the herd and the flock, that occasionally feed within their enclosures.

I may just add, the fall of the autumnal leaf, with the manure of the depasturing cattle, may continue the fertility of these fields without extraneous aid; and where not readily procurable, I may farther add, that in the latter end of the autumn of 1799 I procured turves from different wastes, reserved them on a gravel walk, and thereon dibbled wheat, almost every grain of which succeeded, branched into divers stems, which severally bore a full and perfect grain. In the autumn of 1800 † I repeated the trial, which at this instant is as promising as the other proved. The early spring of this year, 1801, I practised the same mode with tares, pease, oats, and barley, which severally are promising. I bring forward these experiments to show, that generally every waste may be rendered productive by the first simple operation of the plough, and thereby supersede the long process pursued by many; call forth to the earliest production the unprofitable wastes of the kingdom; and hence, as far as human foresight can discover, prevent such a sensible

The soil of various wastes not unsuited to grain.

signed it is on the opinion of a gentleman well acquainted with botanic distinction, who conceives it to be a variety, perhaps of the two former. I may speak from an enlarging experience, that it is a handsome and fast growing tree, multiplies itself distinctly from its roots, while its cuttings take with nearly equal facility as the two former.

* *Pendandria*, (laurel leaved) *amygdalina*, (almond leaf) *alba*, (common gray leaf.) These three species I know, or presume, on the progress the first has already made, will severally grow to a timber bulk. The prospective diversity of contrasted foliage can perhaps be not better exemplified than in the vivid green of the laurel willow, and the heary leaf of the white poplar.

Willows for timber.

† There is an average of four large ears to every grain dibbled, now in full flower, which conveys an expectation of more than a hundred fold increase, the actual increase of the preceding year. These turves or flags have received no aid from manure, or any artificial watering.

 scarcity

scarcity as most of our provinces have recently felt. And again, under the blessing of Providence, witness a competency for ourselves, and a surplus for other nations; and thence be commercially beneficial to a large portion of mankind.

I am, with sincere regard,

Your respectful friend,

JOHN WAGSTAFFE.

V.

Account of Waste Land improved by J. BUTLER, *Esq. of Bramshott, in Hampshire**.

SIR,

Waste land. IN the year 1802 I purchased an estate, situate in the parish of Bramshott, in the county of Hants, of which seventy acres and upwards were then waste lands, growing a little timber, furze, and alder, and supporting a few cows in the summer, but never cultivated or considered worth that expense.

General state. From particular engagements at the time, I did not begin any improvement till 1804, when I found sixty-five acres and a half (statute) of the said waste lands in the following state: twelve acres, the site of old fish ponds, growing nothing but reeds and rubbish; eighteen acres one rood thirty-seven perches, affording a little sour grass and a few alders in wet places; twenty-seven acres three roods one perch, quite a morass or bog, with a few alders; and seven acres one rood four perches of very indifferent furze.

First drained. As the greatest part of the waste was filled with innumerable springs that deluged the whole, and caused the bog to be saturated throughout the year, I considered that

* Trans. of the Soc. of Arts, vol. XXVI, p. 117. The silver medal of the society was voted to Mr. Butler.

draining

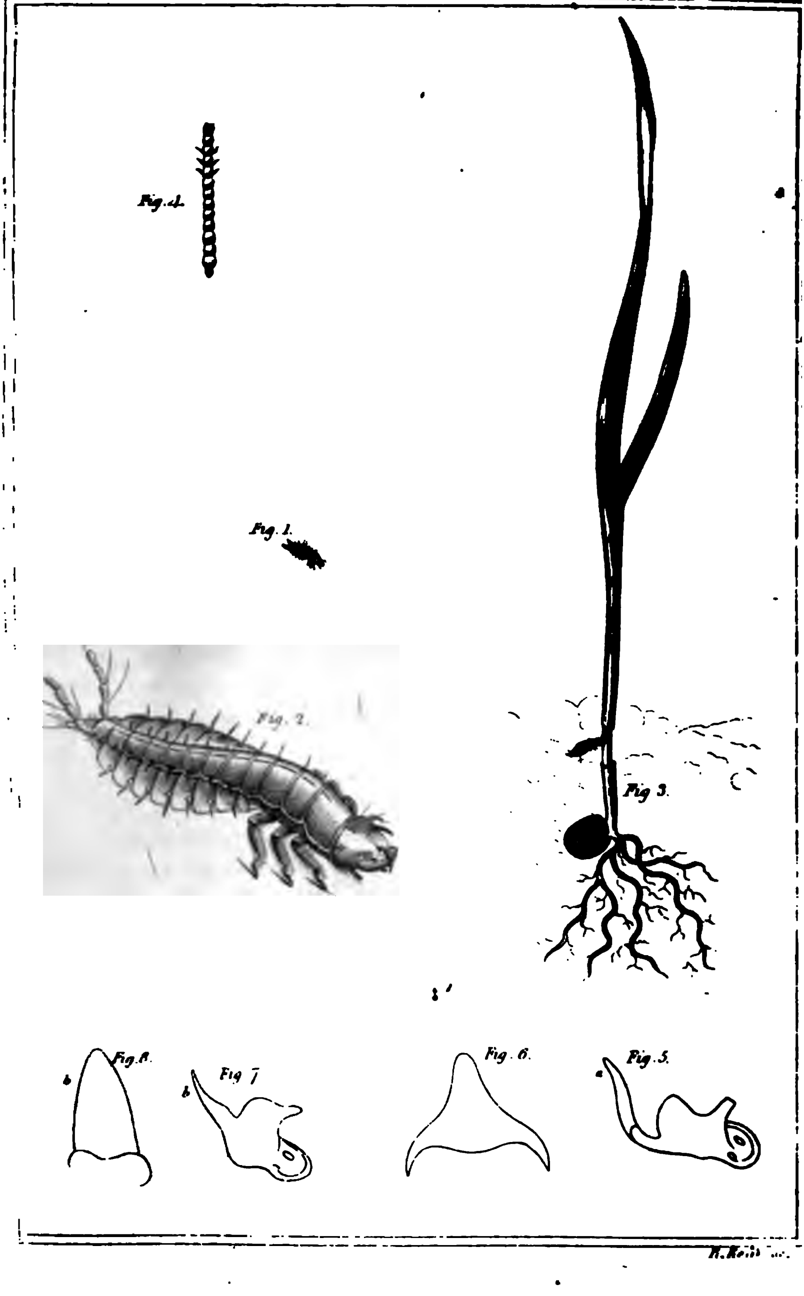


Fig. 4.
Fig. 1.
Fig. 2.
Fig 3.
Fig. 8.
b
Fig 7
b
Fig. 6.
Fig. 5.
a

draining was the first necessary step to be taken. With this view I made an open cut or ditch, of five to seven feet deep or more, in the lowest part of the bog, to let out the stagnated water, and ascertain with precision the cause that produced it. Having obtained the lowest possible fall by the open cut or ditch, I caused other cuts to be made to the heads of different springs which fed the land, occasionally boring with the auger, that no spring might be passed over; and I then laid the open cuts or drains with stones from three to twelve feet below the surface, according to circumstances, to carry off the water, observing always to keep the level.

At the highest ground I found rocks, under which the principal springs lay at the distance of at least fifteen feet, and thence an immensity of water gushed out, which was easily passed off through the drains, and I had the satisfaction to find, that in the course of two years the whole waste became perfectly dry, and so continued. Principal springs.

The extent of land thus drained being great, the cost of course is very considerable, and amounts to the sum of £338 2s. 11d. Cost.

During this I grubbed the greatest part of the land, which from the stems of oak and other timber that had formerly grown there, as well as the alder, furze, and some timber standing at the time I made the purchase, was no inconsiderable work, and cost for each oak stem ninepence, and for the soil on which the alders and furze grew, sixpence per rod, amounting in the whole to the sum of £95. Expense of grubbing.

The ground being now cleared, I ascertained, by the means of a water level, the position of a little brook which ran through the waste land, and found that it was practicable to turn water over thirty acres of it. This being an object of the first consequence, I spared neither pains nor expense to accomplish it. I removed the high banks round the fish ponds, which contained some thousand loads of soil; filled up very deep ditches and stew ponds, and laid several acres on an inclined plane; burnt the roots and rubbish, and prepared, by levelling and making water carriages, thirty acres for irrigation, of which sixteen acres, Prepared for irrigation.

 though

though rough and some pasture as before described, had by draining and feeding began to improve considerably; and in the spring of 1806, I was enabled to turn the water over such sixteen acres, from which I derived a tolerable crop of hay in that summer. Feeding it harder afterward, and watering it the following winter, there was a good supply of feed for sheep till the latter end of April; when it was laid up and watered as before, but with far better success, as the crop was not only greatly improved in quality, but likewise in quantity, producing more than two tons to an acre throughout the sixteen acres.

The residue of the thirty acres prepared for irrigation as before stated, formerly fish ponds and other rough lands, but lately levelled and sown with perpetual grasses, is now looking remarkably well, and will certainly be in readiness to receive the water, as soon as the land is firm enough for this purpose.

Cost.

In accomplishing this work I had the assistance of Joseph Trigger, who lived with and managed the water meadows of the late Mr. Bakewell of Dishley for more than twenty years; and it would be an act of injustice to him not to say, that the said land is prepared for a water meadow in a masterly style. This cost me not less than £223.

Pared and burned.

As soon as I perceived the effect of my drains on the bog, which was composed of a good deep peat, I pared by hand thirteen acres of it, which I burnt, and spread the ashes; then ploughed the land once, and sowed it with cole seed in the month of July. The crop turned out very good, and fed one hundred and sixty two-shear hogs (Leicester) for two months; after which I ploughed it once only, and sowed it with white oats in the month of April 1807. At first the oats appeared sickly, but receiving a few warm showers in May, they recovered and flourished exceedingly, making a most excellent appearance, to the astonishment of the neighbourhood; for when reaped, they were estimated at from ten to thirteen quarters per acre, some parts being preferable to others, but the whole good; and I have no doubt, for at present they are not threshed, that the crop will amount to its estimate.

Sowed cole-seed, and next white oats.

In the course of last year I pared seven acres more of the said

said bog, and burnt and spread the ashes in a similar way to the former, and sowed it with cole seed from one ploughing in July last, which likewise turned out a most excellent crop, and supported seventy-two large sheep on it for more than two months.

The expense of paring and burning the twenty acres came to £29 10*s*. Expense.

The remaining waste land being a lighter peat, mixed more with sand, I did not think it advisable to pare and burn, but contented myself with fallowing it for turnips, with which it was sown last summer; but from the indifference of the season, the crop did not prove abundant, yet as much so as I had any reason to expect; and I have no doubt, by proper management of it, though by far the worst of the waste, it will shortly become very useful land, and produce in succession good turnips, barley, and seeds. Part fallowed for turnips.

On a review of the foregoing statement, it will appear that the expense attending the improvement of the waste has been great; but it will be recollected, that the quantity of land reclaimed is very considerable, the greatest part of which has been drained and grubbed, and the face of it entirely changed; that on the comparison I now submit, I feel great satisfaction in being enabled to assert, in the judgment of able men, that at the time I made the purchase, the waste land was not worth more than 5*s*. per acre per annum on an average, which amounts to £16 7*s*. 6*d*. and that it is now worth and let as follows: General statement of expense & profit.

	£.	s.	d.
Sixteen acres of water meadow, £3 per acre -	48	0	0
Fourteen ditto will shortly be as valuable - -	42	0	0
Twenty ditto of reclaimed bog, £2 per acre -	40	0	0
Fifteen and a half ditto lighter peat, £1 per acre	15	10	0
By the year - - - - - - - - -	£145	10	0

I have not pointed out minutely every step that has been taken to drain, to irrigate, or improve the said waste lands, because the subject is generally so well understood; but I trust I have stated sufficient to prove, that the soil, thus reclaimed, is turned to a great and lasting benefit.

J. BUTLER.

VI.

VI.

Some Observations on an Insect that destroys the Wheat, supposed to be the Wireworm. By THOMAS WALFORD, *Esq. F. A. S. & L. S. With an additional Note, by* THOMAS MARSHAM, *Esq. Treas. L. S.* *

The wireworm not known.

THE insect which is the subject of the following memoir has never, I believe, been noticed or described by any entomologist or agriculturist; its depredations are the annual topic of conversation with the latter, yet few know what insect it is, that destroys the wheat in the months of October and November, under the denomination of the wireworm. Many suppose it to be a *scolopendra*, others a species of *iulus*, and some the larva of a *tipula*, or of the *scarabæus melolontha* of Linnæus. I supposed it to be one of the above, till I found two insects in the very act of destroying the wheat, as represented in the annexed figure (Pl. IV. fig. 3, *a*.). These I believe to be the insects commonly, although very improperly, called the wireworms in Essex and Suffolk: they appear to me *larvæ* of one of the coleopterous tribe; but to what genus they belong can at present only be conjectured. The projecting jaws somewhat resemble those of a *lucanus*. The two jointed bristles, and the cylindrical tail, give it an affinity to *staphylinus*; but the *larva* of this insect is supposed to be carnivorous, and not graminivorous. I fear, therefore, that the genus of this insect cannot be determined, till it is traced to its perfect state.

Larvæ of a coleopterous insect.

I shall now proceed to relate the discovery of the insect, and to detail the injury supposed to be done by it.

Discovery of the insect.

In October 1802, having occasion to call upon an agriculturist †, whose skill and judgment in farming are rarely equalled, he informed me, that his green wheat was dying and losing plant very much, the reason of which he could not comprehend. I immediately suspected, that it was occasioned by the wireworm; but what kind of insect it was,

* Trans. of the Linnean Society, vol. IX, p. 156.

† Mr. Thomas Olley, of Stoke next Clare, in Suffolk.

I could

I could not inform him. I therefore requested, that he would accompany me to the field where the greatest injury was done, in order that we might examine into it. This we accordingly did; and we were successful in discovering three of the insects in question, of which two were in the act of destroying the wheat, as above mentioned. With their projecting jaws these insects cut round the outside grass about an inch below the surface of the soil, to get at the young white shoot in the centre, which they eat: upon this, vegetation is immediately ftopped, and the plant dies. I suspect, that they first eat the flour in the grains which has not been drawn up by vegetation; for, when we touched them, they ran into the husks; and two of the three insects I carried home in the husks, which appear to be their habitations, and probably the place where they change from the *larva* to their present ftate.

Its manner of destroying wheat.

The injury which the public sustains by the ravages of these insects may, in some measure, be calculated from Mr. Olley's loss in 1802: he sowed fifty acres of a clay soil with wheat; out of these ten were destroyed by them, which were replanted by dibbling in one bushel of seed per acre. The price of wheat at that time was eight shillings per bushel.

Great injury done by it.

We here observe one fifth part of the quantity sown destroyed by thefe noxious insects, but the depredations of the wireworm, as I am informed by a friend* whose experience and observation enable him to calculate with superior judgment, being principally confined to wheat sown upon clover leys, old pastures recently broken up, pea and bean stubbles, &c., we may suppose the general average of the injury to amount to much less than a fifth (Mr. Olley's loss): a twentieth part of what is sown upon this description of lands will, I think, be deemed a very fair and moderate calculation. The number of cultivated acres of land in England at the time above mentioned was computed at seven millions, of which 2,400,000 were calculated to be sown with wheat; and as only one half of the wheat annually sown is supposed to be upon clover leys, old pas-

Calculation of annual loss to the kingdom from it.

* Allen Taylor, Esq. Wimbish-hall, Essex.

tures

tures, &c., our calculations must be confined to 1,200,000 acres instead of 2,400,000: this will give 60,000 acres as annually destroyed by the insect in question; which re-planted, at one bushel per acre, will require 60,000 bushels of seed, which, at eight shillings per bushel, are worth £.24,000. Beside this, although no extra expence is incurred by the farmer in preparing the land, yet he has to pay for dibbling in the seed, which, at five shillings and threepence per acre, will cost £.15,750, or, at the full price, six shillings per acre, £.18,000. If the land requires harrowing, there will be a further charge of ninepence per acre, or £2,250, not to name other items, which render it difficult precisely to ascertain the loss of the farmer.

If the above calculation be thought a fair one, and I see no reason why it should not, we find the quantity of wheat lessened to the market by the depredations of these insects is very frequently, if not annually, sixty thousand bushels; which occasions to the farmers an additional expense of at least £.15,750.

Means of preventing the injury to be sought after.

I hope these observations will prove a spur to gentlemen more conversant in entomology and agriculture than myself, to excite them to inquire into this subject, the result of which must ultimately be beneficial to the public at large, by discovering some means of preventing the injury done by these mischievous insects. At present we know of no other than early ploughing, which is not always convenient to the farmer, as he wants to feed his clover land as late as the season will admit of. Unslacked lime has been tried without success*; although it is well known, if laid thick upon the land and ploughed in immediately, it will destroy insects of every kind, that are in the soil; but in many places the expense of procuring lime is too great to think of using it in sufficient quantities to answer the intended purpose†.

Early ploughing not always convenient.

Lime ineffectual.

As

* Farmer's Magazine, page 450.

Grub of the tipula.

† I am aware of its being said that part of the injury sustained is done by the grub of the tipula or crane-fly; but I beg leave to observe, that the injury done by the grub is in the spring, and not in October;

as

As the drawing is from the accurate pencil of Mr. Sowerby, no description of the insect is necessary.

Explanation of the Figures.

Plate IV. Fig. 1. The insect, natural size.
2. The same, magnified.
3. *a.* The same, destroying the wheat.
— *b.* Hole in the husk, into which the insect ran upon being disturbed.

Additional Note, by Mr. Marsham.

True wireworm, in Sweden rootworm, a larva of a species of springing beetle, or skipper.

The above described larva is quite new to me, nor can I find any thing like it in the various authors I have consulted, who have written on the larvæ of insects. I am therefore ignorant to which order it belongs. The name of wireworm seems to be given to various species of larvæ, but what I consider to be the true wireworm was sent to me some time ago by the right honourable Sir Joseph Banks. A figure of this I have added to the plate (Pl. IV, Fig. 4.). The history of this animal I found fully detailed in the Stockholm Transactions for the year 1777, by Mr. Clas Bierkander, vicar of Gothene, near Skarra, under the appellation of root-worm. This larva, when full grown, is about seven lines long, very narrow, of a yellow colour, shining, and very hard: the head is brown, with the extremities of the jaws black. The body is composed of twelve joints, on the last of which are two black indented specks. It has six scaly feet on the fore part of the body. Mr. Bierkander observes, that it remains five years in this state before it changes into a pupa, whence issues *elater segetis* of Linnæus. I have frequently found it both in fields and

as many of the flies have not deposited their eggs till the latter end of September, and those that are deposited earlier are few of them hatched before the spring, as was proved by Mr. Strickney, whose pamphlet, entitled "*Observations respecting the Grub,*" is now before me: therefore the depredations of the grub cannot be greatly prior to that time: besides, they are most plentiful in the fly state at the end of September and beginning of October.

garden;

gardens at the roots of divers plants, but never succeeded in bringing it to perfection. The author above mentioned describes four other species of root-worms; viz. *musca segetis*, *musca hordei*, *phalæna turca*, and *tipula oleracea*.

I flatter myself, that this valuable Essay of Mr. Walford's will stimulate other gentlemen who reside in the country, and who are so materially interested, to enter seriously into a minute examination of the various causes, by which grain is so frequently destroyed; so that, by a number of such inquiries and communications, we may at length be enabled to point out a remedy—as every grain of corn that can be preserved in times like the present must be a public benefit.

Mr. Bierkander's papers on the different root-worms I got translated by a friend; and the translation, with some remarks of my own, was some time since presented to the Board of Agriculture.

THOMAS MARSHAM.

VII.

An Account of the larger and lesser Species of Horse-shoe Bats, proving them to be distinct; together with a Description of Vespertilio Barbastellus, taken in the South of Devonshire. By George Montagu, *Esq. F.L.S.**

Supposed two varieties of the horse-shoe bat.

MOST naturalists have conceived an opinion, that there are two varieties of the Horse-shoe bat, *vespertilio ferrum-equinum* distinguished only by their size; as such, Gmelin quotes the *major* and *minor* of Schreber.

Larger described.

The larger species only has hitherto been noticed in England. This was originally discovered by Doctor Latham, who communicated it to Mr. Pennant, and he first made it public in his *British Zoology*, where he states it to be found in the salt-petre houses belonging to the powder mills at Dartford, frequenting those places in the evening for the sake of gnats; and also observed during winter in a

* Linnean Trans. vol. IX, p. 162.

torpid

torpid state, clinging to the roof. It is described thus: "The length from the nose to the tip of the tail is three inches and a half: the extent fourteen. At the end of the nose is an upright membrane in the form of a horse-shoe. Ears large, broad at their base, inclining backwards, but want the little or internal ear. The colour of the upper part of the body is a deep cinereous; of the lower whitish."

Doctor Shaw, in his *General Zoology*, has nearly followed Mr. Pennant, but adds, "There is said to be a greater and smaller variety; perhaps the male and female: the greater is above three inches and a half long from the nose to the tip of the tail: the extent of the wings above fourteen."

With respect to the smaller horse-shoe bat, nothing more appears to be known than that it is inferior in size, but in other respects similar; from which may be inferred, that it is very little known, and it has not, to my knowledge, been recorded as indigenous to England. It is therefore with no small degree of satisfaction I have to announce, that it is by no means uncommon in particular situations; and I have the pleasure of congratulating the zoologist, that fortunate circumstances have enabled me to put the long unsettled opinion with respect to these two bats beyond all possible doubt; having lately taken a considerable number of both species, in each of which the sexual distinction was evident. But to render the subject more clear and incontrovertible, I shall proceed, by giving a description of the lesser species, and endeavour clearly to define the characteristic distinction between these two very analogous animals. In order, however, to prevent future confusion, I propose that the least of these should be called *vespertilio minutus*, leaving the other in full possesion of the original Linnæan trivial name of *ferrum-equinum*.

Smaller.

A distinct species.

Vespertilio minutus,

Length scarcely two inches and three quarters from the tip of the nose to the end of the tail, of which the latter is full three fourths of an inch: extent of the wings nine inches

Described.

ches and a half: weight from one dram three grains, to one dram twenty grains.

The colour above is pale rufous brown, most rufous on the upper part of the head: the nose is surrounded on the top with a broad membrane somewhat in form of a horse-shoe; within this is a smaller, in which the nostrils are placed; between these are two other small membranes standing a little obliquely, and appearing as valves to the nostrils; behind these stands a much more elevated longitudinal membrane; and further back is another transversely placed, of a pyramidal shape, standing erect behind the eyes; these last are covered slightly with hair, and some long bristles: round the upper lip under the exterior membrane of the nose is a row of minute tubercles, each furnished with a small bristle, equally well calculated to guide the lesser winged insects to the mouth, as the *vibrissæ pectinatæ* observed in several species of birds: the eyes are very small, black, and hidden in the fur: the ears large, pointed, and turned a little back at their tips; their base almost surrounds the opening, but at the outer part in each is a notch, which admits of the fore part of the ear closing within the other as a substitute for a valve so common in most other species, but of which this is destitute.

Found in Wiltshire.

It is now many years since I first noticed this species of bat in Wiltshire; once, in particular, I recollect to have seen a great many taken in the winter over the hollow of a baker's oven, having got in through a small external fissure. In the year 1804, about the latter end of the month of May, I observed several in an old building at the verge of a wood at Lackham, in the same county, erected for the shelter of cattle. In this shaded dark abode, surrounded by lofty oaks, it is not unusual to see several adhering to the plastered roof by their hind claws; and when approached, generally crawling a little to one side, and showing signs of uneasiness by moving their heads about in various directions, but not seeming inclined to take flight, till they have been repeatedly disturbed.

At this time I had not been fortunate enough to discover the haunt of *vespertilio ferrum-equinum*; but my wishes have since been amply gratified, by taking nine of the

v. ferrum-

v. ferrum-equinum, and seven of the *minutus*, many of which were conveyed home alive: of the former there were four males and five females; of the latter five males and two females. Of the *v. ferrum-equinum* the largest and smallest were both females, one preponderating four drams and a half, the other not exceeding four drams. The length of these to the setting on of the tail two inches and a half; to the end of the tail three inches and three quarters: the expansion of the wings about fourteen inches and a half. The two species differ in size,

In colour these two species are perfectly similar, except in some instances the sides and breast of the *v. ferrum-equinum* are more of a ferruginous-brown. scarcely in colour,

With respect to the face, which is so extremely curious, there appears on a cursory view scarcely a perceptible difference, except that the upper lip of the *v. ferrum-equinum* is much more tumid; but the most material distinction is in the formation of the nasal membranes, especially that which is posterior and transverse. To explain this no words can convey what a simple outline will, and therefore the curious are referred to Pl. IV, *fig.* 5, which represents the side view of the membranes of *v. ferrum-equinum*, of which *a* is the posterior tranverse one; the front is seen at *fig.* 6. The same views are given of the nasal membrane of *v. minutus* at *fig.* 7 and 8, where *b b* represent the membranes in different points of view. In these a very striking difference is observable, and it will also be perceived, that the anterior longitudinal membrane is by no means similar in both species. but chiefly in the nasal membranes.

With respect to the teeth, it will be observed, that the *v. ferrum-equinum* possesses two minute distant fore teeth in the upper jaw, which are not to be found in the *v. minutus*; a circumstance that seems to have escaped most naturalists, this genus being usually placed in the division destitute of upper fore teeth: the canine teeth are also much stronger in proportion in *v. ferrum-equinum* than in the other species. Teeth.

Linnæus, when he placed the bats in the first order of *mammalia*, doubtless considered the whole genus to agree in possessing two pectoral teats, and no others; and this opinion Linnæus placed the bats in the first order of mammalia,

nion seems to have been confirmed by succeeding naturalists as far as treading in the path of so great a physiologist may be considered as a proof of the fact. It must, however, be acknowledged, that we should do well, if, at the same time we admire the wisdom and consummate skill of others, we were to recollect, that circumstances do not always concur to throw all the light upon a subject that might be desired, and that the wisest and most skilful philosopher is not proof against mortal fallibility.

Those who are in the habit of searching minutely into the secrets of nature well know how necessary it is to be cautious in admitting of general rules.

That the appearance of two pectoral teats in the bat genus, without any others contiguous, should lead to a conviction, that they were the only papillæ such animals possessed, may easily be conceived; but chance frequently develops what the most scrutinizing eye has sought for in vain.

but the less horse-shoe bat has two abdominal papillæ.

While I was searching for some curious insects, which were observed to move with unusual celerity amongst the fur of these bats*, the pectoral papillæ of one of the *v. minutus* were very conspicuous by the space round them being bare, as if the animal had recently suckled its young; and to my utter astonishment, on turning the fur over in every direction, I discovered two other teats very near together, situate on the lowest part of the abdomen, close to the *pubis*. It may readily be imagined, that so unexpected a discovery scarcely admitted the senses to determine the validity of ocular demonstration: the aid, however, of glasses left no doubt of the fact, and a scientific friend confirmed my opinion. At the moment of this discovery I had embowelled all the specimens of *v. ferrum-equinum*, and consequently cannot determine whether they are similarly formed or not; nor have I since procured a female bat of any other species to examine, so that it yet remains to be ascertained, whether this structure is peculiar to one or more species, or that the two abdominal papillæ are really essential to the generic character of these animals,

Whether this be a character of the genus, or peculiar to a species, not yet ascertained.

* *Celeripes vespertilionis*, a newly discovered insect.

but

but hitherto overlooked, by being so far removed from the others. On future observation must depend the place to which the bats should be properly consigned in the systematic arrangement of quadrupeds. If some species only are found to possess four papillæ, it would be a very considerable violence to nature to divide them on this account: and yet to retain them undivided in the order of *primates*, according to the Linnæan definition, would be inconsistent: but on this part of the subject there is no necessity of enlarging until we become more enlightened.

It is probable the papillæ of all the smaller bats are so contracted, except at the time of administering nourishment to their young, that they are not discoverable with the utmost attention, for even in the *v. ferrum-equinum* no pectoral teats were to be discovered, although the sexual distinction was sufficiently evident. But this very contracted state of these parts, when nature has no demand for the use assigned to them, is not peculiar to these volant quadrupeds, since we find the same difficulty in discovering them in mice.

Teats not easily discoverable except when suckling.

These bats were taken in a large cavern near Torquay in Devonshire, commonly known by the appellation of Kents-hole, and where both species are usually observed in considerable abundance clinging to the vaulted roof of the interior apartments. This vast cavern was explored with a view to obtain whatever species of *vespertilio* might inhabit it, and with expectation of procuring specimens of *v. barbastellus*, and possibly some new species, having been informed the cave abounded in number and variety. Strange, however, as it may appear, not a single instance occurred of any other species becoming an inhabitant of this dark and frightful region.

The two species found in the same place, without any other.

It should therefore appear, that these two bats are as congenial in their animal temperature, as they are similar in habit; and that in constitution they essentially differ from all the other British species.

It is well known, that all places impervious to light, and destitute of a free circulation of air, can neither be suddenly heated nor suddenly cooled by the changes of atmospheric temperature, and that the vicissitudes of such a climate

Resort to caverns from aversion to any change of temperature.

mate are extremely small: thus these species from instinct seek those dark and dreary abodes, and wholly retire from the face of day, their feelings being repugnant to the benign influence of the solar rays, which vivifies and reanimates all nature besides.

Others only shun the extremes.

The *v. noctula, murinus, auritus,* and probably *barbastellus,* whose constitutions appear more robust, do not retire into total darkness, nor wholly remove from the vicissitudes of the surrounding atmosphere; but, being formed by nature to bear a greater degree of either heat or cold, content themselves with such a hybernaculum as is sufficient to protect them equally from the extremes of one or the other. Thus we find these in the fissures of old buildings, in towers, under the eaves of houses and churches, and in the hollows of trees, and not unfrequently congregated; but they seldom or never enter those gloomy regions, which nature has consigned to the others as an exclusive right of inheritance.

The bat superior to most birds in powers of flight.

Contemplating the frolics and evolutions of these little creatures in our summer evenings perambulations must bring to recollection the extraordinary opinion of some philosophers, who scarcely admit their progressive motion to be an act of flying. How little can such have atteutively observed their sudden and rapid turns in pursuit of flies! It might be fairly asked, How much inferior are the aërial excursions of a bat to that of a swallow, one of the most powerful on wing of the feathered tribe? and might we not pronounce, without risk of refutation, that a bat far surpasses the greater part of birds in its powers of flight?

Supposed not to require vision.

If we are to give the utmost credit to the experiments of Spallanzani and Mr. de Jurine, the conclusion would be, that vision is not of any apparent use to these animals, since they fly about with as much ease, and equally avoid obstacles, when their eyes are covered, or even put out, as they do previous to this operation. That their eyes, being minutely small, are not calculated to admit many rays of light, as in most nocturnal birds, must be allowed, but then they have no occasion to distinguish their prey at a distance. If it be denied, that their eyes are of any use in the

in the discerning of objects against which they might strike, surely they must be equally useless in discovering the smaller winged insects, on which they prey in the dusk of the evening.

Can we, however, meditate on the wonderfully rapid turns and evolutions of these creatures in pursuit of their prey, and not allow them the powers of sight to effect the first principle of life, a power not denied to any known animal possessed of a red circulating fluid by the arterial system? To assent to the conclusion which Mr. de Jurine has drawn from his experiments, that the ears of bats are more essential to their discovering objects than their eyes, requires more faith, and less philosophic reasoning, than can be expected of the zootomical philosopher, by whom it might fairly be asked, Since bats see with their ears, do they hear with their eyes? It will not be sufficient for these experimentalists to inform us, that the copious auricles of this class of animals, or their delicate internal structure, are adequate to the double purpose of seeing and hearing, when we perceive, that they are by nature provided with organs of sight similar to what we not only feel most sensibly to be the most inestimable of blessings, but also perceive to be the principal fountains of locomotion in all other animals in the same scale of beings.

But this is improbable.

Although it cannot be admitted, that the Almighty hand gave to these creatures those most wonderfully constructed organs of sight, without endowing them with visual properties, yet it must be allowed, that there is something extremely astonishing and unaccountable in their unembarrassed flight in total darkness, whether by sealing up their eyes, or by their natural habits of finding their way through all the smaller passages and windings into the inmost recesses of their subterraneous abode. By what occult property they direct their course in total darkness, is perhaps a problem of as difficult solution as that of a swallow returning from the torrid to the frigid zone, to breed in the same nest it had prepared the preceding year, and in which it had performed those functions of nature. Can any human understanding develop the cause, that so unerringly directs the carrier-pigeon to its place of nativity, when previously

Its directing its flight in darkness unaccountable,

but no more than other facts in natural history.

 taken

taken to the distance of five hundred miles? How is the bee instructed to find its hive when captured and taken to a distance? This is inexplicable, and yet no one will dispute the fact. Indeed the practice is common in some countries, in order to find the wild hives; for if two bees are taken near the same spot, and turned out at different points, distant from each other a few hundred yards, if belonging to the same hive, the two lines formed by the direction of their flight will discover the hive to be at the intersection of those lines. These are the mysteries of nature, so impenetrable to the human mind, that we are lost in a labyrinth of wonder at such instinctive endowments, which are incomprehensible to our limited faculties. We have only attentively to examine the operations of nature, and we shall find a thousand instances not less astonishing, than that the bat should find its road without one single ray of light to direct its course*.

Mode of finding hives of wild bees.

VESPERTILIO BARBASTELLUS.

Gmel. Syst. i. *p.* 48. *Buffon.* viii. *p.* 130. *t.* 19. *f.* 1.
Pennant Quadr. ii. *p.* 561. *Shaw Zool.* i. *p.* 133.
Brit. Miscellany, *t.* v.

V. barbastellus found in England.

This species has long been known to be an inhabitant of some parts of the European continent, especially France, but, I believe, had not been discovered to inhabit England till the year 1800, when I first noticed it to be indigenous to the south of Devon, and had prepared an account of it for the Linnean Society. Since that period others have occurred in the same county; and we are informed in the *British Miscellany*, that it has been taken in the powder-mill at Dartford in Kent.

The figure and description given in that work are highly satisfactory; but as it is a newly discovered quadruped in this

Teats not perceptible.

* Since the preceding account was written, several of both these species of bats have been collected from the same cavern, and in one of the *v. minutus* the abdominal papillæ were more conspicuous than in the former; but not the least vestige of such could be found in the *v. Ferrum-equinum*: it should, however, be remarked, that in these the pectoral teats were equally invisible.

this island, and of course little known, it may not be uninteresting to give some additional description of it from specimens in my possession, and to make such further remarks as may conduce to its natural history.

The first I obtained was taken on wing in the village of Milton, which is situate near the coast, and, I believe, was a female. Described.

The colour of this is a dusk-black, intermixed with a few gray-brown hairs towards the rump: the membranes of the wings and tail dusky.

On the 17th of August 1805, I procured a male specimen alive; it was found adhering to a small tree near Kingsbridge.

The length is nearly four inches, of which the tail measures one inch seven eighths; the extent of the wings about eleven inches: weight exactly one hundred grains.

The colour differed a little from that of the former, especially in having the middle of the back and the breast mixed with silver gray hairs; the lower belly, thighs, and behind the vent on the tail membrane more gray. The nose is rounded in front, flat, and cavernous on the top, in which part the nostrils are placed: ears large and black, furnished with a linear valve, and unusually broad at the base, extending forwards, and meeting over the nose, so as to cover the forehead: eyes very small, seated within the membrane of the ear: the teeth numerous in both jaws, and much jagged; in the upper, four cutting teeth, but no canine, and a vacant space between those and the grinders: in the lower jaw six cutting teeth and four canine or longer teeth, and between these last on each side is a small intermediate one; these longer teeth fall into the vacant space in the upper jaw.

Buffon appears to be the first naturalist who recorded this species, and his account of it has been copied by succeeding writers.

It seems to partake of the habits of the common bat; but it may readily be distinguished from *vespertilio murinus*, even on the wing, in the earlier part of the evening, by its superior size, and in being by far the darkest in colour of all the British bats. Upon comparison, the flattened nose, more pointed ears, and particularly the base of Its difference from the common bat.

 these

these coming so forward on the forehead as scarcely to leave any space between, will be found essential characters of distinction.

I have not been able to discover the hybernaculum of this species, but it is reasonable to believe its torpid state is passed in similar situations to those in which all but the *v. ferrum-equinum* and *v. minutus* retire during the colder months; none of which appear to be subterraneous.

VIII.

An Account of the Method of hastening the Maturation of Grapes. By JOHN WILLIAMS *Esq., in a Letter to the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S. &c.**

SIR,

Grapes do not always ripen well in this climate.

IT is a fact well known to gardeners, that *vines*, when exposed in this climate to the open air, although trained to walls with southern aspects, and having every advantage of judicious culture, yet in the ordinary course of our seasons ripen their fruit with difficulty. This remark, however, though true in general, admits of some exceptions, for I have occasionally seen trees of the common *white muscadine*, and *black cluster grapes*, that have matured their fruit very well, and earlier by a fortnight or three weeks, than others of the same kinds, and apparently possessing similar advantages of soil and aspect.

Earliest on old trees with long trunks,

The *vines* that ripened the fruit thus early, I have generally remarked, were old trees having trunks eight or ten feet high, before their bearing branches commenced. It occurred to me, that this disposition to ripen early might be occasioned by the dryness and rigidity of the vessels of the old trunk obstructing the circulation of that portion of the sap, which is supposed to descend from the leaf. And to prove whether or not my conjectures were correct, I made incisions through the bark on the trunks of several vines growing in my garden, removing a circle of bark from each

from the circulation being obstructed.

Incisions through the bark, leaving the alburnum naked,

* Horticultural Society, vol. I. p. 107.

each, and thus leaving the naked alburnum above an inch in width completely exposed; this was done in the months of *June* and *July*. The following autumn the fruit growing on these trees came to great perfection, having ripened from a fortnight to three weeks earlier than usual: but in the succeeding spring, the vines did not shoot with their accustomed vigour, and I found that I had injured them by exposing the alburnum unnecessarily.

occasioned the fruit to ripen early.

Last summer these experiments were repeated; at the end of *July* and beginning of *August*, I took annular excisions of bark from the trunks of several of my vines, and that the exposed alburnum might be again covered with new bark by the end of autumn, the removed circles were made rather less than a quarter of an inch in width. Two vines of the *white Frontiniac*, in similiar states of growth, being trained near to each other on a south wall, were selected for trial; one of these was experimented on (if I may use the term), the other was left in its natural state, to form a standard of comparison. When the circle of bark had been removed about a fortnight, the berries on the experimented tree began evidently to swell faster than those on the other, and by the beginning of *September* showed indications of approaching ripeness, while the fruit of the unexperimented tree continued green and small. In the beginning of *October* the fruit on the tree that had the bark removed from it, was quite ripe, the other only just began to show a disposition to ripen, for the bunches were shortly afterwards destroyed by the autumnal frosts. In every case in which circles of bark were removed, I invariably found that the fruit not only ripened earlier, but the berries were considerably larger than usual, and more highly flavoured.

The experiment repeated,

the fruit ripened earlier, and improved in size and flavour.

The effects thus produced I can account for only by adopting Mr. Knight's theory of the downward circulation of the sap, the truth of which these experiments, in my opinion, tend strongly to confirm. I therefore imagine by cutting through the cortex and liber without wounding the alburnum, that the descent of that portion of the sap which has undergone preparation in the leaf is obstructed and confined in the branches situate above the incision; consequently

Theory of the process.

sequently the fruit is better nourished and its maturation hastened. It is certainly a considerable point gained in the culture of the vine, to be able to bring the fruit to perfection, by a process so simple, and so easily performed. But lest there should be any misconception in the foregoing statement, I will briefly describe the exact method to be followed by any person, who may be desirous of trying this mode of ripening grapes. The best time for performing the operation on vines growing in the open air is towards the end of *July*, or beginning of *August*; and it is a material point, not to let the removed circle of bark be too wide: from one to two eighths of an inch will be a space of sufficient width; the exposed alburnum will then be covered again with new bark before the following winter, so that there will be no danger of injuring the future health of the tree.

Proper time of performing the operation.

It is not of much consequence in what part of the tree the incision is made, but in case the trunk is very large, I should then recommend, that the circles be made in the smaller branches.

Caution.

It is to be observed, that all shoots which come out from the root of the vine, or from the front of the trunk situate *below* the incision, must be removed as often as they appear, unless bearing wood is particularly wanted to fill up the lower part of the wall, in which case one or two shoots may be left.

Applicable to vines in forcing houses,

Vines growing in forcing houses are equally improved in point of size and flavour, as well as made to ripen earlier by taking away circles of bark: the time for doing this is when the fruit is set, and the berries are about the size of small shot. The removed circles may here be made wider than on vines growing in the open air, as the bark is sooner renewed in forcing houses, owing to the warmth and moisture in those places. Half an inch will not be too great a width to take off in a circle from a vigorous growing vine, but I do not recommend the operation to be performed at all in weak trees.

and perhaps other fruits, particularly figs.

I think that this practice may be extended to other fruits, so as to hasten their maturity, especially *figs*, in which there is a most abundant flow of returning sap; and it demonstrates

strates to us, why old trees are more disposed to bear fruit than young ones. Miller informs us, that the vineyards in *Italy* are thought to improve every year by age, till they are 50 years old. It therefore appears to me, that nature, in the course of time, produces effects similar to what I have above recommended to be done by art. For, as trees become old, the returning vessels do not convey the sap into the roots, with the same facility they did when young: thus by occasionally removing circles of bark, we only anticipate the process of nature*; in both cases a stagnation of the true sap is obtained in the fruiting branches, and the redundant nutriment then passes into the fruit.

I have sometimes found, that, after the circle of bark has been removed, *a small portion of the inner bark has adhered to the alburnum*: it is of the utmost importance to remove this, though ever so small, otherwise in a very short space of time the communication is again established with the root, and little or no effect produced. Therefore in about ten days after the first operation has been performed, I generally look at the part from whence the bark was removed, and separate any small portion, which may have escaped the knife the first time. No portion of the inner bark must be suffered to remain.

I am, Sir,

Your obedient humble servant,

JOHN WILLIAMS.

Pitmaston, Worcestershire,
20th April, 1808.

* Hence we may infer, that trees thus treated will have their decay accelerated, and their natural duration shortened. C.

IX.

IX.

An Essay on Manures. By ARTHUR YOUNG, *Esq. F.R.S.**

Arrangement of the subject.

MR. Young first arranges the treatment of his subject in the following order. 1. The nature of the manure. 2. Its properties. 3. Collecting. 4. Preparation. 5. State in which applied. 6. Application. 7. Season when applied. 8. Quantity. 9. On what soil.

He next classes manures in two divisions. 1. Such as are made or dug on a farm. 2. Such as are usually purchased. The latter he subdivides into animal, vegetable, and fossil. In the first division comes

1. *Marle.*

Marle.

The marles most common in England are clay, stone, and shell marle. Some distinguish them by their colours, as white, red, blue, black, &c.; but the colour deserves no attention except as indicative of iron.

Its nature.

They are usually composed of sand, clay, and calcareous earth. The red and black have a small quantity of iron. A marle from Cheshire had 1·7 per cent. Even in the whitest prussiate of potash will almost always detect some iron. The calcareous earth varies from 25 to 80 per cent. One of the best clay marles contained 40 calcareous earth, 50 clay, 8 or 10 sand, and clear signs of some iron. It falls in pure water, and by exposure to the air. The clay contains generally a small portion of iron, a little volatile alkali, and some sulphuric acid; and even when deprived of all

* Abridged from the Bath Society's Papers, vol. X, p. 97. This essay was written in consequence of the following subject being announced for a prize, which it obtained. "The Bedfordean gold medal will be presented to the author, who, at or before the first meeting in November 1804, shall produce to the Society the best essay, founded on practical experience, on the nature and properties of manures, and the mode of preparing and applying them to various soils: in which essay shall be pointed out the cheapest manner of collecting and preparing the different kinds of manures, and the state, season, and quantity, in which they should be applied."

all organic matter yields hidrogen gas. Phosphorus may be gained from all calcareous earths.

What renders it particularly valuable is the calcareous earth it contains. But we do not yet know what ought to be the quantity of calcareous earth in a soil. The best specimen analyzed by Giobert had 6 per cent; by Bergman, 30; by Dr. Fordyce, 2; and a rich soil quoted by Mr. Davy had 11. This is an inquiry, concerning which the author has made many experiments, and on soils of the most extraordinary fertility. In one he found 9 per cent; in another 20; in another 3; and in a specimen of famous land, procured from Flanders, 17. Many poor soils however possess nearly the same proportion as the most fertile: and on comparing every circumstance he is disposed to conclude, that the necessity of a large proportion of calcareous earth depends on the deficiency of that organic matter, which is convertible into hidrogen gas. If the farmer find by experiment, that his soil contains but a small quantity of organic matter; or know by his practice, that it is poor, and not worth more than 10, 15, or 20*s.* an acre; it ought to have 20 per cent of calcareous earth in it. If on the contrary it abound with organic matter, and be worth in practice a much larger rent, it will not require marling, though it contains but 5 per cent of calcareous matter, or even less. Marles likewise give tenacity and firmness to a soil, and for this the clay marles are to be preferred. Some soils abound with acid particles, which are prejudicial; and these are neutralized by the calcareous earth. Properties.

The earth found in vegetables is for the greater part calcareous. Hence we may presume, that this earth should make a part of the soil. Lord Dundonald calculates, that all the calcareous earth to be obtained from the vegetable produce of an acre of most crops will not exceed eighty pounds: but if even this quantity be required for every crop, the necessity of occasional supply appears.

Marle is generally obtained by digging, but it is also dredged up from the beds of some rivers. White shell marle, and a very light white species, are found under bogs, and at the bottom of lakes. No person, whose land wants marle, where it is not generally known to exist, should be satisfied Collecting.

satisfied without the most careful examination by boreing. A borer for twenty feet depth does not cost above £3, for 80 feet not above £21, and is used without difficulty by any common workman.

Application. Marle requires no preparation. It is best applied on leys: and the longer it lies on them before it is ploughed in, the better. It should not be ploughed in too deep. The best way therefore is, to plough the ley shallow for pease. To turnips there is but one objection, the giving so much tillage so early after the improvement. Potatoes are mischievous for the first crop after land has been marled. Next to leys, fallows are the best to receive marle. When the farmer has a choice, on wet and heavy soils it should be summer work, and on dry ones it may be winter.

Season.

Quantity. The quantity employed is of great importance. From 120 to 150 cubical yards per acre being laid on a poor sand, the productiveness of the land has been injured for twenty years. Half this quantity would have done good. It is better to marle twice, than apply too much at once. On poor, loose, wet loams more may be used than on loose sands. On loose peat bogs, and on moors, the greater the quantity the greater the improvement. Where the object is to give calcareous earth, the quantity should be small, as from ten to twenty tuns.

Soil requiring marle. The defect of a soil must be understood, before a wise farmer will put himself to the expense of marling. Every day's experience will inform him, whether his land want tenacity and consolidation; but the want of an addition of calcareous earth as a food of plants can be discovered only by analysis. Other circumstances deserve attention. If the chrysanthemum segetum, corn marigold, rumex acetosella, sheep's sorrel, or polygonum pennsylvanicum, abound, the experienced farmer will pronounce, that the land wants marling. Turnips producing deformed strings of roots, without swelling into the proper globular form; or being subject to the well known distemper of the anbury; both afford a proof of too much looseness of texture, and suggest consolidation by clay marle, after which these evils vanish. The erica vulgaris, common heath, or ling, is generally a proof of an acid soil; and all peat soils are found on

on analysis to contain a considerable quantity of the gallic acid. Some have been rendered quite sterile by acids. A stratum of moss in Scotland was so impregnated with vitriolic acid, that from four pounds of it one pound of green vitriol was extracted. In a bog in Bedfordshire sulphate of iron abounded in almost equally extraordinary degree; yet it has been converted into one of the finest water meadows in England by his grace the late Duke of Bedford. Wherever such soils are found, marle is sure to have great effect from its calcareous earth. For wet but loose loams, which when manured are more productive of straw than corn, clay marle is a cure, and attended with unquestionable profit. Another quality of these loams is that of being uncommonly pestered with the red worm; and it is a singular quality of marle, to lessen this evil considerably. Whatever gives them a firmer texture has a tendency to this effect.

2. *Chalk.*

Chalk.

Chalk in its properties nearly resembles marle, but it contains a much larger proportion of calcareous earth. It renders tenacious clay more dry and friable, which stone marle alone will not. It is also more common to chalk grass lands than to marle them; and it works a capital improvement on low, coarse, sour meadows, rendering them firmer, and improving the sweetness of the herbage. Properties.

It is commonly dug from pits like marle: but the general practice of Hertfordshire is to sink shafts for it. The chalk-drawers travel in gangs; chamber the shaft all round, leaving columns to support the incumbent earth; and draw up the chalk in buckets. They will wheel it on to the land for 8*d.* the load of twenty-four bushels to the distance of twenty poles from the shaft. Method of procuring it.

It is generally used in much smaller quantities than marle. In Essex, whither it is brought by sea from the Kentish coast, from five to eight waggon loads per acre are attended with more remarkable effect than even dung itself, if the land have not been chalked before. More than forty cubical yards are seldom spread on an acre. Quantity used.

The most remarkable effects attending it appear to be Effects.

upon

upon good sound loams, worth from 15s. to 20s. an acre. Six or seven waggon loads per acre are seen immediately in the crops, and to an inch. Chalk presently gives the land a reddish colour, so that the part of a fallow which has been chalked will be discernible at a distance from this tinge. A singular circumstance observed in Essex is its being an enemy to what their farmers call grazing, or running to turf. A field, which before chalking will run of itself to a fine head of white clover, does so no longer after chalking. The chalk used there is not soft, but rather hard. The sharpest frosts leave many lumps unbroken, which must be done with pickaxes; and the hard bits, which break to a clear white, are better than those that crumble between the fingers. This is to be attributed to the nature of the soil, which is rather too stiff for turnips.

Where applicable.

Soils abounding spontaneously with sorrel are highly improvable by chalk. It is used successfully on all soils, on which marle is found to answer. It is not a general favourite in Norfolk for poor sands, or even middling ones; but some farmers of considerable note for accuracy of observation have of late used very hard chalk, and with great success. On all moors, peat bogs, and peat fens, every species of calcareous earth may be applied with singularly good effect; and as chalk abounds more than marle in this earth, it is full as valuable on them, if not more so.

3. *Lime.*

Lime.

Every kind of calcareous stone, being in fact a carbonate of lime, may be converted into lime by expelling its carbonic acid and water by means of fire. In this state it is caustic, and has a strong power of reabsorbing moisture, and likewise carbonic acid, if exposed to the atmosphere. As limestones generally contain a portion of clay and sand, these will remain mixed with the calcareous earth in the lime. This is of little consequence, only diminishing the quantity of calcareous earth. But sometimes they have a mixture of magnesia, and this has been said to be detrimental to vegetation. Limestone that contains magnesia is generally of a brownish hue, or fawn colour; but none is found in a stone that breaks blue.

As lime after some months exposure is converted into chalk, it must have similar effects with regard to supplying calcareous earth: but it will not give tenacity to sand like marle, or friability to clay like chalk. When laid on in its caustic state, it destroys the spontaneous growth of soils: and this is a very valuable quality, where this growth is a nuisance. The truth of this observation is visible on limed moors. Its properties.

The most material distinction in the application of lime is that of spreading it fresh in its most caustic state, or keeping it till it is slacked, and has reabsorbed more or less carbonic acid. Application.

On all soils in a state of nature, and greatly abounding with undecayed vegetables, which are required to be speedily destroyed, it should be spread hot from the kiln, as it is termed; that is, in its most caustic state. In other cases it is slacked, before it is spread. Upon waste lands the causticity has an evident and necessary effect; but not on cultivated lands, which this quality of the substance while deprived of its carbonic acid would tend to prejudice rather than improve.

A truly practical husbandman of great experience, Mr. Craike, of Arbigland, gives directions for the application, which merit attention. "Let the whole quantity of lime, intended to be used on any given field of moderate size, be laid in one heap, where water can be had most conveniently. Let it be there thoroughly slacked; and immediately after it is cold, which it will be in a day or two, fill the carts, and spread the lime out of them with shovels equally over the surface. The more common method of laying it down in small heaps over the whole field, to slack by rain, is very erroneous. It is liable to get too much rain, which, in place of reducing it into a fine powder, converts it into a running mortar, in which state it will neither spread equally nor mix with the soil*." And for the same reason, Mr. Wight remarks, both the ground and the lime should be quite dry at the time of spreading. In Dumfriesshire, quick lime being compared with some that had lain in a heap for

* Trans. of the Dumf. Soc. No. II, p. 34.

several

several years in consequence of a lawsuit, the latter did much more good than the former.

Season. Where improvements are carrying on upon a large scale, and draw-kilns are kept at work throughout the year, the choice of season becomes of secondary importance: in other cases liming should no more go on in winter than building. It may be continued from March to October, but summer is the best season. It should be spread on a ley one full year before ploughing, that it may have time to fix itself firmly in the sward. If ploughed too soon it falls to the bottom of the furrow, and will be the sooner lost, for it continually sinks. Three years before breaking up a ley, part was limed with three hundred bushels an acre; the remainder was limed with an equal proportion only one year before it was broken up. The former produced oats 10 for 1 of the seed, the latter 6 for 1.

Quantity. In common cases the quantity ought to be guided by a chemical analysis of the soil. The largest quantities have been spread, and with propriety, on bogs and peat moors, and on mountains. The Bishop of Landaff speaks of a thousand bushels an acre on moors in Derbyshire applied with great success. Five or six hundred are not uncommon there. Lord Chief Baron Foster, in Ireland, went as far as to three hundred barrels, on a moory waste; and found, that the greater the quantity the greater was the improvement. Dr. Anderson tried from one to seven hundred bushels an acre, and found the good effect to increase regularly with the quantity. In more common cases the quantities vary in general practice from thirty-six to a hundred and sixty bushels.

Where applicable. On peat bogs, peat moors, and mountains, the utility of lime cannot be questioned. Experiments on every scale, and under a very great variety of circumstances, speak a uniform language: the benefit of applying lime is great and decided. On liming Kedgley moor, in Northumberland, covered with ling, the ling was killed, and three tuns an acre of white clover were mown without sowing any. Part of Meriden heath, in Warwickshire, was fallowed for a year, ten acres trebly folded with a thousand sheep, ten acres

acres well dressed with good rotten dung, ten acres well limed, and the whole sown with oats and seeds. The part folded had not a bag of oats an acre, and the seeds were not worth saving: that which was dunged succeeded very little better: while that which was limed produced a very excellent crop of oats and seeds.

In Glendale ward, Northumberland, the soil is naturally dry, duffy, light, full of fibrous roots, and, when in fallow, on passing over it you sink to the ankles. After it is sufficiently limed, the fibrous roots disappear, the soil becomes denser, firm to tread, retentive of moisture, and produces better and more abundant crops of grain: and, if laid to grass, white clover appears to an inch where the lime was spread. Even on a burning sand four chaldrons an acre have had a striking effect; but then the sand was covered with a mossy sward.

Lime does worst on a cold hungry clay. It cannot succeed, where in the farmer's language it has nothing to work upon; where water deprives it of its most material properties; or where frequent repetitons have given a full dose of calcareous earth, and consumed every vegetable particle. After paring and burning lime is at best useless, the vegetable fibres being already destroyed by fire.

Where calcareous manures are required, powdered limestone may be employed with excellent effect. Perhaps it may be questioned, whether limestone gravel be not the best of all manures for improving a peat bog. Limestone, & limestone gravel.

4. *Clay, Loam, and Sand.*

The effect of these depends on the deficiency of the soil. Clay is every where beneficial on sand: but sand is not equally so on clay, for many clays contain far more sand in their composition, than farmers are apt to suspect. Sandy loams are frequently considered as clays, because they are heavy for want of effectual draining. Clay, loam, & sand.

Sea-sand partakes of another class of manures. It contains muriate of soda; and if it be a shelly sand it is so far allied to shell marle. Sea sand.

5. *Burnt*

5. *Burnt Clay, Marle, and Earth.*

Burnt clay and earth.

In various parts of the United Kingdom it has been a practice to burn clay, and clay marles, in large heaps, and to spread the ashes as manure. The nature and properties of burnt earth must vary with the portion of it which is calcareous, as this is converted into lime by calcination. Burning clay breaks its cohesion entirely, and reduces it to a permanent state of friability, which does not permit it to combine with any other substance: the sulphuric acid, which most clays contain, is dissipated: the iron and the clay itself are oxigenated: and a faculty of generating nitre is given in some cases. In its burnt state also it has a power of combining with the salt of urine. Burnt clays, says Dr. Darwin, when strewed on the ground, may contribute to vegetation, by their parting with their oxigen in a fluid, not a gaseous form; which, united with carbon, or phosphorus, or nitrogen, might supply nutritious fluids to the roots of vegetables. Its texture is extremely beneficial in dividing and attenuating the harshness of stiff soils, and rendering them more absorbent. These circumstances are amply sufficient, to account for the benefit which many persons have derived from the practice of burning clay and marles. Mr. Leslie, in Ireland, made great exertions in this way: Mr. White Parsons, in Somersetshire, has burned the earth out of ditches and drains successfully: and Mr. Boys, in Kent, has been long in the habit of doing it, paying his men sixpence per load of ashes for digging and burning.

(To be continued in our next.)

X.

X.

On the Construction of Theatres. In a Letter from RICHARD LOVELL EDGEWORTH *Esq., F. R. S. and M. R. I. A.*

To Mr. NICHOLSON.

SIR, *Edgeworthstown, March* 6, 1809.

THE public, by the loss of two theatres in one winter, must be anxious about the plans on which those edifices are to be rebuilt: they will not be satisfied with the opinion of a single architect, they will require an open discussion of the principles, and plans upon which a new theatre is to be constructed; this they have a just right to demand, for their lives and properties are at stake. Every family in London might have mourned the loss of some relative, had the play-houses been filled at the time of the accident; and the whole city might have been burned to ashes by either of the conflagrations.

The building of a theatre an object of public concern.

We are to consider not only the loss of lives by the immediate disaster, but also the apprehensions, which the audience must feel for some time to come; and the anxiety, which those who remain at home must suffer during the absence of their friends at the theatre. Nothing should be left to embitter the cup of innocent pleasure, and "assurance should be made doubly sure," where great hazards are run, from no greater motive than the hope of an hour's amusement.

It cannot be too secure, as it should prevent even apprehension.

Covent-garden playhouse is now rebuilding without any previous appeal to the public, that I have heard of, as to the plan or precautions, that are to be followed in its construction. I know, that some hints were sent on these subjects, which were not even considered, at least not noticed, till after the plan was arranged. Surely it must be infinitely more advantageous to the proprietors and to the nation, that a short delay should take place before a plan is ultimately arranged, than that a new theatre should be opened ten days sooner, or ten days later.

The public should be called on for hints.

The glaring defect, or to speak more properly, the obvious blunder in the building of Drury-lane theatre, was the

Timber should not be intro-

duced as a frame-work.

introduction of timber as a frame work for bricks and stone; this is a fault common to buildings in London, where the public safety is without hesitation sacrificed to the interests of individuals.—But to construct a wooden theatre is an absurdity too gross, to pass without animadversion. A frame-work of timber, filled with cores of brick or stone, and cased perhaps with brick or plaster, is opened for the reception of the public, who are to run the risk of sudden destruction from a spark of fire, or a snuff of candle, from the fireworks and lightning of comedy and tragedy, of pantomime and farce, without any probable means of escape, or any security, except what a few hogsheads of water in a cistern on the top of the house can afford.—No future prologue at the opening of a new theatre could re-assure the audience upon this subject.

Time should be allowed for obtaining information from every quarter.

From a view of these considerations I hope it will appear incumbent upon those, who rebuild Drury-lane, to take time for receiving information from every quarter whence it may be expected: instead of hurrying forward to a beginning before they have well considered the end. A remarkable observation made by that great engineer Mr. Smeaton, in his account of the building of the Eddystone lighthouse, should never be forgotten by those who direct, or by those who undertake extensive public works.—" No resolution of " the proprietors," says he, " ever conduced more to ultimate success, than their leaving me at liberty *(as to time)*; " had they been of the same temper and disposition as by " far the greatest part of those who have employed me, " both before and since, their language would have been, " *Get on, get on, for God's sake get on*, the public is in " expectation, get us something speedily, *to show*, that we " may gain credit with the public."

Observations of Mr. Smeaton.

Architects should be engineers.

Architects and engineers are so nearly connected with each other in the objects of their pursuits, that it would be well both for them and for the public, if every architect were an engineer, and every engineer an architect. That this is not always the case, we have melancholy instances to prove.

Society of civil engineers in London.

There is a society of civil engineers in London, of which Sir Joseph Banks is president, consisting of men of undis-

puted

puted talents and information. Would it not be advisable, to consult this board? No harm could possibly arise from such application, and much good might be the consequence. If in the multitude of counsellors there may be some delay, there is probably much safety. should be consulted.

Having now animadverted upon the steps that should be taken, before any plan is ultimately settled, I shall venture to offer a few hints upon the construction of a theatre. If any thing, which I throw out, should become an object of discussion, I trust that I may have an opportunity of explaining what I propose; and if any thing be adopted from my suggestions, that it may not be followed, without my being acquainted with the mode of execution. Many new attempts fail of their object by the introduction of additional ideas, that appear plausible; or by the omission of small circumstances, that seem in the original plan to be of no material consequence. Plans should be executed under the eye of the proposer, or followed strictly.

In building a theatre, Leading objects in building theatres.

1. Security to the audience is the first and most necessary object.

2. Facility of ingress and egress.

3. Facility of seeing and hearing.

4. Convenience to the performers.

5. Space for scenes, with proper openings for the machinery.

6. And lastly, expense.

1. *To ensure safety*, common sense points out, that as little timber and as small a portion as possible of combustible materials should be employed. The outside walls should be constructed of stone—the coins of large blocks of stone closely jointed, depending upon their own bearings and not made apparently compact by mortar. Bricks for the internal structure should be made under proper inspection, and not worked hastily up, to fulfil a contract. All the joists, rafters, and principals, and the framework of the partitions, should be iron. The framework of the roof should be of the same metal, with a covering of copper. No plumber should be permitted to exercise his dangerous For safety avoid timber, and substitute iron. Roof with copper, and admit no plumber's work.

geous timber in the construction of any part of the building.

Iron not expensive if employed with skill.

It may at first sight appear, that the substitution of iron for timber must be enormously expensive—and it would be enormous, if scientific care were not taken, to calculate the stress and strength of every part of the structure where iron was to be used, and to frame the material together upon mechanical principles of strength and lightness.

Roof of iron cheaper than timber.

Hollow brick flooring.

As to the roof, it could no doubt be made lighter and cheaper of iron than of timber at the present price of that material. Cotton mills are frequently floored with hollow bricks, which are light; and these may be covered with carpetting.

Wood that does not flame advisable.

Deal may be prepared so as to be less inflammable.

Many other parts of the theatre might be constructed of iron and copper; and stucco might be introduced in many places instead of wood. There are kinds of timber that do not flame; these, though not very durable, might be employed for floors and benches. And where deal is absolutely necessary, it may be covered or imbued with a wash, that in some degree will retard inflammation. After the wood work that requires painting has received two coats of oil paint, it may be finished with a coat in distemper, which may frequently be renewed at small expense, and without the disagreeable smell of oil paint.

The private apartments should be heated by steam; and the boiler should be adapted to work an engine for supplying and throwing water.

To heat the green room, dressing rooms, and the withdrawing rooms, steam might be advantageously employed; and the boiler to supply the steam should be so placed, as to serve at a moment's warning, to work a steam engine of force sufficient to draw water at once from the Thames, and to drive it with a strong impulse wherever it should be wanted. This steam engine should be strongly enclosed in a building, to which access on every side could be easily obtained.

Avenues.

2. Some of the theatres at Paris have commodious avenues; but not one theatre in London has been so placed, or so constructed, as to afford tolerable convenience either to the higher or lower class of spectators.

Private property intervenes so much, that it is scarcely to be expected, that any great improvement can be made in

in this respect, by enlarging the area round the site of the late building.

Whether a more convenient situation might be selected, I do not pretend to know; but a theatre built on the old foundation might be rendered extremely commodious as to its entrances, or *vomitories*, as the ancients called the avenues to their amphitheatres.

The entrances might be made very commodious

If the whole building were raised upon arches of a height sufficient to admit carriages, and if numerous flights of stairs were constructed within the piers which support these arches, the audience might depart commodiously in different directions, without confusion or delay.

by raising the building on arches.

The colonnades formed by pillars properly disposed would permit alternate rows of carriages. Company might descend from the boxes almost immediately into their carriages: passages for those who were on foot might be railed off, and rendered secure.

This plan would be attended with considerable expense; but it might be counterbalanced by sparing one of the higher galleries, which lately injured the *audibility* of the performance, without adding much to the profits of the house. Besides it might be so managed, that tickets for the admission of carriages under the *piazzas* should be issued, which would cover the expense of their construction.

This expense might be compensated.

3. *Facility of seeing and hearing.*—As to seeing I believe that very little can be said, but what is obvious to every person of common sense; the actors and the spectators have in this respect opposite interests. It is the interest of the actors, to have that part of the house, which contains the audience, as large as possible. On the contrary it must be the wish of the audience, within certain bounds, to be near the stage; and in all cases, the audience must wish, that every part of the pit, galleries, and boxes, should be equally commodious for seing. Now in a large theatre this is impossible. To extend the pit and boxes, they must recede from the front of the stage; they cannot be extended in breadth without shutting out the view from the side boxes.

Facility of seeing,

Little

and of hearing. Little inconvenience was felt as to seeing at Drury-lane; but every body, who wished to hear, complained. As to the actors, to make any impression, they were obliged to raise their voices above the natural pitch; to substitute pantomimic gesticulation, in the place of inflexions of voice; and to use contortions of features instead of the natural expression of the eyes, and the easy movement of the countenance. It is in vain, that critics inveigh against the bad taste of those, who prefer show, and pantomime, and processions, and dancing, and all that the French call *spectacle*: unless we can hear the sentiments and dialogue, it is useless to write good plays; but all the world loves *spectacle*. Both these tastes should be gratified. Garrick, as I have heard him declare, was always entertained with a pantomime: he told me how many times he had seen Harlequin Fortunatus with delight—the number I forget, however I am sure, that it far exceeded the number of times any man could hear a good comedy or tragedy. Surely the literary and the visual entertainment of different spectators might be gratified.

Garrick fond of pantomime.

Audience part. In the first place, the audience-part of the theatre should be left smaller, and lower, than it was at Drury-lane. Its shape might undoubtedly be improved, by constructing it according to the known laws of accoustics: but this, if rigorously attended to, would contract the space so, as to affect too much the *receipts* of the house.

Stage and scenery. The area for the stage might be as large as it was formerly; but the scenery should be adjusted so as to contract the stage to reasonable dimensions. To confine the voice, the wings should have leaves, or flaps, hinged to them, so as occasionally to close the space between the wings, leaving sufficient room for exits and entrances. When large objects require admission, these leaves might be turned back, and would then allow the same space as usual between the wings. This would be an additional convenience to the actors, while they stand in waiting to enter on the stage, as it would screen them from the cold.

Ceiling of the stage. The ceiling of the stage, which at present is made by strips of painted linen hanging perpendicularly, should be made of well varnished iron or copper

copper frames, turning upon centres so as to open at pleasure like venetian window-blinds; and by this means to contract, at will, the opening of the ceiling, and to conduct the voice of the performers towards the audience. The current of air, so as it does not amount to wind, should flow from the stage to the audience. By experiments tried upon sound by Sir Thomas Morland and some other members of the Royal Society, it appeared, that the propagation of sound was prodigiously obstructed by the assistance or opposition of a slight current of air. We are told by Vitruvius, and Lipsius, that the sound of the actor's voice was increased in a surprising manner by brazen vessels placed under the seats of the audience.

A gentle current of air should flow from the stage.

Sound increased by brazen vessels under the seats.

No satisfactory account remains of the manner in which this desirable effect was produced. It would not however be difficult to try experiments on this subject in any one of our theatres when it is vacant.

About 40 years ago I happened to go with a friend into a large cockpit at an inn at Towcester. My friend, who was at the opposite side of the pit, appeared to me to speak with a voice uncommonly loud and sonorous. Upon my inquiring why he spoke in that manner, he said, that he had not raised his voice above its ordinary pitch. Upon looking about I perceived a large earthen jar behind me, which proved the cause of this increase of sound: for upon repeated trials the voice of my friend sounded as usual when I stood in any other part of the cockpit, but that in which the vase was placed. To the best of my recollection the jar was about five feet high, and twenty inches in diameter. I remember well, that it rung clearly, but slowly, when struck with the knuckle. By what means, and by what materials, the pulses of sound may be best returned for the purposes we have in view, is a subject for the joint efforts of mathematics and experiment.

Sound increased by an earthen jar behind the auditor.

Among other expedients pannelling the backs of the boxes with thin elastic plates of brass might be tried.

Expedient suggested.

A saving and advantage would certainly arise in all cases from using iron, or copper, instead of wood; they would not require renewal for many years, and they would be a preservative against

against fire. The prompter's box might certainly be improved, so as to throw the prompter's voice more distinctly upon the stage, and to prevent its being heard by the audience.

Comforts of the performers should be studied.

4. *Convenience to performers.* Notwithstanding the reveries of Rousseau, and the declamations of the overrighteous, actors have risen in the estimation of the public. We have seen with rational and sincere pleasure the excellent conduct of many female performers. I consider this reform as highly advantageous to morality, and it becomes a duty in the managers of a theatre, to accommodate the performers with every possible convenience, so that they may enjoy that English word *comfort*, which in all situations of life tends to promote independance and morality.

Speaking pipes.

It is scarcely necessary to add, that pipes to speak through should be laid from the green room to every apartment of the actors.

Expense.

6. I have left the article of *expense* to the last, because whatever essentially tends to the convenience and gratification of the public will always find sufficient supplies from the liberality of Britain. A small addition to the price of tickets would amply defray the expense, that would be incurred by any real improvements.

If the united efforts of men of science and men of practice were directed to this object, we might expect to see a theatre superior to any on the continent, adapted both to the purposes of splendid exhibition and of true comedy; where our children might be entertained with the "Forty Thieves," and ourselves with "The Rivals" and "The School for Scandal."

R. L. E.

XI.

XI.

Plan for Preventing or Suppressing Fires. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

THE destructive fires, that have recently taken place in London, have induced me to compress a few ideas on the subject of watching public buildings, which have arisen from a desire to form a plan of safety for a building in which I am myself interested. I shall confine these observations to the prevention or suppression of fire, in such a theatre as *that* lately in Drury Lane, or Covent Garden; and, if they are calculated for a place in your valuable Journal, they are at your service. Watchmen to prevent fires.

Let it be supposed, that such a building is directed to be nominally divided into convenient sections, each capable of being and actually attended to by one watchman. A small chamber, or any other space, in addition to and distinct from these, in a proper situation, shall be occupied by a person to direct or check these watchmen. The direction may be exercised ordinarily without leaving this chamber, in the following manner. Let there be one clock for each watchman, of a certain construction (which is at present partially in use, and proved to accomplish purposes similar to the object of the present paper) fixed in the chamber of the director of the watchmen; each clock communicating with the section of its proper watchman by cranks and wires, or otherwise, in such a manner, that by pulling the wire he shall be able to effect a visible alteration on the clock at a precise moment, as agreed upon, conformably with the construction of the clock, but not at any other moment. This clock shows the usual division of time, and has also a revolving frame in which pins are placed in sockets capable of being pressed down at particular times only, as above stated. Thus, by the use of this clock, a watchman's vigilance or neglect may be proved by the evidence of the clock itself. Method of ensuring their vigilance.

Suppose,

Its operation. Suppose, for example, this clock be so constructed, that a pin shall be pressed down every quarter of an hour, and the proof of this being done shall rest with the director of the watchmen, in the first instance, simply by looking at the clock every quarter of an hour; it is evident, that the neglect of a watchman cannot exist longer than this space of time, if the director fail not in his duty. This man should himself be watched with the most scrupulous suspicion, and detected in his own failure, in the same manner as he should detect the failure of the watchmen; that is, by the proof of a clock on the same principle as the above, placed on the outside of the building, and under the absolute examination of the police, or any other superintendance satisfactory to those most interested. If the director be correct, instant alarm would be led to the section of any watchman whose duty should appear to be neglected; and if the director be incorrect, the alarm would be ulterior, and as active as in the case of positive danger. It may be thought difficult for one man to examine many clocks at the same moment: if it should be found so each clock might be set differently, and every watchman have a clock in his section set by his proper clock in the directer's chamber.

Thus speedy assistance ensured. Hence, in case of fire, a discovery would not only soon take place, but personal assistance would be on the spot, and, with proper access to water at all times ensured, with the best means of applying it, an increase of the first evil would almost certainly be prevented, until additional assistance could be procured: and alarm bells or other signals, by the sound or character of which the particular building might be made known to firemen, could, if necessary, be instantly sounded or displayed, and a constant influx of proper persons would take place in the very infancy of danger.

The plan has been tried. It is not improbable, that this plan may be thought by many persons too elaborate and expensive. To such it will be satisfactory to know, that very extensive and valuable buildings in my neighbourhood, the property of some highly ingenious and respectable gentlemen (one of whom is the inventor of the clock) have been watched for several

years

years by a single watchman, checked by this clock alone, and with extremely few evidences of neglect. This is the result of fines, &c., begun with judgment, and enforced with strictness. But one objection can be offered against this, namely, that the morning only brings the proof of the watchman's conduct, when nothing can be opposed to his neglect but fine or dismissal; while the hours of greater danger must be left to his discretion, and the fear of punishment. Objection.

As many modifications of plans like this are easily devised, and new arrangements made in application to practice, not readily imagined before, it is deemed unnecessary to enter into detail, or to attach any specific regulations for each department, or for the ultimate execution of the whole. This plan may be adapted to circumstances. If it is satisfactorily made out, that the plan is practicable and useful, a slight calculation will show the expense to be insignificant, when compared with the object, or even with the premium of insurance. The expense not an object.

I am, Sir,

Your obedient servant,

Derby, May 11, 1809. M. K.

Annotation. Respecting register clocks for the useful purpose indicated by M. K. see our Journal, vol. V, p. 133.

XII.

On the Method of taking Transit Observations. In a Letter from a Correspondent.

To Mr. NICHOLSON,

SIR,

IN the second volume of your Journal, Mr. Ezekiel Walker, after mentioning Dr. Bradley's method of taking transit observations, by noting the proportional distance of the Method of taking transit observations.

the star from the wire at the two beats of the clock, proposes another, which he thinks superior. This consists in noting the time when the centre of the star comes to one side of the wire; which, he observes, is a real line, and not as in Bradley's, a line drawn by the strength of imagination down the middle of the wire, parallel to the sides. I have tried both these methods with nearly the same success, and must confess that, after all, I am very much at a loss to conjecture, how the fractional part of a second can be estimated in either of these ways to that nicety it appears to be done. In the observations made at Greenwich I observe, the time of a star's passing the meridian is always expressed to the hundredth part of a second. How this extreme precision is obtained, as I am at a loss to conjecture, I shall be obliged to you, or any of your correspondents, to inform me.

I am, Sir,

Your obedient servant,

J. G——.

REPLY.

Method of taking transit observations.

It is certainly not difficult to observe to tenth parts of a second; and of this my correspondent will easily satisfy himself by trial with a common watch of five beats in a second. A phenomenon, as for example, the transit of a star, may take place in any one of the five beats, or between any two of them. If the observer repeat the words (either mentally or otherwise) *One*, one, one, one, one—*Two*, two, two, two, two—*Three*, three, three, three, three, &c., at each beat of his seconds clock, the word in Italic at the very beat, he will be enabled to mark the fractions of seconds with great precision. Musicians, in the rapid execution of prestissimo movements, divide the second still lower. As to the hundredth parts of seconds, though it might by some expedients be practicable to observe them, this is not implied in Astronomical Tables. They are almost always the results of means taken between a number of observations; and the second decimal may be considered

as

as indicating the precise value of the first, instead of the sign + or —, which is sometimes annexed for the like purpose.

W. N.

XIII.

Examination of the Root of Calaguala: by Mr. VAUQUELIN *.

THIS root has a brown colour and a wrinkled surface in consequence of dessication. In some parts it is covered with scales like those found on the roots of common ferns. It is hard, coriaceous, and difficult to powder. It appears to be the root of a species of polypody. External appearance.

Exp. 1. Thirty grammes (463 grains) of this root coarsely powdered were digested in three hundred grammes of distilled water for forty eight hours. The water acquired very little colour, but it had a degree of consistence and unctuosity, so that it would not easily pass the filter. Its taste was slightly saccharine. Digested in water.

The infusion having been mixed with different reagents, the following effects were produced in it. Action of reagents on the infusion.

1. By alcohol was thrown down a yellowish white flocculent precipitate.

2. With sulphate of iron it assumed a blueish green colour, but without any perceptible precipitation.

3. With acetite of lead a very copious yellowish white precipitate was produced.

4. Oxalate of ammonia occasioned a very light precipitate in it.

5. No precipitate occurred on the addition of nitrate of barytes, infusion of galls, or solution of animal gelatine.

6. Lastly it was slightly reddened by infusion of litmus.

The effect of alcohol teaches us, that it contains a mucous substance: that of sulphate of iron, that it contains a resin similar to those of cinchona, of rhubarb, &c.: that acetite of lead indicates an acid, which may perhaps be the malic: Inferences from these effects,

* Annales de Chimie, vol. LV, p. 22.

of

of oxalate of ammonia, a little calcareous salt. The nitrate of barytes proves, that it contains no sulphuric salt: the galls, that it has no animalised substance: the solution of isinglass, that no tannin is present. The infusion of litmus shows the presence of some acid.

corroborated by farther experiments.

The following experiments, to which I was led by the foregoing, will demonstrate by their results the existence of most of the principles indicated above.

Digested in alcohol.

Exp. 2. Thirty grammes of the same root were digested forty eight hours in about 200 grammes of alcohol. This liquid assumed a deeper colour than the water employed in the first experiment. Its taste was at first saccharine, but it left behind a very strong sensation of bitterness.

Precipitated by water.

On the addition of water it became slightly milky, which confirms the existence of the resin mentioned above.

Distilled.

This tincture subjected to distillation till it was reduced to six or seven grammes, afforded a certain quantity of oil of a deep red colour, which was precipitated to the bottom of the liquid. The supernatant fluid had then not so deep a colour, and a less bitter, but more saccharine, taste. These effects were owing to the separation of the resin by the evaporation of the alcohol; and to the fluid remaining as less volatile holding in solution the saccharine matter.

Residuum.

As a little alcohol still remained in the fluid, which retained some of the resin in solution, I evaporated almost to dryness with a gentle heat. I then washed the residuum with a little distilled water, which enabled me to separate the saccharine matter pretty accurately from the resin. The alcohol that had come over had not carried with it any sensible portion of oil, for it was not rendered turbid by the addition of water; but thus mixed it had a peculiar smell, and an acrid taste.

Resin.

The resin separated from the saccharine matter in the manner above mentioned had a brownish red colour, a very strong acrid and bitter taste, and was soluble in alkalis, to which it imparted a brown colour and considerable bitterness. Acids decomposed this alkaline solution, and separated the resin just as it was before.

Probably destroys the tape-worm.

Is not this resinous substance, which ought equally to be found in the other species of ferns, the principle that destroys

destroys the tape-worm? This is not improbable, for we know, that all acrid and caustic oils produce this effect.

The saccharine substance, which had been dissolved by the alcohol at the same time with the resin, gives a slight lemon colour to water. It is reduced to a thick and viscous substance by evaporation. Its taste is sweet, pleasant, and slightly acid. Its smell is nearly similar to that of the juice of apples when evaporated. On being heated it swells up, grows black, and emits a smell exactly resembling that of burned sugar. I found in it perceptible traces of muriate of potash. Thus it appears there can be no doubt, that this substance is a true sugar, with which an acid, probably the malic, is mixed; but of this I could not satisfy myself by experiment, the quantity being too small. Saccharine matter.

Exp. 3. To obtain those principles of the calaguala root, which are not soluble in alcohol, I digested in water for forty eight hours that portion of the root, which had already been treated with alcohol, as has been seen. The colour it imparted to the water was deep, as if it had given out nothing to alcohol. This infusion had no bitter taste, like that in alcohol: it frothed when shaken; it precipitated solution of silver pretty copiously in a substance which had all the appearance of muriate of silver. Evaporated in a gentle heat, it left an extract of a brown yellow colour, transparent, very tenacious and stringy, on which spirit of wine had no effect. This extract had a mucilaginous and slightly nauseous taste: mixed with a little sulphuric acid it grew black, and exhaled copious fumes of muriatic acid: put on a redhot iron it swelled up, and emitted a smell similar to that of gums. This matter then appears to be nothing but a mucilage coloured by a small quantity of extractive matter insoluble in alcohol, and mixed with a certain quantity of a muriatic salt, probably with potash for its base. Root digested in water, after being treated with alcohol. Properties of this infusion.

Exp. 4. The root of calaguala thus successively exhausted by alcohol and water I afterward treated with weak nitric acid, in order to know whether it contained any amylaceous matter. After two days digestion with a gentle heat, I filtered the liquid, which had acquired only a slight amber Residuum treated with nitric acid.

amber colour, while the root had become of a pretty bright red.

An alkali added.

An alkali mixed with this fluid precipitated nothing; but it produced in it a very lively and agreeable violet red colour. The filter too, through which I passed this nitric infusion, assumed on drying a pretty fine red.

Precipitated with alcohol.

The same nitric infusion, being mixed with four parts of alcohol, yielded a light flocculent precipitate of a very fine white colour, which, when separated from the supernatant fluid, and washed with fresh portions of alcohol, redissolved in cold water. This substance had all the appearance of common starch, that had been dissolved in nitric acid, and afterward precipitated by alcohol: but I had not a sufficient quantity, to satisfy myself that it was so in a positive manner. At least there is every reason to believe, that it is not gum, otherwise it would have dissolved in water, and furnished some traces of mucous acid on being treated with nitric acid; but I obtained from it only the oxalic. The nitric acid then, according to all appearance, took up from the calaguala root a certain quantity of amylaceous matter, and a colouring substance insoluble in alcohol, which alkalis turn to a violet.

The residuum, ⅘ of the whole, incinerated.

The calaguala root treated by the different reagents mentioned above, and afterward dried, had lost a fifth of its weight. All that remained was the woody part, and the earths insoluble in acids. To ascertain the nature of the latter, and pretty nearly their quantity, I burned the residuum in a crucible till it was completely incinerated; and from about twelve grammes of the root, I obtained half a gramme of ashes, which were composed of carbonate of lime, that the nitric acid had not dissolved, and certainly did not exist in that state in the root itself, with a small quantity of muriate of potash, and some traces of silex.

The root treated with the same menstrua in a different order.

I treated the calaguala root a second time with the same menstrua, but in an inverted order, beginning with water, next employing alcohol, and finishing with nitric acid. By the first operation I obtained the sugar, the gum, part of the salts, and a little colouring matter. By the second I got the resin, and a little of the sugar, that had escaped the

the action of the water. Lastly by the third I dissolved the amylaceous portion, and the peculiar colouring substance I have mentioned above.

On recapitulating all the products obtained by the different operations mentioned in the course of this paper, we find, that the root of calaguala is formed of

Component parts of the root.

1. A large quantity of woody matter:

2. A gummy substance, which comes next in point of quantity:

3. A red, bitter, acrid resin, the next in proportion:

4. A saccharine matter, tolerably abundant:

5. An amylaceous part, the quantity of which I did not ascertain:

6. A colouring matter soluble in nitric acid, and turning violet on the addition of an alkali.

7. A small quantity of acid, which I could not discriminate, in consequence of its being so little, but which I suspect to be the malic:

8. A tolerably large quantity of muriate of potash:

9. Lastly lime and silex.

Medicinal parts.

Of all these substances those soluble in water and alcohol are alone capable of producing any effect on the animal economy. These substances are the sugar, mucilage, muriate of potash, and resin.

Roots of male fern and common polypody contain the same principles and tannin.

Since the time when I analysed this root at the request of Mr. Alyon, I have subjected to similar experiments the roots of common polypody and the male fern, and obtained from them precisely similar principles nearly in the same proportions as from the calaguala root. The former roots however contain a small quantity of tannin. Thus the analogy of organization, which led Mr. de Jussieu and Mr. Richard to conclude, that the medicinal virtues of the calaguala root must be similar to those of other ferns, is fully confirmed by chemical analysis.

XIV.

On the Chemical Nature of the Smut in Wheat. By Messrs. FOURCROY *and* VAUQUELIN *.

Smut has already been examined imperfectly.

THE smut in wheat has already occupied the attention of several chemists. Parmentier has found in it a fetid, fat, and coally substance. Cornet has observed its oleaginous nature. Girod-Chantrans, in 1804, announced, that it contained also a free, fixed acid, which he supposed to be of a peculiar nature.

This discovery, announced to the Institute in the autumn of that year, induced Mr. Vauquelin and me to undertake a full examination of this degenerated vegetable matter.

Described.

It is well known, that the smut is in fact a corruption of the grain, which exhibits within the husk of the seed, instead of a farinaceous substance, a black, greasy, stinking powder, the most decided and dangerous characteristic of which is its being capable of infecting other grains by contact, and imparting to them the property of propagating smutty wheat. It is known too, that washing with lime and alkalis is the most certain method of removing its contagious property, and preventing the disease from being reproduced, which it constantly is, if this practice, now generally employed by all judicious farmers, be neglected.

Prevented by washing with alkalis.

Smut

The smut, on which we made our experiments, was given us by Mr. Girod-Chantrans.

treated with hot alcohol,

Triturated in an agate mortar, and separated from the husk, the smut imparted to hot alcohol a yellowish green colour; and, without communicating to it any character of acidity, exhibited only about a hundredth part of its weight of a deep green oily matter, as thick as butter, and acrid as rancid grease.

ether,

Ether separated from it the same oil.

and water.

After this action of alcohol, the smut retained both its greasy feel, and filthy smell. Lixiviated with five times its

* La Revue Philosoque, &c. Nov. 1805. Abridged from a paper read at the National Institute.

weight

weight of boiling water, it gave it a brown red colour, a fetid smell, a soapy quality, and a very decided acidity.

This acid, examined by various appropriate reagents, exhibited all the properties of the phosphoric. Acid appeared to be the phosphoric.

On lixiviating pure smut, not previously treated by alcohol, with boiling distilled water, this liquor, which was perceptibly acid, being saturated with potash, gave a precipitate of animal matter, mixed with crystallized ammoniaco-magnesian phosphate, and every proof of an alkaline phosphate. These experiments therefore confirm the existence of free phosphoric acid in smut, known by its fixedness, its insolubility in alcohol, its solubility in water, its precipitation by lime, &c. This confirmed.

After the aqueous infusion had been precipitated by potash, it held in solution a fetid animal matter, resembling in colour, smell, and the phenomena exhibited by its precipitation with various reagents, that found in water in which the gluten of wheat has putrefied. Animal matter resembling that from putrid gluten.

After having undergone the action of alcohol and water successively, the smut of wheat still retained both its fetid smell and greasy feel. Distilled on an open fire it afforded a third of its weight of water impregnated with acid acetate of ammonia; nearly a third of a deep brown, concrete oil, much resembling adipocere in its form, consistence, and fusibility by a gentle heat; and 0·23 of a coal, which, being incinerated, left 1 gramme [15½ grs.], being a hundredth part of the original smut, of white ashes, three fourths of which were phosphate of magnesia, and one fourth phosphate of lime. The residuum distilled.

We examined the smut with its husk, to compare it with that which had been deprived of it, but we did not find difference enough to ascribe to the bran that covers it any decided influence on its analysis. Smut examined with the husk.

From our examination, the leading results of which have just been given, we conclude, that the smut of wheat contains, Its component parts.

1. A green, butyraceous, fetid, and acrid oil, soluble in hot alcohol or ether, composing near a third of its weight, and imparting to it its greasy consistence. Oil.

2. A vegeto-animal substance, soluble in water, insoluble in Vegeto animal substance.

in alcohol, and precipitating most of the metallic salt, as well as galls. It composes rather less than a fourth of the smut, and is perfectly similar to what comes from putrified gluten.

Coal.

3. A coal, amounting to one fifth of its quantity, which gives a black colour to the whole mass; and is an evidence, as it is the product, of a putrid decomposition; a part which it acts equally in mould, and in all the remnants of putrified organic compounds.

Phosphoric acid.

4. Free phosphoric acid, scarcely constituting more than .004 of the smut, but sufficient to impart to it the property of reddening blue vegetable colours.

Phosphates.

Lastly the phosphates of ammonia, magnesia, and lime, in the proportion of a few thousandths only.

A residuum of grain destroyed by putrefaction.

The smut of wheat then is nothing more than a residuum of the putrified grain, which, instead of its original component parts, starch, gluten, and saccharine matter, exhibits only a kind of carbonaceous oily substance, very analogous to a kind of bitumen of animal or vegeto-animal origin.

Putrified gluten exhibits similar results.

We must here remark, that in our examination of gluten decomposed by putrefaction, we found characters very similar to those of the smut of wheat; and that the products of the one are so like those of the other, as to render it difficult in certain cases not to confound them together. It requires a man to be well practised in chemical experiments, to discern the slight differences, that exist between these two putrified matters, because these differences consist only in delicate shades, that are not easily perceivable.

Still we are ignorant of its cause.

Interesting as the results of this analysis may appear, we must confess, there is still a great distance from the knowledge they give us of its nature to that of its cause; and yet more to that of its contagious quality, which is proved by so many experiments, as to leave no room for the slightest doubt. We must own too, that these results, while they indicate the smut to be the residuum of putrified farina, do not entirely agree with the ideas of philosophical agriculturists, who consider this disease as the necessary product of contagion; since it thus seems natural to presume it arises from putrid decomposition, which may proceed from any other circumstance as well as a communicated germe.

May arise without contagion.

Attacks the gluten.

The same results lead us equally to infer, that the putrescency,

trescency, which necessarily precedes the formation of the smut in all cases, whether it depend on contagion or arise spontaneously, attacks particularly the gluten; and precedes, indeed prevents, the formation of the starch: since we know positively, that this fecula, no traces of which are found in the smut of wheat, suffers no alteration from that septic process, which so powerfully attacks the glutinous substance.

XV.

Of the Action of Nitric Acid on Cork; *by Mr.* CHEVREUL*.

BRUGNATELLI having examined the action of nitric acid on cork, in 1787, found, that the cork was converted into a peculiar acid. In 1797 Bouillon-Lagrange resumed the inquiry of the Italian chemist, and confirmed the existence of the suberic acid. In the two papers he published on this subject, he described the characters of this acid, and its combinations with the salifiable bases, which Brugnatelli had not studied. Notwithstanding these labours, several persons still entertained doubts of the existence of this acid. They thought, that it was only one of the acids previously known combined with some matter, by which its properties were concealed. Of the truth of this I was desirous to satisfy myself by experiment. History of the discovery of suberic acid.

To form suberic acid, I followed the common process; which I shall here recite, with the phenomena that occurred in the operation. Preparation of the suberic acid.

In a retort, to which a receiver was adapted, I heated six parts of nitric acid at 29° on one of rasped cork. The matter grew yellow; nitrous gas mixed with carbonic acid was evolved; and a pretty large quantity of prussic acid was formed. I returned the product from the receiver into the retort several times, that the cork might be acted upon sufficiently. When the action of the acid appeared to abate,

* Annales de Chimie, vol. LXII, p. 323.

abate, I poured the matter still hot into a porcelain capsule, where I finished the evaporation with a gentle heat, stirring it continually. As soon as it was reduced to the consistence of an extract, I put it with some water into a large glass phial on a sand heat. At the end of a few hours I withdrew it from the fire; and on cooling two solid substances separated. One of these, which I shall call A, sunk to the bottom in the form of large flocks: the other, B, congealed on the surface of the liquor like wax. This I removed with a piece of card; the other I separated by filtration.

Examination of the matter A.

The flocculent precipitate, A, was insipid; insoluble in water and in alcohol; and of a white colour, but turning a little brown on exposure to the air. Nitric acid at 32° did not act on it perceptibly. Placed on a red hot coal, it burned without swelling up, and emitted a pungent smell of empyreumatic vinegar. Its coal was bulky, and pretty hard. This substance therefore was nothing but the woody part naturally contained in the cork.

Examination of the matter B.

The supernatant substance, B, had very little taste. It was insoluble in water; but boiling alcohol dissolved it, some portion of woody matter excepted. The filtered solution on cooling let fall a white substance resembling wax. This being separated by a second filtration, I added water to the solution, which threw down a straw coloured resinous substance, that turned reddish by exposure to the air, and was acid, notwithstanding I had washed it repeatedly. On distillation it yielded a sort of concrete fat, and a very acid fluid, that precipitated acetate of lead. I could not ascertain its nature from the smallness of its quantity.

The water that had been employed to precipitate the resin acquired a yellow colour by evaporation, and a taste resembling that of bitter almonds. It contained only a little of the yellow matter, and probably a few atoms of prussic acid.

Examination of the fluid separated from the matter A.

The fluid from which the matter A had been separated had an acid and bitter taste; precipitated lime water and calcareous salts; turned solution of indigo green; contained a little iron, as appeared on the addition of galls; and, when the excess of acid was saturated, it did not precipitate gelatine,

gelatine, consequently contained none of the tannin of Mr. Hatchett.

In evaporating the fluid with a gentle heat, it emitted a pretty decided smell of vinegar. This induced me to finish the evaporation in a retort; but I obtained only nitric acid, without any acetous. Whether this were dissipated at the commencement, or its quantity were too small for me to detect it, I cannot say. The liquor, after evaporation and cooling, let fall an *acid sedimental matter*, which I separated by filtration. Four successive evaporations afforded me fresh acid. After the fifth evaporation I obtained crystals of oxalic acid. Having decanted the mother water, which was yellow, and had a very bitter taste, I precipitated the oxalic acid it still retained by lime water in excess, and distilled it. The liquid that came over into the receiver contained a little ammonia. I then precipitated the liquid left in the retort by carbonate of potash, and lime was thrown down. The filtered liquor yielded in a couple of days some small gold coloured crystals of the bitter yellow matter combined with potash *. Suberic acid.

This acid sedimental matter was the suberic acid. I washed off with cold water part of the yellow matter that coloured it, and completed its purification by repeated solution in boiling water, from which it separated by cooling in little white flocks. By concentrating the bitter waters I separated that, which they held in solution. By this process I obtained a very white acid, about five parts of which were obtained from sixty of cork.

The suberic acid is as white as starch. It has an acid taste, without any bitterness. Light does not alter its whiteness. To dissolve one part of this acid requires 38 parts of water at 60° [140° F.], and 80 parts at 13° [554° F.]. Its little solubility prevents us from having it crystallized; so that when it is dry it is always pulverulent and opake. Properties of the suberic acid.

* Having saturated the mother water of these crystals with muriatic acid, I obtained a precipitate, which exhibited all the characteristics of benzoic acid: but I dare not venture to assert, that this acid is constantly formed, for in three operations on cork I obtained it but once, and then in a very small quantity.

Thrown

Volatilized. Thrown on hot coals it is volatilized, without leaving any residuum, and emitting a smell of suet.

Heated in a retort. When heated in a small glass retort on sand, it melts like fat with a gentle heat. If the retort be withdrawn from the fire, and the melted acid diffused over its inside, it crystallizes in needles by cooling. If the distillation be continued, it rises in vapours, which condense in the summit of the retort in white needles, some of which are half an inch long. This sublimate has all the characters of suberic acid. A slight coally mark is left in the retort.

Solution in water. Suberic acid dissolved in water reddens litmus very distinctly. It does not precipitate lime *, strontian, or barytes water, or the saline combinations of these bases. On evaporating lime-water saturated with suberic acid, the calcareous suberate falls down in a white flocculent precipitate, from which muriatic acid separates the suberic. This is indeed an excellent method of obtaining it perfectly white. The muriate of lime may be separated from it, by dissolving it in a small quantity of hot water; when by cooling we obtain the acid, which is always in a pulverulent form †, and similar to what it was before being combined with the lime; only this base takes from it the remains of the colouring matter, which the water had not dissolved.

Method of purifying.

Mistake of Brugnatelli. * It seems to me, that Mr. Brugnatelli must have deceived himself, when he says, that suberic acid precipitates lime water, and all the mineral calcareous salts. The oxalic acid, which no person has mentioned, and which is formed with the suberic acid, was no doubt the occasion of the precipitate he obtained. It appears to me also, on reading the article suberic acid in Brugnatelli's Elements of Chemistry, vol. II, p. 106, that the acid he describes still retains bitter matter, resinous matter, and oxalic acid.

† I made this experiment, in order to see whether the suberic acid were analogous to the benzoic, and, in this case, to separate it from the matter that prevented its crystallization.

Purification by barytes. I repeated the same experiment with barytes instead of lime, and had the same result. The suberate of barytes is deposited by concentration, and its decomposition by muriatic acid afforded me the suberic acid perfectly white. A small excess of the acid should always be employed, in order to separate the last portions of base, which the suberic acid might retain.

Ammonia

Ammonia and the fixed alkalis dissolve suberic acid very well. These combinations, when concentrated, let fall their acid on the addition of sulphuric acid, muriatic, &c. Action of alkalis.

The suberate of ammonia precipitates the solution of alum, and the nitrate and muriate of lime. But to obtain precipitates with the latter concentrated solutions must be employed, for the suberate of lime is pretty soluble. Suberate of ammonia.

Suberic acid throws down a white precipitate from a perfectly neutral solution of silver, from muriate of tin at a minimum, from sulphate of iron at a minimum, from nitrate and acetate of lead, and from nitrate of mercury. It does not precipitate sulphate of copper * or of zinc. Action of the acid on the metals.

Suberate of ammonia decomposes almost all the metallic solutions. The cupreous salts are precipitated by it of a pale blue; the cobaltic, rose-coloured; those of zinc, white; &c. Action of the suberate of ammonia.

Nitric acid has no action on the suberic. I boiled twelve parts of the former at 32° on one of the latter, without having any sensible decomposition. The suberic acid was dissolved, and this solution, being boiled down, deposited suberic acid some hours after cooling. I observed, that the addition of water promoted this separation. I thought at first, that I might obtain crystals from this acid solution, but I could not succeed. Nitric acid does not act on it.

Alcohol dissolves the suberic acid very well. When saturated with it, water precipitates a portion. Soluble in alcohol.

The suberic acid does not turn green the solution of indigo in sulphuric acid. Mr. Bouillon-Lagrange however lays much stress on this property, which he considers as a characteristic of the acid; and in fact if this change of colour were owing to a chemical action, it would be very surprising, that a substance formed amidst nitric acid should not have attained its complete oxidation, but remain capa- Mistake of Bouillon Lagrange.

* Bouillon Lagrange says, Ann. de Chimie, vol. XXIII, p. 48, that the suberic acid decomposes nitrate of mercury, and the sulphates of copper, iron, and zinc; and p. 56, that the suberic acid yields mercury and zinc to the three mineral acids, and iron and copper to sulphuric acid; which appears to me contradictory. Mistake of Bouillon Lagrange.

ble

ble of deoxiding indigo. Mr. Bouillon-Lagrange has ascribed to the suberic acid a property, that belongs to the bitter yellow matter, which forms a green by mixture with the blue of the indigo. It is this too, that turns a solution of copper green; for I have satisfied myself, that the white acid merely dilutes the blue colour, just as an equal quantity of water would have done.

Analogous to the sebacic acid.

From what has been said I conclude, that the suberic acid has great analogy with the sebacic, with which Mr. Thenard has made us acquainted *; and that the only striking difference between them is the crystalline form, which the suberic acid assumes when dissolved in water or in alcohol.

XVI.

Method of Fabricating artificial Stone employed in the Vicinity of Dunkirk. By Mr. Bertrand, *Apothecary to the Army of the Coast*†.

Method of making artificial stone in France.

THE materials employed for this purpose are the ruins of the citadel, consisting of bricks, lime, and sand. These are broken to pieces by means of a mill, formed of two stone wheels, following each other, and drawn by a horse. Water is added; and the matter, when well ground, is reddish. This is put into a trough, and kept soft by means of water.

When the trough is full, some lime is burned, and slacked by leaving it exposed to the air, and this is mixed in the proportion of one eighth with the cement above.

A wooden mould is laid on the stone, and after a thin layer of sand is thrown on the stone, to prevent the cement's adhering to it, a layer of cement is poured in, and on this a

* See Journal, vol. I, p. 34.

† Annales de Chimie, vol. LV, p. 285.

layer

layer of bricks broken into acute-angled fragments. Thus two other strata are put in, before the last, which is of pure cement. The mould being removed, the stones thus formed are laid in heaps to dry. The lime being very greedy of water, and quickly becoming solid, these stones are not long in forming a hard body fit for building. Method of making artificial stone in France.

The lime is not very dear, being burned with pitcoal. The labour is not dear, requiring only one strong man assisted by two or three boys of twelve years old. The materials, being from old ruins, are cheap: and only one horse is employed in this manufactory, which is not the only one in the country. I believe others exist in Prussian Poland where these stones are made with much more success, because fragments of basaltes, which are better adapted to form a solid body with lime and alumine, are there used.

The pebbles of Boulogne would be still preferable, and I doubt not with these artificial stone might be made equal to natural stone in goodness.

XVII.

Letter from Mr. Link, *Professor of Chemistry at Rostock, to Mr.* Vogel*.

I HAVE just examined the pollen of the hazel nut. It differs greatly from that of the date tree, which Messrs. Fourcroy and Vauquelin have analysed. It contains a large quantity of tannin, a resin, a great deal of gluten, and a little fibrin. There is animal matter therefore in this pollen. Pollen of the hazel nut.

To learn the properties of the membranous part of plants, I subjected to research the pith of elder, and procured from it by nitric acid every thing, that Bouillon-La- Pith of elder.

* Annales de Chimie, vol. LXII, p. 292.

grange

grange obtained from cork, but without this substance leaving any residuum.

Suberic acid characteristic of vegetable membrane.

As Mr. Brugnatelli obtained suberic acid from paper, I believe it is a peculiar characteristic of vegetable membrane, to furnish this acid.

Crystals in the root of tree-primrose.

In the roots of the *œnothera biennis*, broadleaved tree-primrose, I have seen by the help of a good microscope extremely small crystals, regularly formed, accumulated in the cellular texture. It was difficult to obtain a sufficient quantity for a chemical analysis. They appeared to me somewhat analogous to the crystals obtained from indigo by Nicholson: they are very little, if at all, soluble in water, alcohol, or many of the acids: sulphuric acid itself acts but very feebly on them; the nitric acid alone is their true solvent.

Muriate of silver not blackened without light.

I have endeavoured to blacken the muriate of silver by a current of air employed in the dark, but found it impossible to succeed.

Berthollet's hypothesis.

Mr. Berthollet, as I see in his work, was able to blacken it by a simple current of air. He says, that light acts upon this salt by taking from it a portion of muriatic acid. But how will this celebrated chemist account for the black colour, that muriate of silver assumes when covered with muriatic acid?

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Mineral strata of Clackmannanshire.

AT the meeting of this Society on the 8th of April, was read the first part of a Description of the Mineral Strata of Clackmananshire, from the bed of the river Forth, to the base of the Ochils, illustrated by a large and very accurate plan and section of those strata, done from actual survey,

vey, and from the register of the borings and workings for coal in Mr. Erskine of Mar's estate in that district; communicated by Mr. Robert Bald, civil engineer, Alloa. In this first part, Mr. Bald treated only of the alluvial strata. In continuing the subject, he is to illustrate it still farther by exhibiting specimens of the rocks themselves.

Mr. Charles Stewart laid before the Society a list of the Insects found by him in the neighbourhood of Edinburgh, with introductory remarks on the study of entomology. It would appear, that the neighbourhood of Edinburgh possesses no very peculiar insects, and but few rare ones. The list contained about four hundred species; which, Mr. Stewart stated, must be considered as the most common, as they were collected in the course of two seasons only, and without very favourable opportunities. It was produced (he added) merely as an incitement to younger and more zealous entomologists. Insects near Edinburg.

At this meeting there were laid on the Society's table the first two volumes, 4to. of Count de Bournon's System of Mineralogy, with a volumn of Outlines; a present from the author.

AT a meeting of this Society on the 13th of May, the second part of Mr. Bald's interesting Mineralogical Description of Clackmananshire was read; giving a particular account of two very remarkable *slips* or *shifts* in the strata, near one hundred feet in depth, by reason of which the main coal field of the country is divided into three fields, on all of which extensive collieries have been erected. Mineralogy of Clackmananshire.

The Rev. Mr. Fleming of Bressay laid before the Society an outline of the Flora of Linlithgowshire, including only such plants as are omitted by Mr. Lightfoot, or marked as uncommon by Dr. Smith. This, he stated, was to be considered as the first of a series of communications illustrative of the natural history of his native country. Flora of Linlithgow.

Mr. P. Walker stated a curious fact in the history of the common eel. A number of eels, old and young, were Eels found in a subterranean pool.

found

found in a subterraneous pool at the bottom of an old quarry, which had been filled up, and its surface ploughed and cropped for above a dozen of years past.

Seasnake. The Secretary read a letter from the Rev. Mr. Maclean, of Small Isles, mentioning the appearance of a vast sea-snake, between 70 and 80 feet long, among the Hebrides, in June, 1808.

Plants near Edinburgh. And he produced a list of about one hundred herbaceous plants, and two hundred cryptogamia, found in the King's Park, Edinburgh, and not enumerated in Mr. Yalden's catalogue of plants growing there; communicated by Mr. G. Dow, of Forfar, late superintendant of the Royal Botanic Garden at Edinburgh.

Elementory treatise on Geology. Mr. De Luc has in the press an *Elementary Treatise on Geology*, which will contain an examination of some modern geological systems, and particularly of the Huttonian Theory of the Earth. We understand, that this work is translated from the French manuscript of the Rev. H. De la Fite, M. A., and will form an octavo volume.

French Journals. I HAVE just received some of the French Journals, that have been so long in arrear; and am informed, that the rest are on their way from Paris. From those that have come to hand I extract the following.

Potash in mica. Mr. Klaproth has discovered in mica sixteen per cent of potash.

Turkois analysed. Dr. John, of Berlin, has lately described and analysed an oriental turquoise from Bisiapoor, near Corasan, which he found to contain

Alumine	73
Oxide of copper	4·5
——— iron	4
Water	18
	99·5

This

This result verifies that of the late Lowitz. We have therefore two distinct species of the turquoise; and may give to this now mentioned Pliny's name of *calaïs*.

Dr. John likewise conceives, that he has found a new volatile and acidifiable metal in the grey ore of manganese from Saxony. He obtained it by distilling the ore with sulphuric acid. The volatile metallic acid combines with a weak solution of potash put into the receiver, and tinges it crimson. From this red liquor gallic acid, or infusion of galls, throws down a chesnut brown precipitate. Prussiates immediately change the red colour to a fine lemon yellow, but without any precipitation. The carbonates do not precipitate the red solution; but if it be heated with a little alcohol, the red colour changes to a green, a smell of ether is given out, and then the carbonates throw down a brown oxide, which is soluble in muriatic acid. New metal.

Mr. Bucholz has found, that the schorliform beryl of Bavaria is a true beryl containing 0·12 of glucine. Bavarian beryl.

Mr. Braconnot has analysed some fossile horns of an extraordinary size found in an excavation at St. Martin, near Commercy. He supposes these to have been the horns of the great wild ox, the *urus* of the ancients, *aurochs* of the Germans. From a hundred parts he obtained

Analysis of some fossil horns.

Ferriferous quartz sand		4
Solid gelatine		4·6
Bituminous matter		4·4
Oxide of iron		0·5
Alumine		0·7
Phosphate of magnesia		1
Water		11
Carbonate of lime		4·5
Phosphate of lime, composed of		
Phosphoric acid	28·3	69·3
Lime	41	
		100·

To CORRESPONDENTS.

Mr. Ibbetson's and Mr. Rootsey's Papers, and Mr. Thompson's Analysis of Sulphate of Barytes, will appear in our next number.

Meteorolo-

METEOROLOGICAL JOURNAL

For MAY, 1809.

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

APR. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
23	43	41	46	40	30·01	Rain	Cloudy
24	41	43	48	38	30·28	Fair	Ditto
25	40	43	48	41	30·32	Ditto	Ditto
26	45	44	51	43	29·92	Rain*	Rain
27	48	48	53	46	29·71	Ditto	Cloudy
28	49	47	52	40	29·41	Ditto	Rain
29	42	41	46	35	29·67	Fair	Fair†
30	41	45	50	40	29·75	Ditto	Cloudy
MAY							
1	42	42	50	38	29·35	Rain	Fair
2	43	43	49	37	29·46	Hail ‡	Ditto
3	42	45	52	41	29·80	Fair	Ditto
4	46	44	52	42	29·93	Rain	Ditto
5	48	48	55	40	29·96	Fair	Cloudy
6	50	53	57	50	30·22	Ditto	Fair
7	53	58	64	50	30·32	Ditto	Ditto
8	54	58	64	48	30·30	Ditto	Ditto
9	55	59	65	49	30·22	Ditto	Ditto
10	57	59	67	52	30·08	Ditto	Ditto
11	59	61	70	56	30·00	Ditto	Ditto
12	62	64	72	55	30·00	Ditto	Ditto
13	64	63	71	56	30.00	Ditto	Ditto
14	64	62	72	56	29·96	Ditto	Ditto §
15	66	58	68	55	29·86	Rain	Ditto
16	65	64	70	57	29·79	Fair	Ditto
17	64	65	73	60	29·86	Ditto	Cloudy ‖
18	65	70	72	63	29·82	Ditto	Rain
19	65	68	72	55	29·60	Rain ¶	Ditto
20	58	58	61	51	29·79	Ditto	Fair
21	55	57	61	50	29·93	Fair	Ditto
22	54	54	63	53	30·18	Ditto	Ditto
23	58	55	66	50	30.24	Fair	Ditto
24	56	55	65	51	30·21	Ditto	Fair
25	53	53	62	51	30·09	Ditto	Ditto
26	54	51	63		29·90	Ditto	Air chilly, with rain

* The whole day.
† Too cloudy at 11 and afterward, to observe the eclipse.
‡ Hail at 11 A. M., lightning and thunder at 1 P. M.
§ Lightning at 11 P. M. ‖ At 10 high wind with lightning—sultry hot.
¶ In the afternoon tremendous thunder and lightning with heavy rain.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1809.

ARTICLE I.

On the Impregnation of the Seed, and first Shooting of the Nerve of Life, in the Embryo of Plants. In a Letter from A. IBBETSON, *Esq.*

SIR,

FOR many years botany and the study of the anatomy of plants have been my favourite occupation in solitude, nor had I any intention to subject that, which was undertaken only as a recreation, to the notice of the public: but some curious details having occurred, which appear to me not well known, if you think them worthy a place in your excellent Journal, they are at your service.

Difficulties in the study.

The very exact description that has been given by many intelligent botanists of the growth of the infant plant, from the time the seed is ripe for the Earth, renders it unnecessary for any one to repeat, what has been so well detailed; but there are curious particulars, *preceding* this time, of which little is said, and still less understood; which I have long

long made my particular study, though I have had to encounter difficulties not a little discouraging, and in the investigation of which such patience is required, as would deter the most laborious students; beside the necessity of a most powerful solar microscope for opake objects; to which is added, improvements not generally applied, and which causes it greatly to excel in clearness of vision.

Impregnation of the seed.

The investigation I mean is, "The impregnation of the seed; and the first shooting of the infant plant, or rather of the germe or vessel which precedes it." It is almost impossible to ascertain the exact time when the seed is first formed in the pericarp. I have always found them in the winter bud, where there is any large enough for dissection. It is most curious to see the vessels, which may properly be called the life, tracing their way to each flower bud; for a seed may be said to depend for perfection on *two separate moments:* the one in which life *first enters the seed,* when the whole outward form appears to be perfected; and the second, when the impregnation of the seed takes place, by the ripening of the pollen, as I shall hereafter show. But when the life enters, it leaves a little string, and afterward remains a long time in a torpid state. This string crosses the corculum, or heart of the seed, so called because it is the cradle of the infant plant.

Outward form of the seed.

Two distinct organs attaching the seed to the seed vessel.

The seed is attached to the seed vessel by two distinct organs, which the first botanists have agreed to call the umbilical cord; but I think they are improperly so named, since they do not convey the nourishment to the infant plant, which is wholly the office of the *second set of vessels.*

The first is, I conceive, the life of the plant, since without it the plant dies, and with it *uninjured,* every other part may by degrees be eradicated, and will grow again. I have tried the experiment on many thousands, and never failed. These delicate simple vessels, carrying a juice of a particular nature, are to be traced in every part, lying between the wood and the pith. Nature has plainly shown their consequence, by denying them to the leaf bud; (and what gardener would take the leaf bud to *bud with? None;* for it possesses *not the life)* but Providence by a sort of instinct most curious teaches it to pass by the leaf bud, and proceed

to the female flower, where it establishes a new life in the seed. This life will enable it to grow, but not give life again, without impregnation. These vessels are the life therefore, from which all flower branches grow, and all root threads proceed. In calling it so, I only express what its office seems to denote. Hill traced it exactly, and called it the circle of propagation.

The next organ, that attaches the seed to the seed vessel, consists of the nourishing vessels. I am rather inclined to think, that these proceed from the inner bark: at least they may certainly be traced thence after the infant plant has left the seed. When introduced they enter not the seed at the same place as the life does; they come not into the corculum, but pass it, and spread themselves over a small spot below it, which is visibly of a different nature from the rest of the seed. In farinaceous plants it is yellower, and yields a milk white juice; but in other seeds it is whiter, and gives a glutinous water of a sweetish taste. Probably the vessels come from the fruit *filled with this juice*, which medicated with that part of the seed (which very apparently dissolves) they together form a nourishment suited to the infant plant.

The nourishing vessels.

Juices of each seed.

When the seed is so far perfected, it remains in an almost torpid state, or growing very little; while the flower expands daily, and the stamens are hastily advancing to their perfect state. It is now that beautiful process takes place, which, by an almost imperceptible contraction of the lower part of the pistil, raises the juice to the pointal, whence it may be seen hanging in a large glutinous drop, but which never falls. As soon however as the heat of the mid-day ceases, this juice, which is peculiar to the pistil, retires again within the tube, the contraction ceasing with the heat that caused it. This is continued each day, till the stamens are ripe, and ready to give out their interior powder; the greater part of which the pistil is always so placed as to receive; and as the pollen requires only *moisture* to burst it, it soon yields that fine and imperceptible dust, which quickly *melting* and *mixing with the before-mentioned liquid*, forms a combination of so *powerful* and *stimulating* a quality, that it no sooner runs down the interior of the

Contraction of the pistil.

The rising of the drop in the pointal

 style,

style, and touches the nerve of life in the heart of the seed; but *this vessel shoots forth* in the most surprising degree, forming directly a species of *circular hook* within *the void*; which in less than two days is often completely filled, though it had perhaps lain for many weeks before in an absolute torpor. This circular nerve is soon covered by an excrescence that hides it; but if the corculum is divided with a fine lancet, the circular hook is discoverable, till the young plant is near leaving its cradle or seed. At the turn of the hook the cotyledons grow, and the root shoots from the curved end.

Fills the void when the stamens are ripe.

The plant may be now said to lie in the seed in a contrary direction from that in which it will at a future time grow, since the root is above, and the stem below: but Nature has provided for their change of place, since it is effected as they leave the seed. I have mentioned before, that the nourishment of the infant plant is medicated between the juice brought in the nourishing vessels, and the peculiar spot in the seed. This liquid continues to abound, indeed the infant plant may be said to *repose in it*, till the root has opened the whole, or part of the seed. The root then changes its direction, and runs into the earth, soon forming a number of stringy hairs, which serve as so many suckers to draw the liquid nourishment from the earth, while the plant quickly shows, by the rapid progress it makes, the advantage it receives from its change of diet; for it soon raises itself from its prostrate posture, emerges from the seed, and is now seen in its proper direction.

Change of posture in leaving the seed.

Nourishment of the plant.

Root strings pump up the nourishment.

I would not interrupt my account of the growth of the young plant, though my letter was written merely to detail the first steps, which are I believe unknown, but which confirm I think most thoroughly the sexual system, though some of the *syngenesian orders* give, if possible, a more convincing proof of it. The pistil runs up from the seed, being mostly single; and the juice of the pistil has no other way of reaching the pointal but passing through the seed, which it does without producing any effect, or filling up the vacancy at the top of the corculum. But no sooner does this same juice get mixed with the flower of the pollen, which dissolves in it, than the void becomes filled, the hook

Prove the sexual system.

hook is soon formed, and the young plant is raised to life.

They who doubt, that each part of the plant has its different juices, *proceeding from* and appertaining to the produce of one part alone; that is, the wood, when rising to the flowering part, gives its juice *only* to *form* the stamens; the line of life to form the pistil; the bark to form the corol, &c.; would no longer deny their assent, if they would dissect, and very much magnify the part of the pericarp just *above* and *below* the seeds, and see the extreme pains nature takes, that the juices may in no manner be mixed. I have drawings of almost every different *formed flower* in these parts, both *English* and *exotic;* and I think I could *prove* the truth of this assertion, without having recourse to the *rationalia* of the matter, which would certainly show the impossibility, that such parts, so different in their appearance, so opposite in their tendency, should grow from the same vessels, and proceed from the same juices. Nature gives us also a proof of the confusion occasioned by the mixture of the juices in the double flower, which owes its deformity probably to this cause only; as I have always found, on *dissecting* and *comparing* double and *single flowers of the same species together*, that, when it is the pistil that fails, the *style* is *discovered* to be *burst the whole way*, so that the juices can neither pass to the stigma for impregnation, nor return again to the seed: but when the stamens are imperfect, the seeds are often found in the pericarp, but they never have the void in the corculum *filled up*; and I have often seen the inner vessel of the style hanging like a useless thread in the middle of the seed vessel, and a confusion visible in every part, which seems to prove a general mixture of the juices, from the *excess* of *nourishment bursting* the *delicate* fibres, that contained each peculiar liquid.

Peculiar juices appropriate to each part.

Double flowers owing to too much nourishment bursting the finer vessel, and mixing the juices, thus causing monsters.

I meant not however to enter into this digression, as it is a subject that requires many drawings to elucidate it, and more reasonings than a short paragraph will admit of. I return therefore to the infant plant, and shall venture to add a few of the innumerable experiments made to prove whether this cord of life (or as it is generally called *umbilical*

The loss of the cotyledons does not kill a plant.

cord) is or is not the life of the plant. I placed a bean in the earth, and when the infant plant was ready to leave the seed; I opened it with a fine lancet, and cut off the cotyledons, just where they join the heart and the circular hook I have before described. Tying a piece of thread, easy to be broken, 'round the bean, I replaced it in the earth. The cotyledons grew again, though higher up, but they appeared very weak and sickly for some time.

The loss of the root does not kill it.

I then placed another bean in the earth, and at the same age I cut off the root. In a few days it grew again, and appeared perfectly healthy.

Throws out hairs to convey nourishment.

To see what the effect of taking away only the nourishing vessels would be, I separated and cut them off from each side of the bean; but the quantity of hairs, that grew from the wounded part, and attained the moisture to convey the nourishment, and supply the place of the part I cut away, is almost incredible.

Invariably dies with the loss of the nerve of life.

I now took a bean about four days in the earth, and opening it with great care, I took out with a fine lancet the part which I esteem the cord of life (See Pl. V, fig. 1, *ll*), that is, the part which crossed the corculum, and shot forth on the first impregnation of the plant. The whole decayed. I repeated this more than a dozen times, the plant always died.

I took a flower of the lilium species, as having a large seed vessel easily attained; and, being careful not to separate it from the nourishing vessels, I divided the line of life, cutting each thread between the seeds. Its seeds were never impregnated.

Infant plant killed by taking out the line of life.

I now tried the taking the nerve of life from the chesnut, the walnut, acorn, &c., first opening a seed *without touching* the nerve, that I might be assured that the opening was not the cause of its death. Those from which I took the nerve, all died; and the others, that I had merely laid open, lived. It is only at the first beginning of life, that the plant is to be killed by this process; when older, if the nerves decay, they shoot out above the declining part, and run into any part of the stem that is pure, to preserve themselves. This is the source of life in very decayed trees.

Source of life in decayed trees.

This is the cause of a double pith, or at least of the appearance

ance of it, in many trees. This also in many grasses has a very particular appearance. I once found in the spring four yards of the poa trivialis with a root now and then, the whole dead; but on farther examining the plant, the end farthest removed from the root was beginning to shoot. On subjecting it to the solar microscope, I found the nerve of life had run in one diminutive string of vessels finer than a hair, of a bright green, and defended from the inclemency of the weather by the deadened part. As soon as the mildness of the season permitted, it shot forth; the rest of the parts were added by degrees, and the decayed fell off. Double pith. Poa trivialis.

I have many curious specimens of stems in which the vessels of life have been turned out of their natural situation: but it requires so many drawings to give a perfect idea of them, that of course such a work as yours could not admit them. I once traced these vessels from the stem to the apple, and thence to the line in the seed in one string; but this is extremely difficult to be done. Vessels of life turned out of their natural situation.

I shall now conclude with noticing two extraordinary proofs of volition in some plants difficult to be accounted for by mechanical force only. I divided a bean into two pieces, and planted that half in which the young plant is found. In five days the stem had forced itself out at the usual place, but the root had taken a *shorter road*, and come out at the truncated part as more immediate to the earth. What mechanical power could occasion this difference? I took a bean in health, that had just quitted the seed, and cut off the root. The nourishing vessels had been dried up a day or two. I wrapped the truncated part in paper, fearing that it would throw out hairs to nourish itself, and then replaced it in the ground. How great was my astonishment to find, not only that the bean lived, but that the nourishing vessels had *reassumed their office* of supporting the plant! that the bean, which had been perfectly dry, was now as moist as in its earliest state, and continued to support the plant till the root had again grown, and forced itself through the paper! I have ever been an advocate for mechanical power, but can scarce reconcile these two instances to such a cause. Proofs of volition in plants. Nourishing vessels regain their office.

The various names given to the infant plant and its different

ferent parts have made me very unwilling to fix on an appellation, till it is ascertained what *are its parts and their uses*; as I cannot but imagine, that so many various appellations have the effect of making those that write unintelligible to one another, and much more so to those, who wish for information without much previous study. I shall add a little account of the names used to the sketch annexed, which will, I hope, make the parts easy to be comprehended.

Your obliged servant,

Bellevue, near Exeter. A. IBBETSON.

Explanation of Plate V.

Fig. 1. Representation of the bean. *o o* the nourishing vessels. *L* to *n* the seminal leaves, or cotyledons.

l to *l* the embryo: what I esteem the first shoot which the nerve of life makes, when it enters the corculum, or heart, which is more easily seen in the seed of the lily as at fig. 2, *l l*, where it crosses the empty part of the corculum as before explained.

When I took out the line of life in the bean, it was the two vessels within, from *l* to *l*. When in the lily, fig. 3, I merely divided the line *l*, preventing that communication from seed to seed, and not touching *o o*, which I think is the nourishing vessel, as may be seen at fig. 2, *o*, where they enter. Fig. 4 is the seed of the gooseberry, *o* the nourishing vessels, *l* the line of life, and *m* the corculum, or heart. Fig. 5 is the heart taken out of the seed of a chesnut. *l* is the circular hook, *o o* the nourishiug vessels, and *l l* the line of life, which I took out where it crosses the heart at *m*. In almost every kind of seed it shows itself differently. In many it enters at or near the stalk, and runs under the albumen, or outward case. Having much more studied nature than botanical works; which indeed I began with, till I found that they inclined me to embrace a system, which I wished much to avoid; I have since trusted to nature only. I hope therefore to be excused the contradicting any one, as I may truly say I have not advanced a thing I have not tried

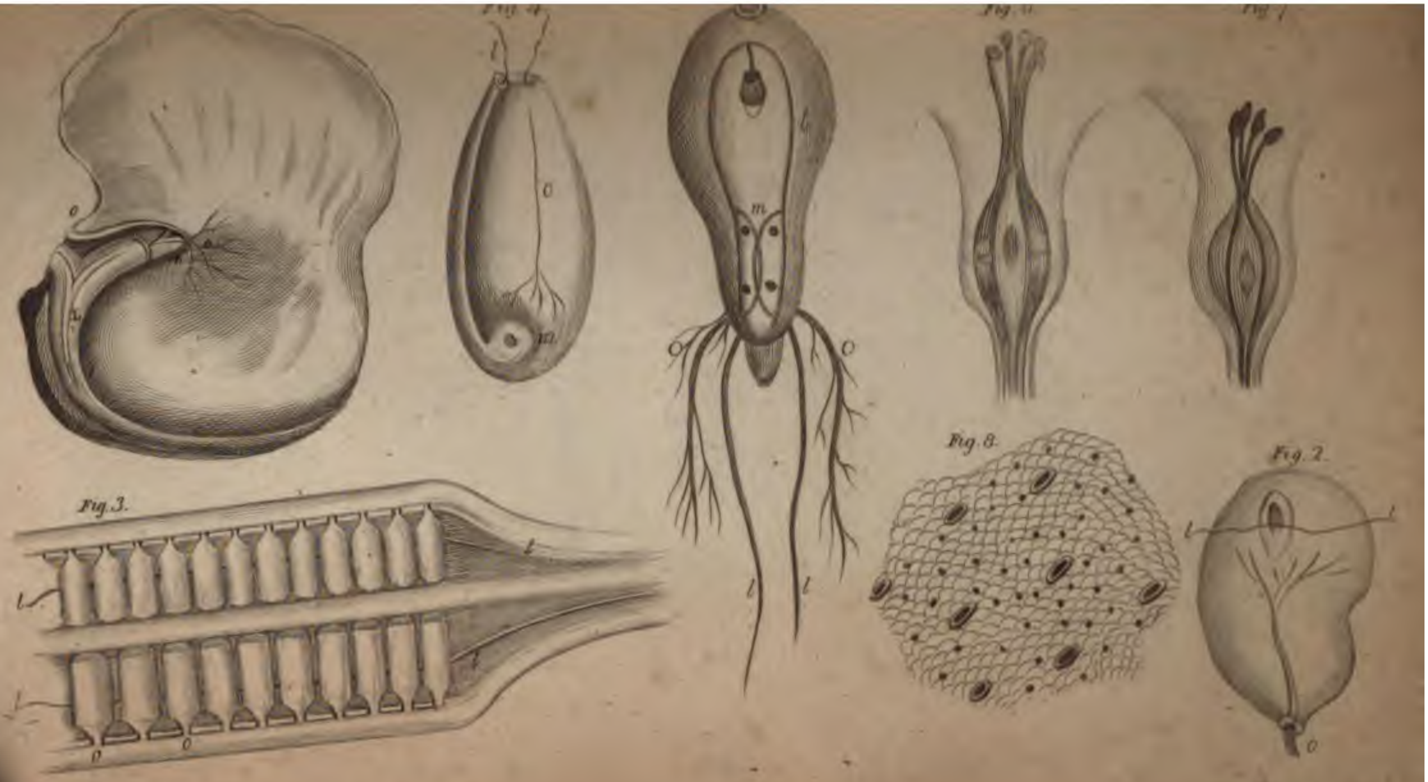


Fig. 3.
Fig. 8.
Fig. 2.

tried a number of times. Yet I am but too sensible how open I am to deception, and it is with a real feeling of humility I offer these opinions.

That the life of the plant is peculiarly resident in the vessels that run in a circular collection between the pith and wood, or medulla and liber, is most strongly proved by the manner in which all fruit is killed, if examined the morning after a sudden frost. It is *not* the *corolla*, the *calyx*, the *males*, or the *seeds* that are hurt; but the *female* is struck with *death*. And if the pistil is examined with care, it will be found, that it is the *line of life* which is decayed, and that this is the first part in which mortification commences. The peculiar liquor of the pistil turns to a blood red, and the vessels that run up to the pointal turn black. I have marked one at fig. 7 just taken from the tree, and killed in the last frost. The dark lines in fig. 7, which is dead, show the black and red vessels mentioned above; these being yellow in their natural state, which is delineated at fig. 6.

It is almost unnecessary to mention, that seeds must be examined in their first formation, to show the line of life; which, when once it has done its office, detaches itself. If the seed is boiled, the line of life and nourishing vessels will mark themselves by turning a dark colour. In very small seeds the mouth is often the best dissecter.

II.

On the Perspiration of Plants. By A. IBBETSON, *Esq.*

SIR,

AS my first paper is short, I shall venture to join to it an incident, that has surprised me not a little, and that may perhaps from its novelty be acceptable to your readers. I have long entertained great doubts respecting the evaporation of plants; I mean not that insensible perspiration that will show itself by throwing a mist on the glass that covers it; but that which Bonnet insists on, and which Du Hamel weighed (which in 24 hours was double and treble the

Perspiration of plants.

the weight of the plant, even in a *sunflower*, which is *the heaviest* of plants); and my experiments have so fully answered my ideas respecting it, and *confirmed my doubts*, without however throwing the least blame on the very perfect experiments of these excellent botanists, that I shall have the greatest pleasure in offering you the result.

Doubts respecting it.

The constant habit of watching my plants at a very early hour in the morning, and examining them with very powerful microscopes, had almost convinced me, that the idea of their perspiring was a mistake; still, being acknowledged by such excellent botanists, it required the most *absolute* conviction, to gain courage to deny a fact so universally received as a truth. I rise at a very early hour, and had often observed, that, when there was no dew, the leaves remained perfectly dry, though examined with a powerful microscope; that when plants remained within doors, they collected dust like any other furniture; and that this dust was to be blown off with ease, neither agglutinating nor sticking, which it would do if partially wet: that, after placing a leaf for 4 hours in the opake solar microscope, though it was so placed as to be in its growing state, and was magnified so greatly as to show both species of pores, yet I could never see the smallest quantity of moisture exude, except what I shall now mention, and what I suppose may be the insensible perspiration before insisted on.

Insensible perspiration.

Almost every leaf, if subjected to a large magnifier, appears covered with a very fine scurf, which I have seen exude as water with the oxigen it is continually giving out, as long as the sun shines. In a very short time it turns to a jelly; which is, *I think*, received again into the same pores with the dews of the night; and which I doubt not helps to form that beautiful combination, which changes dead and unorganised matter into living bodies, fitted, as Mirbel beautifully expresses it, for the support of the animal creation. But this is so trifling a perspiration, that it will merely account for the dew, that appears when a vegetable is placed under a glass; but will not raise, or in a very slight degree only, the hygrometer placed within it.

Taken back.

This very trifling.

These doubts suggested the idea of investigating the matter more thoroughly, and I set on foot a number of experiments

periments, which I shall now detail, prefacing them with an observation which is necessary to begin with, because it is one of the signs given of perspiration, which I cannot assent to. Hales and Bonnet both observe, that, having placed a plant under a glass, the water after a time ran down or bedewed the glass. Put a wet sponge under a cylinder, and it will produce the same effect; and yet we should not say, that the sponge perspired, but that some of the moisture within the sponge had evaporated, and was condensed by the cold of the glass. In short it is merely a sign, that the *object* thus confined is *full of moisture*. False sign of perspiration.

I shall now mention the experiments in the order in which I made them. I wished first to prove, which yielded most moisture, the *earth* or *plants*. I placed a small rose tree under a large glass in a pot of earth, placing at the same time Captain Kater's excellent hygrometer * with it, which then stood at 620 from excess of dryness. In 8 hours the moisture ran down the glass, and the hygrometer was at 1100, nearly excess of moisture. I then took away the rose tree, and, drying the glass, I put a pot of fresh earth the same size and weight, and with the same arrangement. In 8 hours the hygrometer, which had been put in at 616, came out 1049, or 433 more moist than it was when placed there. It was the *earth* therefore, that gave all that excess of moisture, not the plant. Experiments on the rose tree; compared with fresh earth:

The next trial was made by fastening down a laurel branch, and passing it through a piece of sheet lead, without separating it from the tree; making it to fit a very large glass cylinder, then luting it round the lead, and at its entrance, to keep out the circulation of air, and prevent the wet vapour from passing upwards. After 8 hours the hygrometer was 130 nearer to dryness than when placed there, and though the glass was steamed, it did not run down with water; nor could I, with the largest magnifiers, *discover any dew drops* on the leaves. on a laurel branch:

I now tried a vast number of plants with the same result, the hygrometer *never* showed an *increase*, which it would certainly have done, had the perspiration been so excessive; and on various plants.

* For a description of this hygrometer, see our present number.

and

and it must have been perceptible on the leaves, but this was so far from the case, that the scarf before mentioned was not to be seen; it had certainly all settled in dew within the circumference of the glass.

Perspiration not perceptible.

I felt now perfect conviction, though not able to account for the *mistaken opinion* that prevailed, till walking one morning with my microscope in my hand, I found a pea plant covered with bubbles of water, and there had certainly been no dew. Here then was perspiration. I directly wiped off the drops, and covering the plant with a glass, treated it in the same manner as I had done the laurel, and many hundred of other plants. In a few hours it was again covered with bubbles of water, and the hygrometer indicating *extreme moisture*. I then tried a number of the same *genus*, but without effect, no *bubbles* were to be *seen*. I now concluded, that *some vegetables* did perspire, but that the numbers were *few*.

Perspiration of the pea.

Tried others of the genus but without effect.

Talking to a friend of the conviction I had gained, he intreated me to repeat a part of the experiments before him; I consented; and having first prepared the pea, in an hour or two it was covered with bubbles; but my friend not being yet arrived, I cut off the branch, and laid it on the table by me, fearful the bubbles would evaporate in the open air. In an hour I was surprised to see them turn of a milk white. I then applied to my solar microscope, and soon found, that the bubbles I had taken for *water* were a cryptogamian plant, having a regular stalk, which did not however raise it from the leaf, for it was so heavy it appeared *incapable* of *rising*. It lies like a long bubble, dies in a few hours, and is soon succeeded by a fresh set.

The supposed bubbles of water a cryptogamian plant.

No person could in its first state take it for any thing but water; indeed so completely did the bubbles resemble water, that the smallest touch broke the film which covered them, and their liquor was expended. Nor would any one believe it was not water, without seeing the stalk on which it grew, or without beholding its change of form. Its last state is an almost hard and long ball, which soon drops off. It is to be seen by a common little microscope; though stronger powers are required to view the whole process, especially the stalks. But so entirely does it cover the leaves, that

This plant described.

that it doubles the weight of the plant, causes the hygrometer to indicate extreme moisture, and, confined under a glass, much of its liquor evaporates, condenses on the interior of the glass, and runs down on every side. I have since tried every plant specified as peculiar for their excessive perspiration by Bonnet, Hales, and others, and have found them all loaded with the cryptogamian plant, so that I have not the smallest doubt, that this was by them taken for perspiration; for what torrents of water would be necessary to supply such a *transpiration?* the air would be constantly loaded. The possibility of the mistake any person may convince themselves of, and how very likely it was to happen, by taking a pea plant, a sunflower, and a number of other plants unnecessary to mention.

Its liquor condenses on the glass, and has been mistaken for perspiration.

I said, that leaves had *two species of pores*; the first *large*, which are open all the night for the admission of the dew; the second *small*, from which the oxigen flows. See Pl. V, fig. 8, representing part of a leaf sufficiently magnified, to show both sorts of pores. It is from the *smaller* that the jelly I have mentioned proceeds; for when the oxigen is saturated with moisture, it will naturally give it out in passing these narrow apertures, and this is that scurf which appears, when the leaves are not covered with a glass; but which flies upward, and is condensed on the interior, when they are.

Leaves have two kinds of pores.

I believe almost every air or gas has moisture, and that a full stream of oxigen directed against a glass would cover it with *steam*. I have just tried the experiment, and it has succeeded. It will of course depend upon its being nearly saturated with moisture, or not; and upon the pressure it afterward receives. I have endeavoured to condense my subject as much as possible, without I hope rendering it unintelligible. Should I see this in your first publication, it will serve as a hint to give you a farther letter on the formation of the leaf, and the winter bud. The latter certainly is of the first consequence in botany, and may be called the first source of life in the vegetable world.

Effect of a stream of oxigen.

Your obliged servant,

A. IBBETSON.

III.

III.

On the Analysis of Sulphate of Barytes. By Mr. James Thomson. *Communicated by the Author.*

Sulphate of barytes not yet accurately ascertained.

THE analysis of sulphate of barytes has engaged the attention of many distinguished chemists; yet the problem, though of easy solution, may be considered as still unresolved, since the greatest discordance prevails in their results. The accurate determination of the relative proportions of its constituents, as far as concerns its chemical or mineralogical history, is a matter of secondary consideration; but the soluble combinations of barytes being themselves important instruments of analysis, in detecting the presence and ascertaining the quantities of sulphuric acid in any compound by the production of sulphate of barytes; the analysis of sulphate of barytes itself becomes an object of considerable importance, and involves in it the accuracy of the analysis of almost all compounds, into which sulphur or sulphuric acid enters.

This an important object.

Its composition according to various authors.

Withering, Black, and Klaproth, who have examined the composition of this salt, agree with Kirwan in stating it as composed of sulphuric acid 33, barytes 67.

According to Fourcroy it is composed of acid 34, barytes 66.

According to Thenard of acid 25·18, barytes 74·82.

According to Berthollet of acid 27, barytes 73.

And according to the experiments of Chenevix, of acid 23·5, barytes 76·5.

Clement and Desormes, in consequence of the discordance of these results, engaged in a series of experiments, which appear to have been conducted with great care; and from which they conclude, that sulphate of barytes is composed of acid 32·18, barytes 67·82.

And Klaproth once more revised and confirmed his former analysis, which gave 33 acid and 67 barytes, as the composition of this salt.

The labours of these distinguished chemists, together with the general accordance of their results with those obtained

tained by Richter, Bucholz, Clayfield, and others I have not particularly quoted, might have been supposed decisive of the question; yet in a memoir on the composition of alum, subsequent to that of Clement and Desormes on the barytic salts, and posterior also to the last experiments of Klaproth, Messrs. Thenard and Roard have adopted the proportion of 26 per cent of sulphuric acid in sulphate of barytes, as the mean of the results obtained by one of them, and those of Berthollet, after experiments conducted with the greatest care.

Occasion of the present paper.

The question remaining still therefore undecided, and having myself engaged in a series of experiments on the constitution and properties of the principal mordants employed in dyeing and calico printing, in which I had frequent occasion to ascertain the presence and quantities of various sulphuric salts, I was under the necessity of satisfying myself respecting the composition of sulphate of barytes by direct experiments, the particulars of which form the subject of this paper.

Comparative results of former analyses.

On comparing the results of the different experiments on this subject, it will be seen, that, with the exception of those of Thenard, Berthollet, and Chenevix, they all agree in stating the proportion of acid between 31 and 34 per cent; the mean of the whole, and by far the greater number, making it about 33. Klaproth, Clement and Desormes, and others, have deduced the composition of sulphate of barytes, from that of the carbonate and nitrate; and as this mode appeared to me at once simple and unobjectionable, I followed it in the first instance exactly.

Carbonate of Barytes.

Carbonate of barytes dissolved in muriatic acid.

One hundred grains of carbonate of barytes were dissolved in dilute muriatic acid, with all the precautions necessary to prevent the dissipation of the solution, or loss from too rapid disengagement of the carbonic acid. When the effervescence ceased, the last portions of gas were expelled by a momentary exposure to heat. The loss amounted to 21·65 grains. The experiment repeated on 50 grains of the carbonate gave 10·85 grains, or 21·7 per cent;

cent; and a third experiment 21·85 grains. The mean of these results gives the proportion of carbonic acid in 100 of carbonate of barytes as 21·75 grains, a quantity which differs only ¼ of a grain from that obtained by Klaproth, or Clement and Desormes, who make it 22 per cent.

The solution precipitated by carbonate of ammonia.

2. The muriatic solution, containing 100 grains of carbonate of barytes, was precipitated by carbonate of ammonia. The precipitate, well washed and dried at a heat below ignition, weighed 100·2 grains.

The artificial carbonate similar to the native.

3. One hundred grains of artificial carbonate of barytes, precipitated from very pure muriate of barytes by carbonate of ammonia, and dried at a temperature somewhat below ignition, were redissolved in dilute muriatic acid, and the loss of weight carefully ascertained. The experiment repeated afforded the same result as the preceding with the native carbonate, establishing the identity of the two combinations, and proving, that carbonate of barytes both native and artificial is composed of

Carbonic acid	21·75
Barytes	78·25
	100

Nitrate of Barytes.

Carbonate of barytes dissolved in nitrous acid.

One hundred grains of carbonate of barytes, dissolved in nitrous acid, and gradually evaporated to dryness, afforded 132 grains of nitrate of barytes. The experiment repeated on larger quantities, with a view to the preparation of this salt for the purposes of analysis, gave precisely the same results. One hundred and thirty-two grains of nitrate of barytes therefore contain 78·25 grains of barytes, the quantity contained in 100 of the carbonate; and 100 parts of the nitrate are composed of

Composition of the nitrate.

59·3 barytes,
40·7 acid and water.

100

Clement and Desormes obtained 130 grains of nitrate of barytes only from 100 of the carbonate, which gives for the composition of nitrate of barytes, 60 barytes, 40 acid and water.

water. It is here our experiments chiefly disagree; but the difference does not amount to one per cent, and more perfect accordance will hardly be expected by those, who are in the habit of making such experiments.

Sulphate of Barytes.

One hundred grains of carbonate of barytes were dissolved in muriatic acid, in a platina crucible, and precipitated by sulphuric acid. After slow and careful evaporation to dryness, the crucible was exposed to a white heat during half an hour, and afterwards weighed. The calcined sulphate of barytes amounted to 116·8 grains. Muriatic solution precipitated by sulphuric acid.

2. One hundred grains of nitrate of barytes were decomposed by solution of sulphate of soda added in excess, and the mixture gently heated. The precipitate well washed, dried, and calcined, weighed 88·6 grains. Nitrate of barytes decomposed by sulphate of soda.

Now 100 grains of carbonate of barytes contain 78·25 grains of barytes, and produce 116.8 grains of calcined sulphate of barytes; Composition of the sulphate.

And 100 grains of nitrate of barytes, containing 59·3 grains of barytes, produce 88·6 of sulphate;

From which it follows, that sulphate of barytes is composed of

Sulphuric acid	33·04
Barytes	66·96
	100

The results of the preceding experiments, every one of which was carefully repeated three or four times, and their perfect accordance with those of Withering, Klaproth, and others I have already quoted, left no doubt of their accuracy on my mind. The cause of the disagreement between eminent chymists sought.

Aware however, that no individual authority, however respectable, can add to or detract from the confidence which the names of Thenard, Berthollet, and Chenevix inspire; and sensible that my single testimony added to the rest would weigh but little in the scale against them; I was desirous, if possible, of detecting the source of this discordance in their experiments, as the surest and only

Thenard's mode of ascertaining the composition imperfectly given.

means of finally deciding the question. In the extract, which Guyton has given of the memoir of Thenard on the different states of antimony, in the 32d volume of the Annales de Chimie, the mode in which he ascertained the composition of sulphate of barytes is not stated with sufficient minuteness, to enable any one to repeat his experiment. One hundred grains of pure barytes, fused in a crucible, are stated to have afforded 133·3 grains of calcined sulphate of barytes; but whether by direct combination, which would be liable to errour, or through the medium of some other solvent, is not mentioned. Nor is the mode by which the pure barytes was obtained noticed in Guyton's extract, though of the utmost importance in this inquiry. The experiment indeed does not appear to have been made in a way favourable to accuracy and precision, though for want of sufficient details it is not possible satisfactorily to point out the sources of errour. The experiments of Berthollet, which determined the propertion of acid in sulphate of barytes at 27 per cent, I am wholly unacquainted with; nor do I know the mode which this celebrated chemist pursued in making them; which I regret the more, as they are stated to have been conducted with scrupulous exactness.

Berthollet's experiments not given.

Mr. Chenevix more particular.

Mr. Chenevix's paper in the Memoirs of the Irish Academy however contains all the details necessary for the examination of his experiments, and fortunately also furnishes additional proofs of the accuracy of my own results.

His process.

To ascertain the quantity of sulphuric acid in sulphate of barytes, Mr. Chenevix decomposed a given weight of sulphate of lime (the composition of which he had ascertained by previous experiments); and having found the quantity of sulphate of barytes, which it afforded, the proportion of sulphuric acid in the latter was readily deduced. "Upon 100 grains of calcined sulphate of lime," says Mr. Chenevix, "I poured some oxalic acid, which attracts the basis with an affinity superior to that exercised by sulphuric acid. Oxalate of lime was here formed, but oxalate of lime is soluble in a very small excess of any acid. A little muriatic acid operated a complete solution, and thus

thus a great quantity of sulphate of lime required but little water to dissolve it. Into the liquor muriate of barytes was poured, and suffered to remain some time gently heated; by these means any oxalate of barytes, that might have been formed, was retained in solution by the original excess of acid, and the entire quantity of sulphate of barytes was deposited. Of the exactness of all those methods, which I used as the instruments by which I ascertained these results, I convinced myself by various preliminary experiments. After the usual filtration, washing, and drying at the gentle heat of a sand bath, I obtained in one experiment 185, in another 183, and lastly in another 180. We may therefore take 183 as the mean proportion. Consequently we shall say, 183 grains of sulphate of barytes contain the same quantity of sulphuric acid, as 100 of sulphate of lime (43); and 183 : 43 :: 100 : 23·5. Therefore 23·5 are the proportion of sulphuric acid in 100 of sulphate of barytes."

This experiment repeated.

I repeated this experiment of Mr. Chenevix with calcined sulphate of lime carefully prepared, and obtained from 100 grains, as he had done, 180·5 grains of sulphate of barytes dried at the heat of a sand bath. Suspecting, however, that the various and complicated affinities, which are brought into play in this process, might be productive of some errour; and that the mode was defective, though the results were correctly given; I dissolved 10 grains of calcined sulphate of lime in a pint of boiling distilled water, and poured in muriatic of barytes. The precipitate, washed, dried, and calcined, weighed 17·7 grains. This accorded so nearly with the experiment of Mr. Chenevix, that I was satisfied of the exactness of his method, and that it was not here I was to look for the source of the discordance. His analysis of sulphate of lime I had not verified, having an indistinct recollection of its agreeing nearly with the composition of this salt as stated by others. On a more attentive examination however I found, that the proportions, as given by Mr. Chevenix *, are the converse of those of Klaproth;

The sulphate of lime dissolved in distilled water.

The proportions of acid and base in the sulphate of lime the cause of the difference.

* Dr. Thompson, in his excellent System of Chemistry, vol. II, p. 855, 2d edition, has, by a very natural mistake in quoting from the

Klaproth; the former making it contain 57 parts of lime and 43 acid in a hundred, and the latter 57 acid and 43 lime nearly. I at first imagined this was a typographical, or perhaps an arithmetical errour; but this is not the case: 100 parts of pure lime afforded Mr. Chenevix 176 grains of calcined sulphate, which gives the proportions exactly as stated in his memoir. Here then evidently hinges the difference in Mr. Chenevix's analysis of sulphate of barytes compared with mine and others; it remained therefore to ascertain, which of the two analyses of sulphate of lime was to be relied on; that which makes the proportion of acid 43 per cent, or that which makes it amount to 57.

These therefore investigated.

Lime dissolved in muriatic acid and precipitated by sulphuric.

1. I dissolved 100 grains of pure lime, prepared as Mr. Chenevix has directed, in muriatic acid in a platina crucible; and, after precipitating with sulphuric acid, evaporated the mass slowly to dryness. The crucible was then exposed during an hour to a white heat. The calcined sulphate of lime weighed 240 grains.

Carbonate of lime dissolved in acetic acid and precipitated by sulphuric.

2. Fifty grains of pure carbonate of lime were dissolved in acetic acid, and sulphuric acid added in excess. The mass, after slow and careful evaporation to dryness, was exposed to a white heat near an hour, and afforded 67·3 grains of sulphate of lime.

Proportions according to these experiments.

The first experiment, in which 100 grains of pure lime afforded 240 of calcined sulphate, gives for the composition of the latter 58·34 acid, and 41·66 lime. The second, if we admit with Dr. Marcet, that carbonate of lime contains 44 per cent of carbonic acid, gives for the composition of sulphate of lime, acid 59, lime 41, which are exactly the proportions of Kirwan. I feel disposed however to place greater confidence in the first result; the experiment was several times repeated, and I think, if we state the proportions in sulphate of lime as 58 acid and 42 lime, we shall not be far from the truth.

These confirm the analysis of

Now Mr. Chenevix found, that 100 parts of calcined

Phil. Mag. vol. XI, p. 115, the proportions of acid and base, as given by Mr. Chenevix in his analysis of sulphate of lime, and thus restored them to accuracy. This errour has been copied into a work of very inferior merit, the "Chimie appliquée aux Arts" of Chaptal.

sulphate

sulphate of lime afforded 183 grains of sulphate of barytes dried at the gentle heat of a sand bath; but the sulphate of barytes dried at this heat contains still near 3 per cent of water, which deducted leaves 178·5 grains. If we say therefore, that 178·5 grains of sulphate of barytes contain the same quantity of sulphuric acid as 100 grains of sulphate of lime, and that 100 grains of sulphate of lime contain 58 sulphuric acid; we have for the composition of sulphate of barytes, sulphuric acid 32·5, barytes 67·5; which differs only half a grain per cent from what I have myself obtained.

Sulphate of barytes before given.

Still farther to confirm the preceding results, I made the following experiments. Into a solution of nitrate of barytes I poured 100 grains of sulphuric acid (the spec. grav. of which I omitted to note). Care was taken to have an excess of nitrate of barytes, and the solution was slowly evaporated down to dryness. The precipitate carefully washed from the remaining nitrate, dried, and calcined, weighed 231 grains.

Further confirmations of it.

An equal weight of the same sulphuric acid was poured into a solution of acetate of lime, in which the latter was in excess. After gradual evaporation to dryness, the acetate of lime was separated by repeated washing with alcohol, and the sulphate of lime dried and calcined. It weighed 133 grains.

Lastly, 100 grains of sulphuric acid were poured into a solution of acetate of lead in excess, and the precipitate carefully separated, washed, and dried. It weighed 296 grains.

From these experiments it appears, that 231 grains of sulphate of barytes, 133 grains of sulphate of lime, and 296 grains of sulphate of lead, contain equal quantities of sulphuric acid; and if in estimating the real quantities of acid they contain, we adopt Klaproth's analysis of sulphate of lead as the standard, to which to refer them, we shall have

Results of these experiments.

296 grains of sulphate of lead, containing 78·4 grains of sulphate of acid, or 26·5 per cent;

231 grains of sulphate of barytes, containing 78·4 grains of sulphate of acid, or 33·9 per cent;

133 grains

133 grains of sulphate of lime, containing 78·4 grains of sulphuric acid, or 58·6 per cent.

These results, though not in perfect accordance with those I had previously obtained, I considered as sufficiently exact to establish their general accuracy; and I did not think it necessary to verify them by more careful repetition, in which it is possible these slight differences might have wholly disappeared.

General conclusions.

The experiments detailed in this paper then confirm, with trifling variation, the results already obtained by Withering, Klaproth, Kirwan, Clement and Desormes, and others; and prove,

1. That carbonate of barytes, both native and artificial, is composed of carbonic acid 21·75, barytes 78·25.

2. That nitrate of barytes is composed of acid and water 40·7, barytes 59·3.

3. That calcined sulphate of lime contains sulphuric acid 58, lime 42.

4. And lastly, that calcined sulphate of barytes is composed of sulphuric acid 33, barytes 67.

Church Bridge, near Blackburn.

IV.

Experiments on the Expansion of moist Air raised to the boiling Temperature. In a Letter from JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR,

Objections to the new doctrine of the constitution of the atmosphere,

PERHAPS you will recollect, that I proposed some time ago in your Journal* various objections to the new doctrine respecting the Constitution of the Atmosphere, and the independent equilibrium of its component gasses. The intention of these objections was to invalidate the hypothesis, by showing its inability to explain natural phenomena; and at the same time to point out certain palpable absurdi-

* Vol. XVI, p. 4.

ties,

ties, which are necessary consequences of this novelty in meteorology. This method of examining the subject led me to use arguments, and to avoid experiments made by myself, as much as possible. The choice was suggested by common prudence; for any person can form a correct judgment of a syllogism; the value of which does not depend on the character of the logician, but on qualities that are apparent, and constitute its intrinsic merits or imperfections. On the contrary when an experiment is described, we have no right to expect the reader will assent to the truth of it, until he is convinced of the experimenter's abilities, and of his candour too; which is very liable to suspicion in the course of a controversy.

Supported by arguments preferably.

Experiments in certain cases less convincing.

The preceding reasons determined me at the time to defer the experimental part of the refutation to a future opportunity, in hopes, that some other person would undertake the task; but the silence of both parties has hitherto disappointed this expectation, and it almost obliges me to publish certain experiments in my possession; which in all probability will place the controverted point in a clearer light. If air and water be confined by a pellet of mercury in a glass tube, closed at one end, and the apparatus be afterward raised to the boiling temperature, the new hypothesis maintains, that the vapour of the water will make its way through the pores of the permanent gasses, and counteract the pressure of the atmosphere on the pellet of mercury, thereby leaving the included air at liberty to expand indefinitely. The practical method of showing the truth of this proposition by the manometer never appeared satisfactory to me, in consequence of which I undertook to have the experiments repeated in the following manner.

Reason for recurring to e at present.

Exp. 1. Barometer 30·06, a tube one twelfth of an inch in bore, and containing a quantity of water in the sealed end, measured $6\frac{1}{4}$ inches from the surface of the water to the open end. A column of air $\frac{4}{10}$ of an inch in length, or something more than $\frac{1}{20}$ of the open space $6\frac{1}{4}$ inches, was confined in contact with the water in the tube by a column of mercury, $\frac{1}{4}$ of an inch long, the temperature of the instrument being 46°. The open end of the manometer was then fixed into the neck of a narrow bottle by means of a perforated

Exp. 1.

Exp. 1. perforated cork, which was made watertight; and the edge of this end projected about ½ a line above that extremity of the cork which entered the bottle, so that the sealed end of the tube, which was out of the bottle, fell 5½ inches below the neck when the bottom was turned upwards. Things being thus prepared, the bottom of the phial was cut away to open a free communication betwixt the atmosphere and the orifice of the manometer. A strong wire was then tied round the bottle, by which it was kept in an oblique position in a large pan of water, so that the open end of the manometer was 3 inches below the surface. At the same time the interposition of the cork and bottle preserved this aperture dry and exposed to the air. The intention of the preceding arrangement scarcely requires an explanation, for it is evident, that, if the pan were made to boil, the tube would receive all the heat which the water could communicate to it, and the size of the boiling vessel was such, as to permit the manometer to be suspended in it free of the sides and bottom, which is a necessary precaution. Lastly, the oblique position of the tube gave the pellet an opportunity to roll over the edge of the orifice, after which it would remain on the cork, provided the spring of the air proved sufficient to expel it. In order to find if this would really be the case, the pan was gradually heated from 46° to boiling, with the manometer suspended in it: and after the water had continued to boil a few minutes, the instrument was taken out of the pan; upon which the mercury was seen to descend quickly towards the sealed end of the tube. According to this experiment the gas or gasses of the manometer were limited in expansion under the pressure of 30·185 inches of mercury to twenty times their original bulk at most. Now the advocates of the new hypothesis say, that the vapour alone sustained 30·06 of this force, or the barometrical pressure. Consequently the dilated air supported nothing more than the weight of the mercurial stopple, or ⅛ of an inch of mercury. But air rarified 20 times will sustain more than 1½ inch of mercury, when the barometer stands at 30·06; neglecting the increased elasticity, which was occasioned in the present instance by raising

The barome- the pan and its contents from 46° to 212°. May not we safely

safely conclude then from this experiment, that the barometrical pressure is not counteracted by free vapour, which certainly would be the case, were the hypothesis in question consonant with the operations of nature?

Barometrical pressure not counteracted by free vapour.

After ascertaining the preceding fact, I was desirous to approximate with a greater degree of exactness to the limit of the expansion, if a proper instrument could be procured. I say a proper instrument, because the manometer appears to be objectionable on two accounts. In the first place it would be difficult to graduate a tube of a moderate length so accurately, as to discover the dilatation by it truly to two or three places of figures. In the next place a manometer of this construction may be made to give different results by a little management, which will be evident from the following experiment.

The manometer objectionable.

Exp. 2. A manometer $\frac{1}{16}$ of an inch in diameter was cooled by water to 35°, and the height of the column of air was then marked on the glass. In the next place the tube was suddenly plunged into water of 95°, and the height of the column marked as before. On cooling the instrument again as suddenly to 35°, the air contracted to its former dimensions; after which the temperature was raised a second time to 95° in a very gradual manner. The consequence was, that the column fell short of its former height by nearly $\frac{1}{10}$ of its length. This circumstance determined me, to prefer an æolipile to a manometer, the method of using which will appear in the following paragraph.

Exp. 2.

Æolipile preferable.

Exp. 3. What I have called an æolipile is a copper vessel of a conical figure and having a flat bottom. The slender part of the truncated cone has an aperture $\frac{1}{4}$ of an inch in diameter, which is turned directly downwards when the bottom of the æolipile is parallel to the horizon. 110 grains of water at the temperature of 64° were put into this vessel, which required the addition of 2895 grains of water at the same temperature to fill it. Things being thus prepared, the æolipile was immersed in a large pan, and suspended free of the sides and bottom by wires. The pan was then heated to 212°, and kept boiling for some time; after which it was reduced to 64° as quickly as possible by pouring cold water

Exp. 3.

water into it. The æolipile was then removed from the pan, the aperture being covered by the finger of the operator. After being carefully wiped with dry clothes, it was weighed, and found to contain 185 grain measures of air, which was evidently saturated with moisture, and at the temperature of 64°. But 53 measures of air thus circumstanced contain 52 measures of dry air. Thus it appears, that 181·5 measures of dry air at 64° occupy 2895 such measures when raised to 212° in contact with water of the same temperature: whence it follows, that 1 measure of dry air dilates so as to become equal to 15·95 measures in similar circumstances. It is proper to observe, that the barometer stood at 29·66 at the time; and that the height of the water in the pan, reckoning from the mouth of the æolipile, increased the pressure to 29·90: therefore the true dilation of one measure amounts to 16·70. But one measure of dry air at 64° occupies no more than 0·93344 parts of a measure when cooled to 32°; therefore the whole bulk of one measure of dry air raised from 32° to 212° in contact with water may be stated at 17·100 measures.

Experiments against the existence of an aqueous atmosphere.

I have made several experiments both with this æolipile and a glass flask on air of 64°, which was raised to temperatures less than 212°, but the results did not correspond to the theorem given in the Manchester Memoirs for the purpose of finding the dilatation of moist gasses confined in the manometer. Does not then the evidence of direct experiments authorize us to say, that the existence of an aqueous atmosphere is not proved? or more properly does not the same evidence show this imperceptible fluid to be not only invisible, but also imaginary?

Attention to minutiæ necessary.

Some of your readers may think the preceding experiments are related too minutely, particularly the first and third; but should an impartial person wish to repeat them, he will be of a different opinion. In fact too much precaution cannot be used to prevent the manometer or æolipile from touching the bottom of the boiler; for if this be not done, the experiment will fail, as I have found on different occasions; and this has happened when the water in the pan did not boil. I should also recommend a wide cylindrical boiler in preference to a small vessel with a long narrow neck

neck; because the resistence which vapour meets with in its escape from the latter will in all probability augment its temperature.

The foregoing remarks are confined to the gas of water, which is supposed by the new hypothesis to exist independently in the atmosphere; but I possess observations and experiments respecting the permanent gasses, and their mutual impenetrability, which want of room obliges me to omit at present. The author has made experiments on the permanent gasses.

Middleshaw, I remain, &c.
May 22d, 1809. JOHN GOUGH.

V.

An Essay on Manures. By Arthur Young, *Esq., F.R.S.*

(Continued from p. 123.)

Paring and Burning.

THESE are mechanical operations; and though nothing is directly added to the soil by them, yet the effects are in many instances very extraordinary, and as such ought to be treated of here. There is no subject in husbandry about which so many misconceptions are afloat, or such misrepresetations hazarded, as on this. Much misconceived and misrepresented.

1. *The Nature of the Ashes resulting from this Operation.*

We shall examine the result of burning Effects of paring and burning.

1st. Vegetables.

2d. Earths, including,........ { 1. Clay, 2. Loam, 3, Sand, 4. Chalk, 5. Peat:

under one of which heads every soil may be arranged.

These two articles will include all that generally comes within the sphere of paring and burning; for the animal substances in this case are too inconsiderable to demand attention, Destruction of worms and insects.

tention, although the destruction of *living* animals, as worms and insects, is a main benefit of the work.

Paring and burning, says Mr. Kirwan, reduces the roots of vegetables to coal and ashes, and thus prepares both a stimulant and nutriment for plants.

Ashes. Lord Dundonald observes, that "it is only from the ashes of fresh or growing vegetables, that saline substances, or alkaline salts, are to be obtained; none can be got from peat or decayed vegetable matter. The saline matter produced in the process consists of vitriolated tartar; the alkali of the burnt vegetables combining with the vitriolic acid, which in different states of combination is contained in most soils. Vitriolated tartar has very powerful effects in promoting vegetation." It promotes, as Mr. Senebier remarks, the decomposition of water. It will hereafter be seen, that hidrogen is a most active food of plants. Whatever, therefore, assists in this decomposition must act a very important part in vegetation.

Mr. Fourcroy thinks, that the ashes of burnt vegetables, which have been supposed to consist of earth or clay, when the fixed alkali is washed from them, are principally calcareous phosphorus, like those of animal bones. Lord Dundonald is of the same opinion. This observation is a most important one, and ought to be pursued. In regard to the calcination of earths, that of clay and chalk has been already treated. The circumstances are numerous in which this operation may be highly beneficial.

Loam or sand. Loam is composed of various combinations of sand, clay, and calcareous earths. The effect of fire exerted on sand, whether mixed in the form of loam, or by itself in a sandy loam, has not been sufficiently ascertained; and to draw conclusions from theory would be dangerous. If I were to reason upon the point, I should imagine that fire would add nothing to the nature of sand which could render it more fertile. The tendency of its operation would be to lessen its small degree of cohesion, from whatever cause arising, and might so far be prejudicial. Iron brought into combination with pure air lessens the aggregation *.

* Davy.

It

It is however a question demanding the combined efforts of the chemist and the farmer, not *reasoning* but *experimenting*.

Effect of heat.

The effect of heat in this operation is remarkable. Wherever burning has been much practised, experience has demonstrated the necessity of removing all the ashes where the fires were made; and though careful farmers remove some of the uncalcined earth, still these spots manifest a deeper green in the crop, than is observable in any other part of the field. The *general* warmth diffused may probably have a greater effect than is suspected.

2. *The Properties of the Ashes resulting from Paring and Burning.*

Properties of the ashes.

Vegetable ashes imbibe carbonic acid from the atmosphere *. They act in decomposition, and yield three fourths in carbonic acid, and one fourth a little inflammable; and last many years, by reabsorbing in winter the principles they had lost in summer †.

I imagine that the advantage of paring and burning some soils depends on the heat emitted from the burning vegetable fibres uniting oxygen with the clay, which forms more than the half of the slices of turf as they are dug from the ground ‡.

That the ashes produced by paring and burning operate as a very powerful manure, cannot be doubted; since in nine tenths of the trials that have been made through the wide range of so many counties, the crops which followed have been found to be very great indeed, and generally superior to those procured by means of any other manure.

Caution.

It is not the want of this success that has made so many enemies to the practice, but rather the contrary; the crops have been so large, and so often repeated, *because great*, that the soil has been left in a state of exhaustion.

This is a subject that demands the attention of the experimental chemist more than most others in the theory of agriculture. The examinations which have been made on

* Priestley. † Fabbroni. ‡ Darwin.

the

Good effects not fully accounted for.

the ashes of vegetables, and of earths, will account for a certain degree of benefit resulting from their use; but perhaps it does not fully account for the enormous crops, which are gained by the operation of paring and burning. I have gone through not an inconsiderable course of reading, with a view to discover the theory of this fact; but my research has not entirely satisfied me. The formation of charcoal, sulphate of potash, and phosphate of lime, with the decomposition of water, and the oxigenation of clay, added to the mechanical change effected by the fire, may certainly account for a considerable part of the improvement.

3. *The Paring and the Burning.*

Method of performing the operation.

The common practice is to pare from two inches on peat soils to half an inch on others: an inch is the more general depth.

Mr. Wilkes, of Derbyshire, has ploughed nine inches deep, and burnt the whole furrow with the assistance of coal *sleck;* manuring double the quantity of land burnt, but working an immense improvement on the space thus deeply burnt. I have seen other cases in which four inches depth was burnt with great success. In the fens of Cambridgeshire the paring is done with a plough, and the depth from one inch to two. On sand the paring should be as shallow as possible.

The chief attention paid in burning is to guard against too great a calcination; as the general opinion of those who have most practised this husbandry is, that the turfs should be rather scorched or charred than reduced to ashes. If burned during a brisk wind, sands frequently vitrify, and will not afterwards in many years, if ever, be restored to a state capable of contributing any thing to the support of vegetables: hence it is a practice with those who are aware of it, prior to burning, to shake out, in dry weather, from the grass-roots, the greatest part of their substance with harrows. The heaps should always be small, and the fire be applied on the sheltered side of them: this method, in a degree, should be regarded in the burning of earths of almost

most every kind; as hereby alone a carbonized substance, called the black ash, will be obtained; instead of a red brick earth, of much less fertility in the outset, afterwards less susceptible of its principles as imbibed from the atmosphere.

In practice, however, as I have found more than once on my own farm, other circumstances will govern this point; such as, the weather in drying the turf, the depth to which pared, and the age of the grass; for these points have all an influence on the size of the heaps.

4. *State in which the Ashes are applied.*

Here occurs a considerable variation in common practice. There are two methods; one, to spread and plough in immediately; the other, to spread immediately, but to leave them exposed to the atmosphere some months before turning in. Mr. Wedge, on the thin sand soil on a chalk bottom of Newmarket heath, had in one field a treble experiment; part was pared and burnt in the spring, and the ashes spread and exposed till ploughing in the autumn for wheat; part pared and burnt late, the ashes left in heaps, and spread just before ploughing for wheat; the third pared, and not burnt at all, by reason of bad weather. The first was by far the best; the second the next; and the third beyond all comparison inferior. This seems to be a decided proof, that the ashes absorb some matter from the atmosphere, which adds to their fertilizing qualities.

Application of the ashes.

5. *Application.*

The circumstances which may with propriety be touched on under this head, are,

Mode of applying them.

1st. Spreading.
2d. Depth of tillage.

The fact of the ashes improving more after having been for some time exposed to the atmosphere was probably the motive, which induced Mr. Tuke, of York, to pursue on the wolds of Lincoln a practice that deserves attention. It is

is to pare along the centre of the lands a width sufficient for the heaps and burning; to move the sods, in order to plough the breadth; then to plough it; to make the heaps for burning on the land so ploughed; by which means all the land may be ploughed before the ashes are spread, and by this means kept on the surface: two material objects being attained; 1st, the exposition of the ashes; and, 2d, they are not ploughed to the bottom of the furrow, but kept on the surface to combine with the land, and early sinking prevented.

Evenness of spreading is always a material object, whatever may be the manure.

The universal practice (except in one very singular instance) is to plough the first time very shallow. A multiplicity of observations have convinced the farmers in almost every part of the kingdom, that these ashes have a tendency to sink; and the aim has therefore been to keep them near the surface by shallow tillage, especially at first. The method of ploughing before they are spread entirely obviates the necessity of such a practice.

7. *Season.*

Season of the year.

As the work can only be done in dry weather, it is usually begun in March, in which month the NE. winds are more drying than at any other time. When the space to be burned is large, it is continued till September; and as the ashes are the better for exposition to the atmosphere, any crop may be put in that best suits the farmer's conveniency.

8. *Soil.*

As the *quantity* of the manure thus gained depends entirely on the depth of paring, I pass on to the consideration of soils on which the practice may be recommended.

Soil.

I have tried it myself but on two soils; on mountain peat, and on middling loam: on both these I had entire success. But the information, which the respectable society I address look for, must be derived from more varied experience than

than it is possible for one person to pretend to, I shall therefore select a few cases which will embrace all the soils. These might be multiplied tenfold, but it would swell these papers to too great a length to offer more than a sketch.

Clay.

Mr. Bailey, of Northumberland, speaking from great experience, says, "that he has found this operation the most effectual remedy or preventive of the calamity of the red worm and grubs." The advantage of the practice is the certainty of full crops. "I do not," says he, "recollect an instance where the cultivator was ever disappointed; and it is this amazing fertility, that has tempted many people to go on with repeated corn crops, until the soil was exhausted." Clay.

Loam.

On the enclosure of Stanwell in Middlesex, the allotments succeeded well under the perfect practice of paring and burning; and ill, where the turf was ploughed without the application of fire*. In the former case the land was immediately fit for turnips, tares, barley, and clover. In the latter, the tough wiry bent heath, and dwarf furze, kept the land too light and spungy for any crops; and the farmer will be plagued for many years. The difference between the two methods is more than the value of the freehold in favour of burning. I have observed in various counties the same decided preference†. Loam.

In the enclosure of Enfield Chase, (the soil, loam) Dr. Wilkinson states, from experience, that paring and burning saves a very heavy expense; that the ashes possess most fertilizing qualities; that grasses are thus much sooner to be introduced; that it is a security against the ravages of the worm; and that so far from ruining its staple, the land has afterwards retained its fertility during five successive crops‡.

* And which will be the case 99 times in 100 universally.

† Middleton's Middlesex. ‡ Ibid.

After nine years cultivation of land broken up without burning, it has been noticed, that on being laid down, young furze sprung up generally; burning is therefore absolutely necessary*.

Mr. Exter, near Barnstable, broke up a grass field in an enclosed farm, one half by paring and burning, the other half by fallow. The first crop was wheat; the burnt gave thirty-five bushels per acre, the ploughed seventeen; the former was clean, the latter had much couch. Winter tares; the burnt were fourteen inches long, when the ploughed were only six; when eaten off by sheep, the second growth was in length as twelve to four. The next crop being turnips, and dunged equally, the burnt side was free from the fly. Barley succeeded, which was considerably better on the burnt part. Clover was next, which was closer eaten on the burnt part; and when laid to grass was worth 5s. per acre more than on the ploughed half.

Does not diminish the soil.

Mr. Dalton, of Yorkshire, on a dry loam on limestone and gravel. "It is a mere chimera to suppose, that the soil is diminished by paring and burning. I have done it in the same field twice in the course of fifteen years, and could not discover it in the smallest degree†." On a light loam in Cornwall, Mr. Ans observes, "I was not *singularly* misled by speculative writers (who, I fear, have much to answer for) to think that burning caused a lasting injury to the earth. I fallowed three fields. I expected them to continue free from moss beyond the common period of its return. I found myself much mistaken; besides the crops failing, like those of some of my neighbours who had not burned, the moss returned as usual. Hence I and all my fellow sufferers from following have totally abandoned this practice, and stick to the ancient one of burning."

"It has been the practice of a friend of mine, and his father before him, and of others before them, for near a century past, (the estate having been in the family for many generations) on their thin limestone land, constantly to pare and burn after ten years grass. The soil is so thin,

* Middleton's Middlesex.

† Communication to the Board of Agriculture.

that

that the plough scalps the rock; yet no diminution of soil is in the least discovered*."

Sand.

"Upon sand I have tried paring and burning, but unsuccessfully†." But Colonel Vavasour speaks of it favourably on this soil, and from experience. Query, whether this difference of result did not hold to their courses of crops? The former speaks, in another case, of two crops of wheat, and one of oats. The latter, 1. turnips, 2. buck wheat, 3. seeds. If Mr. Wright looked on sand for corn, and not grass, no wonder he was unsuccessful. Sand.

Chalk.

Mr. Boys, near Sandwich, in 1783, pared and burnt twenty acres of loose dry chalk mould, four inches deep, on a hard chalk rock, value 1s. per acre, and sowed barley and sainfoin in March. His whole expense, barley crop included, 53l. Produce sixty-six quarters of barley, at 26s., 86l.: his *profit* 33l., or the fee-simple of the land at twenty-two years purchase, the price at that time. The sainfoin took well‡. In 1795, he writes to the author of the periodical work just quoted, "Should any of your friends, who so much condemn paring and burning, come into Kent this summer, I can show them several scores of acres of wheat, barley, oats, and sainfoin, now growing on land which has several times undergone the operation:—the crops of sufficient value to purchase the land at more than forty years purchase, at a fairly estimated rent, before the improvement. This will be ocular demonstration to them." Chalk.

Peat.

Twenty years past a field of coarse rushy land was broken up; part pared and burnt, the rest not. Whilst in tillage, Peat.

* Mr. Wright. † Ibid. ‡ Annals.

the part burnt yielded crops uniformly better than the others. It has been down to grass several years; the burnt part is quite free from rushes, and covered with a good sweet herbage; the other part full of rushes, and the herbage coarse*."

Mr. Simpson says, "I ploughed ten acres of moor, on a lime stone bottom, in the part most free from ling, without burning, and I have had sufficient cause to repent it; for I have not had even one middling crop since; and although laid down with seeds, they have by no means so good an appearance as those sown the same year on similar soils after burning, although I have expended as much lime and manure on this as on any part of the farm †."

Near Orton, on a peat moss, six or eight inches deep, on a stiff bluish clay; the only vegetable produce spongy moss, bent grass, dwarf rush, &c. wet and not drained; pared three inches deep, and burnt in the spring; then manured with thirty bushels of lime an acre; ploughed slightly for turnips, which were not hoed. They were worth 3l. an acre; and being sown with oats, produced seventy bushels per acre ‡."

Miss Graham was the first that pared and burnt moss in Monteith. Several acres, that were burnt above forty years ago, continue to carry a close sward of green grass at this day, without a single pile of heath §.

"Of all the methods of breaking up peaty soils which I have practised or seen, the best mode is paring and burning. I have seen various methods on several thousand acres, but none ever equalled this ||."

(To be continued in our next.)

* North-Riding Report.

† Ibid.

‡ Todd. Society's Transactions.

§ Perth Report.

|| Bailey.

Table of the Rain, that fell at various Places in the Year 1808, by the Rev. J. Blanchard, of Nottingham; with a Meteorological Table for the same Year, by Dr. Clarke, of that Town.

RAIN TABLE, by the Rev. J. Blanchard, of Nottingham.

1808.	Chichester.	London.	Bristol.	Cheltenham.	West Bridgford, near Nottingham.	Horncastle, Lincolnshire.	Chatsworth, Derbyshire.	Manchester.	Ferriby, Kingston upon Hull.	Heath, near Wakefield, Yorkshire.	Lancaster.	Dalton, Lancashire.	Kendal.	Edinburgh.
January	3·04	1·52	1·05	0·80	2·85	0·93	1·39	2·70	0·59	1·14	2·90	3·88	5·25	1·67
February	0·90	1·12	0·53	0·20	2·23	0·77	1·35	1·48	0·92	1·26	2·00	1·85	2·42	2·31
March	1·42	0·20	0·35	0·05	1·30	0·42	0·37	0·24	0·29	0·99	0·00	0·55	0·28	0·65
April	2·67	2·42	5·37	5·05	2·01	3·58	2·57	1·32	2·47	3·49	1·81	1·78	2·80	3·04
May	1·72	1·58	2·99	1·30	2·45	1·65	1·68	1·76	2·01	3·08	2·38	4·14	3·95	1·92
June	1·51	0·78	1·75	5·10	2·20	1·18	3·25	2·05	1·21	2·34	1·25	1·84	2·02	2·61
July	5·67	3·22	2·76	—	1·45	2·50	3·71	2·44	3·24	3·44	4·12	3·91	4·85	2·45
August	2·69	0·96	3·06	—	1·92	1·69	2·13	2·18	2·44	2·66	3·75	4·87	5·37	7·51
September ...	4·87	4·18	4·36	—	2·45	1·53	3·80	2·71	3·27	3·03	1·23	3·05	2·62	2·50
October	6·41	3·82	5·26	—	1·82	2·77	3·98	5·32	2·99	2·49	7·08	6·53	7·25	2·01
November	2·92	2·18	3·08	—	0·80	3·20	2·60	3·10	2·51	3·16	4·27	5·20	3·92	0·74
December	2·80	1·00	1·52	—	1·74	4·10	1·98	1·79	5·01	2·91	1·69	2·39	2·61	1·93
Total	36·62	22·98	32·08	12·50	23·22	24·32	28·81	27·09	26·95	29·99	32·48	39·99	43·34	29·34

RECAPITULATION.

	Inches. Decim.
Kendal	43·34
Dalton	39·99
Chichester	36·62
Lancaster	32·48
Bristol	32·08
Heath	29·99
Edinburgh	29·34
Chatsworth	28·81
Manchester	27·09
Ferriby	26·95
Horncastle	24·32
West Bridgford	23·22
London	22·98
Cheltenham, for the first six months	12·50

METEOROLOGICAL TABLE,

By Dr. CLARKE, of Nottingham.

1808.	Thermometer.				Barometer.				Weather.		Winds.			
MONTH.	Highest.	Lowest.	Mean.	Greatest Variation in 24 hours.	Highest.	Lowest.	Mean.	Greatest Variation in 24 hours.	Fair.	Wet.	N. & N. E.	E. & S. E.	S. & S. W.	W. & N. W.
January..	49	17	38·17	19	30·39	28·97	29·79	·85	20	11	2	3	51	37
February	55	22	38·65	13	30·74	29·42	30·05	·57	21	8	21	3	21	42
March ..	52	32	40·21	6	30·39	29·57	30·12	·31	23	8	65	16	—	12
April	56	30	43·62	10	30·20	29·07	29·79	·51	13	17	14	2	27	47
May	82	48	59·61	11	30·17	29·47	29·84	·24	21	10	21	20	41	11
June	72	56	59·95	9	30·25	29·62	29·91	·30	19	11	18	26	19	27
July	89	54	67·19	12	30·16	29·54	29·89	·21	24	7	19	17	36	21
August ..	76	53	64·62	10	30·17	29·35	29·78	·71	20	11	7	9	42	35
September	68	40	57·32	9	30·28	29·28	29·76	·38	19	11	29	7	26	28
October..	60	34	46·31	10	30·32	28·98	29·62	·91	19	12	10	6	40	37
November	55	30	45·06	13	30·25	28·72	29·76	·68	17	13	26	15	26	23
December	49	22	37·96	14	30·26	29·08	29·76	·55	21	10	30	4	27	32

ANNUAL RESULTS.

THERMOMETER.	WIND.	BAROMETER.	
Highest Observation, July 13th,	89°SW.	Highest Observation, Feb. 25th,	30·7
Lowest Observation, Jan. 22d,	17°SW.	Lowest Observation, Nov. 18th,	28·7
Greatest Variation, in 24 hours, Jan. 22d-23d, ...	19°	Greatest Variation in 24 hours, October 13th-14th, ...	·9
The Mean, ...	49·88	The Mean, ...	29·1

WEATHER.	DAYS.	WINDS.	TIMES.	RAIN.	INC
Fair	237	N. & NE.	262	Greatest Quantity in April,	3
Wet	128	E. & SE.	128	Smallest ditto, in February	
		S. & SW.	356		
	365	W. & NW. ..	352	Total	22
			1098		

REM-

REMARKS.

The town of Nottingham is situate in latitude 52° 59′ 35″ north, and in 1° 7′ 0″ longitude west of London. It rises with much grandeur from the banks of the small river *Leen*, gradually increasing its elevation as it extends to the N. E., so that above one half stands on a considerable eminence. The foundation is a soft sand stone rock, easily excavated, and forming excellent cellars. The buildings are chiefly of brick, and commonly three or four stories high. The streets are, in general, narrow. The neighbourhood produces an ample supply of coal, which is the only fuel used in the town. The *Trent*, a fine navigable river, flows, from west to east, within a mile of the town; it is subject to very sudden swells, which sometimes produce floods, that inundate the meadow ground between the river and the town. The atmosphere must be, in some measure, influenced by the evaporation that follows, as well as by the dense haze over the river in summer evenings, and the thick fogs of winter. Situation of Nottingham.

The barometer, thermometer, and pluviameter (or rain gauge), are new instruments, made by Jones, of Holborn. The thermometer, on Fahrenheit's scale, is placed outside a window, facing the west, in the centre of the town, but in a situation protected from currents of air, or reflected heat. The observations were made daily, at 8 A. M., 2 P. M., and 11 P. M., and from them the averages are deduced.—The barometer (of the portable kind) is firmly fixed to a standard wall over a stair-case, on a level of 130 feet above the sea. The observations were taken daily at 2 P. M., and from these the mean was obtained.—The pluviameter is placed in a garden, on an elevation of 140 feet above the level of the sea, where it cannot be affected by buildings, or gusts of wind. The observations are taken at the end of each month.—The observations on the wind were made at 8 A. M., 2 P. M., and at dusk, from the vane of a church steeple, the most elevated part in the town. Instruments & observations.

The following Copy of a Monthly Journal will be the best elucidation of the plan that has been pursued.

METEOROLOGICAL

METEOROLOGICAL JOURNAL for April, 1808.

Day.	Therm. 8 A.M.	Therm. 2 P.M.	Therm. 11 P.M.	Bar. 2 P.M.	Winds. 8 A.M.	Winds. 2 P.M.	Winds. Dusk.	Weather.
1	35	39	30	29·69f.	NW.	NW.	NW.	Snowy
2	32	12	33	29·84r.	SW.	SW.	W.	Fine
3	35	49	41	29·75f.	SW.	SW.	W.	Snowy, hail & rain
4	42	52	51	29·36f.	SW.	SW.	S.	Windy, showery
5	50	53	44	29·07r.	SW.	W.	SW.	Stormy
6	46	48	51	29·58f.	SW.	E.	SW.	Rain
7	54	56	48	29·76f.	SW.	W.	NW.	Showery
8	44	50	40	29·82f.	NW.	NW.	N.	Showery
9	43	57	44	30·20f.	NW.	W.	W.	Fine
10	46	52	46	30·20s.	W.	W.	W.	Cloudy
11	46	51	46	30·20f.	W.	SW.	W.	Cloudy
12	48	55	46	30·14r.	NW.	NW.	NW.	Showery
13	46	54	46	30·21f.	W.	W.	W.	Fine
14	47	56	45	30·13f.	NW.	W.	SW.	Fine
15	48	55	43	30·04f.	W.	NW.	W.	Cloudy
16	40	48	35	30·07r.	NW.	NW.	N.	Cloudy
17	38	39	37	30·09f.	SW.	NW.	NW.	Hail, rain
18	35	45	37	29·96f.	NW.	NW.	SW.	Cloudy
19	35	43	35	29·52f.	SW.	SW.	SW.	Snowy
20	37	50	35	29·55f.	SW.	SW.	NE.	Cloudy
21	33	38	36	29·23r.	NW.	NW.	W.	Snowy
22	38	43	39	29·11r.	SW.	SW.	SW.	Drizzly
23	38	43	40	29·42s.	NW.	NW.	NW.	Rain
24	40	44	38	29·72r.	N.	NW.	NW.	Showery
25	38	43	39	29·86r.	NW.	NW.	NW.	Showery
26	40	47	41	29·86s.	NW.	SE.	S.	Showery
27	43	45	40	29·95s.	N.	NE.	NE.	Showery
28	43	49	41	29·92r.	N.	NE.	N.	Cloudy
29	41	47	43	29·93s.	N.	N.	NE.	Cloudy
30	44	45	49	29·80f.	SW.	SW.	W.	Fine

Thermometer.	Wind	Barometer.	Wind
Highest observation 56°	W.	Highest observation 30·20f.	W.
Lowest ditto 30	W.	Lowest ditto...... 29 07r.	
Greatest variation in 24 hours .... 10°.		Greatest variation in 24 hours .... ..·51	
Mean in the Morning 41·50			
Afternoon 48·06			
At Night 41·30		Mean for the Month....29·79	
Mean for the Month..43·62		Rain for the Month 3·85 In.	

Weather.	Days	Wind.	Times		Times
		N.........	8	S.	2
Fair	13	N. E.	6	S.W.....	25
		E.........	1	W.......	19
Wet	17	S. E.......	1	N.W. ...	28
	30				90

Observations.—April 3d, Much snow fell in the morning, followed by hail and rain. 4th, Heavy rain in the night, with much wind. 5th, The weather tempestuous until evening, and much rain in the night. 7th, Heavy rain in the night. 8th, Sharp frost in the night. 15th, Sharp frost in the night. 16th, Hail storm at 7 P. M. and sharp frost in the night. 17th, Several hail storms in the afternoon, and sharp frost in the night. 20th, Heavy fall of snow in the night. 23d, Much heavy rain in the night.

Nottingham.

VII.

Observations on Sulphuric Ether, and its Preparation; by Mr. Boullay, *Apothecary, of Paris**.

The making of sulphuric ether may be improved.

THE use of sulphuric ether is at present very extensive, and its consumption so great, that it has become a produce of the arts in the large way. Its preparation, though much simplified, still merits attention; and appears capable of being improved, not only in respect to economy, but also as to the purity of the product.

The latter products always impure.

In the formation of sulphuric ether, whether by the distillation of a simple mixture of concentrated sulphuric acid and alcohol, or the addition of fresh alcohol to the residuum, all the quantity obtained is not equally dulcified; and, in spite of careful rectifications, the last portions always retain a more or less unpleasant smell, that may be ascribed to some oil intimately united with it, which it is very difficult to separate completely.

Theory of the formation of ether.

According to the theory of Messrs. Fourcroy and Vauquelin, founded on their learned researches into the subject, the attraction of sulphuric acid for water, assisted by heat, determines the transformation of alcohol into ether. This reaction of the principles of alcohol, exerted under the influence of the sulphuric acid, precedes the carbonization of the mixture, the formation of the oleum dulce, the extrication of sulphuric acid, and the other phenomena of the process carried to its end. We may even venture to say, that ether is no longer formed, when these products appear; and that what passes over after that time is only separated the residuum, in which it was contained ready formed. It would be an advantage therefore, to prevent, or at least retard, the appearance of these products, which announce a complete decomposition of the alcohol; and, by adding at a proper time fresh quantites of this liquid, to keep up such proportions, that the etherification may go on much longer. For this it appears necessary, that the sulphuric

What injurious to it.

This should be prevented.

* Annales de Chimie, vol. LXII, p. 242.

acid

acid should never compose more than two thirds of the contents of the retort, and that the alcohol should be scarcely ever less than the other third*. In this way the sulphuric acid is prevented from burning the alcohol to its loss, and we obtain none of the results of a decomposition carried too far, which is injurious to the etherification, and immediately follows it. We shall then have a better product, and in larger quantity; and the production of ether will continue, till the sulphuric acid is so much diluted by the water formed and separated, as to be unable to effect any change in the alcohol.

Apparatus.

The particular kind of funnel, which has facilitated my making ether by means of the phosphoric acid †, and is applicable to many other chemical processes, enabled me to carry this theory into practice in the following manner.

Improved process.

To a large tubulated glass retort, placed on a sand heat, I adapted a glass worm immersed in a vessel of cold water. The extremity of the worm was inserted into the neck of a large bottle, between which and a second bottle filled with water a communication was established by means of a siphon. Into the retort I introduced ten kilogrammes [22lbs. avoird.] of sulphuric acid concentrated to 66°. In the tubulure was inserted the funnel with two cocks, so that its pipe descended nearly to the bottom of the retort, passing through the sulphuric acid. Ten kilogrammes of alcohol at 36° of Beaumé's areometer were then poured in quickly, being conveyed through the acid by means of the funnel. The mixture was very well effected, though with violence; and it was the less coloured in proportion as the introduction of the alcohol was more speedy. The distillation was kept up by means of a fire under the retort; and as soon as about two kilogrammes had passed over, ten kilogrammes

The middle product best.

* The proportions of equal parts of sulphuric acid and spirit of wine, constantly adopted, appear to be most suitable. It is to be observed however, notwithstanding the utmost care taken to separate the alcohol, that comes over first, the product that follows does not attain the lightness, that constitutes true ether, till toward the middle of the process.

† See Journal, vol. XVIII, p. 64, and Pl. II, fig. 4.

of

of fresh alcohol at 40°* were introduced drop by drop, regulating the quantity as nearly as possible by what passed over into the receiver. The process was continued so as to obtain fifteen kilogrammes of a white limpid product, of the most agreeable ethereal smell and taste, containing no traces of sulphurous acid or oleum dulce, and yielding, when rectified on a water-bath, eight kilogrammes of pure ether, with some alcohol of an ethereal smell well adapted for future processes.

The liquid remaining in the retort was of the colour of beer, and very clear. It consisted of nearly the whole of the sulphuric acid employed, some alcohol, water, and no doubt a certain quantity of ether completely formed. The residuum.

This residuum, heated afresh, quickly assumed a black colour, and became sulphurous and oily. In this state it may enter into the composition of Hoffmann's mineral anodyne liquor. The residuum might also be turned to account, by using it as sulphuric acid where the alcohol could do no harm, as for instance, in forming different salts. Purposes to which it may be applied.

VIII.

Investigation of a Problem in the Doctrine of Permutations. By Mr. PETER BARLOW.

To Mr. NICHOLSON.

SIR,

IN the course of a mathematical investigation, in which I was lately engaged, it was necessary for me to determine —How many combinations could be formed out of a given number of things, in which there were several things of one sort, Problem in the doctrine of permutations

* I have observed, that alcohol at 36° is best adapted for the common preparation of sulphuric ether; and that the mixture is less coloured when it is at this strength, than if it contain less water. But at the second addition, as the acid is already weakened, it is better to employ it at 40°.

sort, several things of another sort, &c., by taking one at a time, two at a time, &c., to any given number of things at a time.

has been considered only partially, I have not been able to find, that this problem has been considered by any authors, at least, that I am acquainted with, who have written on the doctrine of permutations and combinations; except indeed Emerson, and one or two other authors of a later date, who have a similar problem, that is, a partial case of the above general one, which from a repetition of operations would be sufficient for the solution and the rule too tedious for practice. of the present question, but the rule which is given by them for determining the number of combinations in each particular case is so long and tedious, that it is really of no use, being little better, or less trouble, than finding the answer from repeated trials.

A very simple general rule. This circumstance led me to consider the problem independently of the measures there adopted, and having fallen upon a very simple rule, which includes the particular case of Emerson's in the general one above mentioned; and as it has not, to the best of my knowledge, been given by any author, who has written on this subject, I have been induced to submit it to you for insertion in your Journal, should you think it deserving a place in that useful work.

Problem.

Problem. To determine the number of combinations, that can be formed out of a given number of things, in which there are m things of one sort, n things of another sort, p things of another sort, &c.; by taking 1 at a time, 2 at a time, &c., to any given number of things at a time.

Rule.

Rule. Place in one horizontal row $m+1$ units, annexing ciphers on the right hand, till the whole number of units and ciphers exceeds the greatest number of things to be taken at a time by unity.

Under each of these terms write the sum of the $n+1$ left hand terms, including that as one of them, under which the number is placed; and under each of these write the sum of the $p+1$ left hand terms of the last line. Under each

each of these last the sum of the $q+1$ left terms, and so on, through all the number of different things, and the last line will be the answer: that is, the second term shows the number of combinations taking *one* at a time, the third term, the number of combinations taking *two* at a time, &c.

Example.

Given a number of the form $a^5\, b^5\, c^4\, d^4\, e^4\, f^3\, g$, to find how many different divisors it has, each of which shall be the product of ten factors, of nine factors, of eight factors, &c.; a, b, c, &c. being prime numbers. Example.

Here $m=5$, $n=5$, $p=4$, $q=4$, $r=4$, $s=3$, $t=1$, therefore by the rules

1	1	1	1	1	1	0	0	0	0	0	$= m+1$ units
1	2	3	4	5	6	5	4	3	2	1	$= n+1$ terms
1	3	6	10	15	20	23	24	23	20	15	$= p+1$ terms
1	4	10	20	35	54	74	92	105	110	105	$= q+1$ terms
1	5	15	35	70	123	193	275	360	435	486	$= r+1$ terms
1	6	21	56	125	243	421	661	951	1263	1556	$= s+1$ terms
1	7	27	77	181	368	664	1082	1612	2214	2819	answers.

That is, the number has seven prime divisions, twenty-seven that are composed of two factors, seventy-seven having three factors, &c.

I have selected this question, because it includes the particular case given by Emerson in his last example; in order that, by a comparison of both methods, an estimate may be formed of the labour that is saved by this rule. It may not at the same time be amiss to observe, that Emerson has not put down a twentieth part of the work, that is necessary for the operation. This rule compared with Emerson's.

Investigation of the Rule.

By the developement of the formula $(1+a+a^2\cdots\cdots a^m)\cdot(1+b+b^2\cdots\cdots b^m)\cdot(1+c+c^2\cdots\cdots c^p)\cdot(1+d+d^2\cdots\cdots d^q)$ &c., we shall evidently obtain all the possible combinations that can be formed with m a s, n b s, p c s, q d s, &c.; and, as we proceed in this developement, the law whence the above rule is deduced will be readily perceived. Investigation of the rule.

But,

But, for this purpose it will be best to give determinate values to m, n, p, q, &c.; by which means the operation will be more simple, and at the same time the law of formation will be equally obvious. Therefore suppose $m = 4$, $n = 3$, $p = 2$, then by actual multiplication we have

$$\begin{array}{l} 1 + a + a^2 + a^3 + a^4 \\ 1 + b + b^2 + b^3 \\ \hline \end{array}$$

$$1 + \left\{\begin{matrix} a \\ b \end{matrix}\right. + \left\{\begin{matrix} a^2\,1 \\ a\cdot b \\ 1\cdot b^2 \end{matrix}\right. + \left\{\begin{matrix} a^3\,1 \\ a^2\,b \\ a\;\,b^2 \\ 1\;\,b^3 \end{matrix}\right. + \left\{\begin{matrix} a^4\,1 \\ a^3\,b \\ a^2\,b^2 \\ a\;\,b^3 \end{matrix}\right. + \left\{\begin{matrix} a^4\,b \\ a^3\,b^2 \\ a^2\,b^3 \end{matrix}\right. + \left\{ \&c. \right.$$

And again, multiplying this last product by $1 + c + c^2$, we obtain the following result.

$$1 + \left\{\begin{matrix} a \\ b \end{matrix}\right. + \left\{\begin{matrix} a^2\,1 \\ a\,.\,b \\ 1\;\,b^2 \end{matrix}\right. + \left\{\begin{matrix} a^3\,1 \\ a^2\,b \\ a\,.\,b^2 \\ 1\cdot b^3 \end{matrix}\right. + \left\{\begin{matrix} a^4\,1 \\ a^3\,b \\ a^2\,b^2 \\ a\cdot b^3 \end{matrix}\right. + \left\{\begin{matrix} a^4\,b \\ a^3\,b^2 \\ a^2\,b^3 \end{matrix}\right. + \left\{ \&c. \right.$$

$$c + c\left\{\begin{matrix} a \\ b \end{matrix}\right. + c\left\{\begin{matrix} a^2\,1 \\ a\cdot b \\ 1\cdot b^2 \end{matrix}\right. + c\left\{\begin{matrix} a^3\,1 \\ a^2\,b \\ a\;\,b^2 \\ 1\cdot b^3 \end{matrix}\right. + c\left\{\begin{matrix} a^4\,1 \\ a^3\,b \\ a^2\,b^2 \\ a\;\,b^3 \end{matrix}\right. + c\left\{ \&c. \right.$$

$$c^2 + c^2\left\{\begin{matrix} a \\ b \end{matrix}\right. + c^2\left\{\begin{matrix} a^2\,1 \\ a\cdot b \\ 1\cdot b^2 \end{matrix}\right. + c^2\left\{\begin{matrix} a^3\,1 \\ a^2\,b \\ a\cdot b^2 \\ 1\cdot b^3 \end{matrix}\right. + c^2\left\{ \&c. \right.$$

Now, without pursuing the developement any farther, we shall readily perceive, that all the combinations in the second place, in both products, consist of *one* letter, in the third place, of *two* letters, and in the fourth of *three* letters, &c. And farther, that in any term, for example the fifth term, the number of combinations is equal to the number in the fifth, fourth, and third, of the foregoing product; the number of combinations in the fourth term is equal to the number in the fourth, third, and second: that is, the number of combinations in each term is equal to the number in the three last named terms of the foregoing product; and if we had used c^3, then the number in each term would have been equal to the *four* last named terms of the foregoing product; and generally, if we had employed c^p, the number

ber of combinations in each term would have been equal to the number in the $\overline{p+1}$ left-hand terms of the preceding line. And exactly the same law is observed when we multiply this last product by $(1+d+d^2-d^q)$, that is to say, each term of the new product is equal to the number of combinations in the $q+1$ left hand terms of the line which precedes it; and so on, for any number of multiplications whatever. Whence the truth of the rule is manifest.

We may farther remark, that, if the greatest number of things to be taken at a time exceeds half the number of things given, still, we need not pursue the operation for more than half the given number, as will be evident from a closer inspection of the above formulæ. For it must be readily observed, that, were we to carry the operation of each multiplication to its whole extent, the terms on each product would increase, from the first to the middle terms, and then decrease again in the same manner to the other extremity of the line.

Yours, &c.

PETER BARLOW.

Royal Military Academy, Woolwich.
May 31*st*, 1809.

IX.

Description of a very sensible Hygrometer. By Lieutenant HENRY KATER, *of his Majesty's* 12*th Regiment* *.

IN the *Mysoor* and *Carnatic* is found a species of grass, which the natives call, in the Canarese language, *oobeena hooloo*, in the Maratta, *guvataa sæ cooslee*, and, in Tamul, *yerudoovaal pilloo* †. It is met with in the greatest abundance, about the month of January, on the hills; but may be procured in almost every part of the country, and is very generally known. An Indian grass

* Abridged from the Asiatic Researches, vol. IX, p. 24.

† It is the *andropogon contortum* of Linnæus, and may be easily distinguished from all others, by the seeds attaching themselves to the clothes of those who walk where it grows.

Accident

has a beard very sensible of moisture.

Accident led me to remark, that the bearded seed of this grass possessed an extreme sensibility of moisture; and being then in want of an *hygrometer*, I constructed one of this material, which, on trial, far exceeded my expectations.

Hygrometer made of it.

A B C D, Pl. VI, fig. 1, represents a piece of wood, about fourteen inches long, three inches broad,, and one inch and two tenths thick. The upper part is cut out, as in the figure, to the depth of two inches, leaving the sides A and B, about three tenths of an inch thick. The wood, thus prepared, is morticed into a square board, which serves as its support.

Fig. 2 is an ivory wheel*, about an inch and two tenths *diameter*, and two tenths of an inch broad at the rim. A semicircular groove is made in the circumference, of such a depth, that the *diameter* of the wheel, taken at the bottom of the groove, is one inch. Through the axis, which projects on one side four tenths of an inch, a hole is made, the size of a common sewing needle; and, on this, as a centre, the wheel should be carefully turned; for, on the truth of the wheel the accuracy and sensibility of the instrument chiefly depend. From the bottom of the groove a small hole is made obliquely through the side of the wheel, to admit a fine thread. All the superfluous ivory should be turned away, that the wheel may be as light as possible.

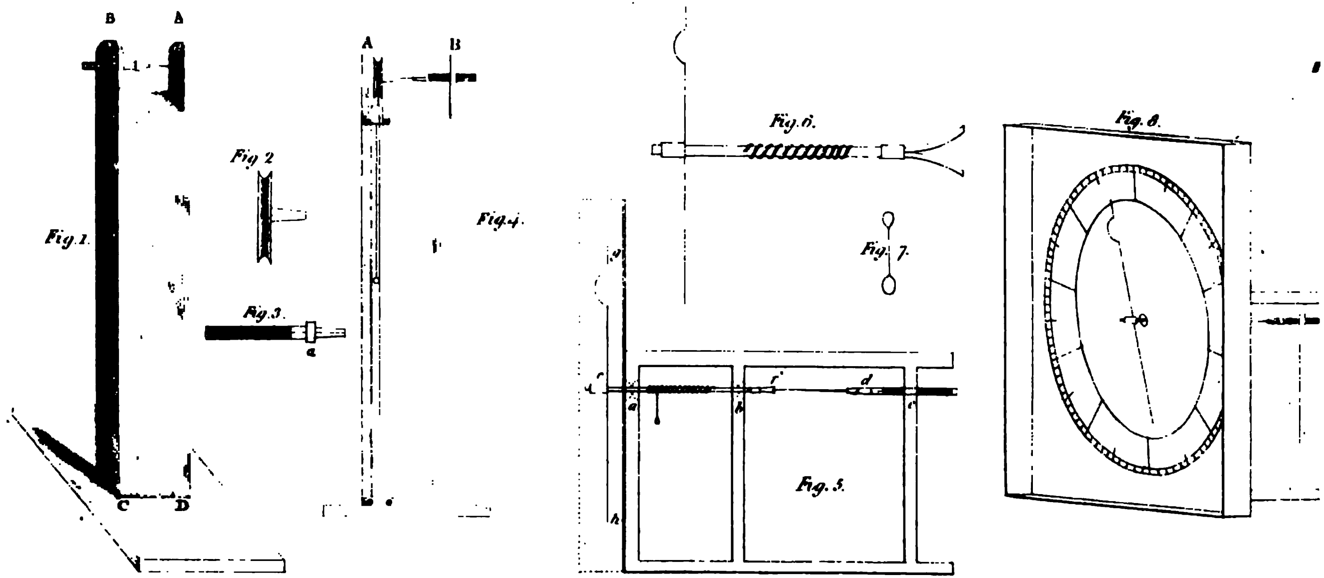
Fig. 3 represents a piece of brass wire, two inches long; on one end of which a screw is made, an inch and a half in length; and, in the other, a notch is cut, with a fine saw, to the depth of half an inch. This part is tapered off, so that the notch, which is intended to hold the beard of grass, in the manner hereafter described, may be closed, by means of a small brass ring (*a*) which slides on the taper part of the wire.

A little below the centres of the semicircles A and B, fig. 1, two holes are made, precisely in the same direction: one of these is intended to receive the screw, fig. 3, and the

* In my first experiments I used a wheel made of card paper, with an axis of wood, which answered very well.

other

Lieut. Henry Kater's Hygrometer



other a gold pin, which is to project four tenths of an inch beyond the inside of the part A. The pin is made rather smaller than the hole in the axis of the ivory wheel, and is highly polished; in order that the motion of the wheel may be the less impeded by friction.

Two fine threads, about fourteen inches long, are passed together through the hole in the groove of the wheel, and are prevented from returning, by a knot on the outside. To the ends of these threads two weights are attached, *exactly similar*, and just heavy enough to keep the threads extended.

One of the threads having been wound on its circumference, the wheel is to be placed on the pin, about the tenth of an inch from the side A, as in fig. 4. Two glass tubes, of a sufficient bore to admit the free motion of the weights, are fixed in grooves, in such a manner, that each thread should fall exactly in the axis of the tube. The tubes are so long as nearly to touch the ivory wheel.

The beard of the *oobeena hooloo*, being prepared by cutting off that part which is useless, is inserted about the tenth of an inch in the projecting end of the axis of the wheel, and confined by a small wooden pin, which is to be broken off close to the axis; the other end is placed in the notch of the brass screw, before described, and secured by means of the sliding ring.

It is evident, that when the grass untwists, the wheel will turn on the gold pin; and the thread, which is wound about it, with the weight attached, will descend in the one glass tube; while, on the contrary, the weight on the opposite tube will ascend, and *vice versa*. Action of the hygrometer.

The beard of the grass is now to be thoroughly wetted, with a hair pencil and water; and when the wheel is stationary, the weights are to be so adjusted, by turning the brass screw, that the one shall be at the top, and the other at the bottom of the glass tubes; which points will mark *extreme moisture*. Adjustment for moisture

The instrument must then be exposed to the sun, or to some heat, not powerful enough to injure it, but sufficient to obtain a considerable degree of dryness. The weights will now change situations; and, probably, on the first trial, will and dryness.

will continue to move *beyond* the glass tubes. Should this happen, the beard of grass is to be shortened, by sliding back the ring, and advancing the brass screw, so as to include a longer portion in the notch. Other trials are to be made, and the length of the grass varied, till the extremes of dryness and moisture are within the limits of the glass tubes.

Inconvenience of this form.

If the whole of that part of the *oobeena kooloo*, which possesses the hygroscopic property, be used, the scale will comprise more than *twenty-four* inches; a length, which, though perhaps useful on particular occasions, will not be found convenient for general purposes.

Trial of its accuracy.

From an idea, that in a high state of moisture the grass would not retain sufficient power to move the wheel equably, it was thoroughly wetted, till it indicated extreme moisture, and, while in this state, the wheel was drawn round, by laying hold of one of the threads: on releasing it, it instantly regained its former situation, with considerable force. The same experiment was made, in various other states of moisture, and it was always found, that the weights returned immediately to the degree from which they had been removed.

A metal wheel may be used.

It would perhaps be an improvement, if a light wheel of brass, or any other metal, not liable to rust, were used instead of the ivory one; the grass having been found, by experiment, to be capable of moving a wheel of lead. The axis of the wheel might be made very small, and supported on Ys, which probably would add much to the sensibility of the instrument.

Adapted to slight variation of moisture.

I have yet had no opportunity of comparing this with any other hygrometer; but it is simple in its construction, not easily disordered, and should seem, from the extent of its scale, to be particularly adapted to experiments, in which small variations of moisture are to be observed.

Hygrometrical observations connected with refraction.

The *hygrometer* has been hitherto an instrument rather of curiosity than utility. But from most accounts that we have, it appears very probable, that this instrument has more to do with the phenomena of *refraction*, than either *barometer* or *thermometer*. If then we could obtain a number of observations of apparent altitudes together with

with data from which to calculate the true, noting at the same time the *hygrometer*, *barometer*, and *thermometer*, perhaps some law might be discovered, which might enable us to ascertain the quantity of the effect of moisture on refraction. It was with this view the *hygrometer* above described was constructed; but not having yet had an opportunity of obtaining the requisite observations, it is to be hoped they may be made by those, who are in possession of time and instruments equal to the undertaking.

X.

Description of an improved Hygrometer. By Lieutenant HENRY KATER, *of his Majesty's 12th Regiment**.

SINCE I had the honour of laying before the Asiatic Society "a description of a very sensible Hygrometer," I have attended much to the improvement of the instrument, and am induced to think that some farther account of it may not be deemed wholly unacceptable.

Improvement of the preceding hygrometer.

The principal objection to the hygrometer described in my former paper arose from the necessity of shortening the beard of the *oubeena hooloo*, in order to reduce the scale to a convenient length; this was to be obviated only by giving the instrument a circular form, and inventing some mode of ascertaining without difficulty the number of revolutions made by the index.

A B C D, (Pl. VI, fig. 5) is a frame, made of small square bars of brass or silver; this frame is soldered to a square plate B E, the edges of which are turned up, as represented by the dotted lines, to secure the *index* from injury: on the face of the plate is engraved a circle (see fig. 8) which is divided into *one hundred* equal parts. Three holes, *a*, *b*, *c*, are made through the frame and plate in the same direction; the holes *a* and *b*, are of a conical form as represented by the dotted lines, and are highly polished to lessen friction; the hole at *c* receives a screw, one end of which is tapered, and has a *notch* cut in it with a fine saw, which may be closed by means of the sliding ring *d*.

Description of the improved instrument.

* Ibid, p. 394.

The

The *axis e f* is made of silver wire, very smooth and straight, and of the size of a large knitting needle; on the axis *a screw* is formed, by twisting a smaller silver wire tightly around it *from left to right*: this screw should be fourteen or fifteen threads in length; the end of the axis, *f*, is divided, and is to be closed by a small sliding ring. As this is the most important part of the hygrometer, fig. 6, represents it on an enlarged scale.

A *loop* and *drop* (fig. 7) is made of fine gold wire, of such a size as that when suspended on the screw it may slide along it with perfect freedom by means of the revolution of the axis, but not escape from one interval to another by any other motion; should the loop, on trial, be found too large (as indeed it ought to be) it may be easily closed a little, by placing it on the screw, and pulling it gently by the drop, it will then assume an elliptical form, as in the figure. This loop is intended to register the number of revolutions made by the index, as it hangs freely from the *axis*, and advances one *interval* between the threads of the screw, for each revolution.

The *index*, *g h*, is made of fine wire, accurately balanced, and as light as possible; it fits on the end of the axis *e*, *and is to be placed at right angles with the commencement of the screw.* (See fig. 6.)

The beard of the *oobeena hooloo* is represented between *f* and *d*, (fig. 5.) The top of it, which is crooked, being cut off, it is first secured between the cheeks of the axis, at *f*, by means of the small sliding ring; the axis is then turned round till the gold loop is brought to the fifth or sixth *interval* of the *screw*, counting from the dial plate; the screw at *c* is then advanced, so as to receive the lower or thick extremity of the beard of the *oobeena hooloo* in the notch, where it is also confined by the sliding ring *d*.

Adjustment of this hygrometer. The extremes of *dryness* and *moisture* are determined in the following manner. The hygrometer is placed in a new earthen pot, which has never been wetted, and exposed for a considerable time to as great a heat as the glass can bear without injury: when the *index* is perfectly steady, the hygrometer is to be taken out of the vessel, and the screw at *c* turned round with a pair of pincers, so as to bring the gold

gold loop to the *first interval* of the screw on the axis, counting as before from the dial plate, (which is to be placed to the left hand) and the index to 100, or zero. The hygrometer must now be suffered to cool gradually, during which, if the atmosphere be in a mean state of moisture, the index will make four or five revolutions; the *oobeena hooloo* is then to be continually wetted with a hair pencil and water, till the index is again perfectly steady. This will require some time, as it moves very slowly when within a few degrees of *extreme moisture*. The degree at which the index stands is now to be noted, and the number of *intervals* counted between the dial plate and the gold loop, and this number prefixed to the observed degrees will give the extent of the scale.

Reduction of the observations to a standard.

All observations made with this hygrometer are to be reduced to what they would have been had the scale consisted of 1000 parts, or ten revolutions of the index. This is most convenient, as it facilitates the comparison of observations made with different hygrometers. An example may not be thought superfluous. Suppose the scale of the hygrometer to be 1145, or eleven *intervals* and forty-five *parts*; and that at the time of observation, there are *four intervals* between the dial plate and gold loop, and 50 *parts* shown by the *index*; this would be written 450. Then, as 1145 : 1000 : : 450 : 393 nearly, the number of degrees to be registered.

If two of these hygrometers, in which the extremes of dryness and moisture are well determined, be compared together, they will seldom differ *ten divisions* from each other, which is as near a coincidence as can be expected.

The *oobeena hooloo* or *andropogon contortus* is found in every part of the country, in the month of January, when it should be gathered, and thoroughly dried in the sun, before it is used.

Sensibility and other advantages of the instrument.

This grass appears to be far superior to any other hygroscopic substance, hitherto discovered. In the Encyclopædia Britannica, the scale of SAUSSURE's hygrometer is said to consist of 400 degrees, or rather more than *one* revolution of the index; the hygrometer here described makes *eleven* or *twelve* revolutions; it possesses also the advantage

vantage of being perfectly portable, cannot easily be deranged, and may be much reduced in size, if thought necessary, without affecting the extent of the scale.

XI.

On the Germination of Seeds. In a Letter from Mr. J. Acton, of Ipswich.

To Mr. NICHOLSON,

DEAR SIR,

Physiology difficult of investigation.

IT is admitted by the most enlightened philosophers, that scarcely any subject can present itself more difficult of investigation than animal and vegetable physiology. The functions depending on vitality must not be compared to the common chemical processes, or to those changes constantly taking place in nature by the action of inorganic bodies on each other. Life itself is a phenomenon enveloped in mystery, and probably will ever remain so. We can form no judgment of it but from its effects; and those are of so complex a nature, that it is only by the most attentive and studious examination of them we can expect to withdraw the veil of obscurity, under which they are hidden, or at all approximate to the truth. Any suggestions presenting themselves to the mind on so important a subject should be encouraged; and if we can hope to throw the least additional light upon it by our exertions, no obstacles should stop us; not even the (almost) certainty of ultimate failure ought for a moment to lessen the energy of our pursuits.

Functions of organic bodies particularly deserving notice.

Perhaps none of the functions of organic bodies deserve our attention more than those tending immediately to existence, namely the respiration of animals, the germination of seeds, and the consequent vegetation of plants; as also the alterations taking place in the surrounding atmosphere during their operation. The following humble attempts, having for their object the farther illustration of these phenomena by experiment, are with diffidence submitted

Object of the writer.

through

through the channel of your widely circulated Journal to the eyes of the philosophic world; and if they shall be found of sufficient consequence to clear up any doubt, or induce one single effort in others toward explaining the matters to be treated of; my end will be entirely answered, and my trouble rewarded. They have been undertaken and preserved amidst many interruptions and discouragements; and if they shall be found not to have all the regularity and accuracy to be desired, I trust they will yet have some claim to attention, if not from their originality, at least from the persevering and disinterested industry, which gave rise to them, and brought them to a conclusion, the striving as much as possible to corroborate each experiment by repetition, and the avoiding to make any deductions but such as are fully warranted by facts only.

General opinion, that respiration destroys oxigen, and vegetation restores it.

Since the time of Dr. Priestley, the generally received opinion has been, that in respiration the oxigen gas of the atmospheric air is absorbed, and carbonic acid gas given out; and that in vegetation plants are constantly absorbing the carbonic acid gas as their natural food, and emitting oxigen gas, tending to restore the air to its original purity; in this manner keeping up a regular series of compositions and decompositions, beautiful from their apparent simplicity, and the more deserving of admiration from seeming to harmonize with what was known of the great system of the universe.

This doctrine lately opposed.

No fundamental opposition appears to have been successfully made to this doctrine, till about two years ago; when a work on the subject was published, in which the respectable and learned author* brought together in a small compass almost all the experiments that had been performed, and added a few of his own, for the express purpose of announcing and endeavouring to demonstrate the following theory: "That no air enters the plant or animal during its "appropriate living processes; but that, during the opera- "tion of their respective functions of germination, vegeta- "tion, and respiration, solid carbon is emitted as a secre- "tion

New theory of Mr. Ellis.

* Mr. D. Ellis on Germination, &c.

mer situation. After continuing this practice for some time, while engaged in the same manner, I was hastily called away, and left the quill partly in the jar, with one end rising out of the mercury. The jar was then two thirds full of gas, but on my return in about half an hour, I perceived it had increased very considerably, and on placing the quill in other jars, I distinctly heard a shrill whistling noise, like that of air under pressure passing through a capillary tube, and I observed the mercury slowly to sink, till it was on a level on the inside and outside of the jar. I was then convinced the atmospheric air had rushed in by means of the quill, and consequently that all the experiments, in which this had been introduced, must have been vitiated. I reversed the quill, and it still had the same effect. I tried it in jars over water, but no air passed. I afterward made use of string, and other substances, and they all admitted the air through quicksilver, though in different degrees, some being much slower conductors than others. After considering this phenomenon, the best judgment I am able to form of it is, that the air does not pass through the body of the quill, or other substance, but between the mercury and its sides; and in water the passage is prevented by their being in closer contact with each other. Whether this explanation be satisfactory, I leave to your superior knowledge to determine: I confess I was gratified with the discovery, as far as concerned my experiments, as it enabled me to prevent their being so rendered incorrect for the future.

Air of the atmosphere conveyed through mercury by a feather,

but not through water. String, & other substances, acted in the same way.

Mercury does not form a close contact.

It having been stated as a principal argument in favour of the emission of solid carbon from the seed to unite with the oxigen gas of the air, that the quantity of carbonic acid produced was found to be equal to that of the oxigen gas disappearing; upon reflection, it appeared to me replete with difficulty, if not impossible, to ascertain this to any degree of accuracy, from the moistened seeds never ceasing to give out carbonic acid gas, whether oxigen gas be present or not. I was therefore desirous of informing myself upon this subject, and for this purpose I instituted the following method of proceeding.

Argument for the emission of solid carbon in germination questioned.

Exp. 1. Into an inverted jar, containing about 13 cubic inches carefully filled with mercury, I introduced a considerable

Experiments to ascertain the fact.

Germinating barley.

derable portion of barley previously steeped in water, and suffered to germinate till the radicles had shot out about one third of an inch. In passing them under the quicksilver it is almost impossible, even with the utmost care, to avoid the introduction of a small portion of atmospheric air, which closely adheres to them; but this being trifling, the results will not be materially affected by it. The seeds were suffered to remain in this situation from the 11th of February to the 2d of April, the gas being occasionally taken out and tried in the following manner.

Temp. 48°, Pressure 28·68.

In 24 hours	1·60 cub. in.	63·23	absorbed out of 100 parts by lime water.
same time	6·60	91·00	
48 hours	7·20	98·00	
same time	4·90	98·16	
same time	6·50	98·18	
3 days	7·00	98·18	
several days	5·50	98·46	
several days	2·00	99·00	
2d April	1·00	99·00	
	42·30	whole of the gas produced.	

A cubic inch was each time exposed to lime water in Pepys's eudiometer. The remaining gas, generally consisting of several cubic inches, was removed into a narrow graduated tube, and a small quantity of a solution of caustic potash passed up. The results of both these trials were compared, and they were as nearly as could be analogous. Two smaller jars were also charged with some barley in the same manner: the gas produced was in proportion to the above, and the absorption nearly similar.

2d Exp. Germinating beans.

Exp. 2. On the 19th of February, temp. 48°, pressure 30·10, eighteen very small beans, freshly germinating, were passed up into aninverted jar full of mercury, holding about 5 cubic inches.

In

In 48 hours····	0·56 cub. in.	90·17	absorbed out of 100,
several days	3·00 ······	98·00	
14 days ····	3·00 ······	98·47	
several days	2·00 ······	99·00	
several days	2·50 ······	99·00	
	11·06		

Exp. 3. On the 24th of March, temp. 54°, press. 29·34, twenty germinating pease were placed in a similar situation under a jar, containing about 2½ cubic inches. 3d Exp. Germinating pease.

In 3 days	2·00 cubic inches	96·00	absorbed out of 100,
In 3 days	1·00 ··········	98·90	
	3·00.		

By these experiments it appears, that seeds, having once begun to germinate, give out carbonic acid gas in considerable quantity, even at low temperatures, though excluded from oxigen gas, and placed in the most awkward and unfavourable situations. And this circumstance should be kept in view, as it will have some influence in determining, if there be a possibility of ascertaining the moment when germination ceases in seeds placed in a confined portion of oxigen gas, or common air, or whether any other carbonic gas be formed, than what is *supposed* to arise from the solid carbon uniting to the oxigen gas, and which has been assumed to be in an equal proportion to the oxigen gas that disappears. Germinating seeds give out carbonic acid gas, when oxigen gas is excluded.

Now it seems evident, that carbonic acid gas can be readily produced by moistened seeds without the contact of oxigen gas; and in several trials I have observed the gas beginning to appear in a few minutes after passing the seeds up the quicksilver, and when from their being in a healthy vigorous state of germination there was no possibility of incipient putrefaction. In most instances on a small scale, on examination of the gas collected in the first 24 hours, the absorption by limewater has been about 90 per cent: and as this has been an invariable case, even where every precaution was taken for the exclusion of common air, I suspected, that in wholesome germination a small portion of nitrogen A little nitrogen suspected

gas

to be emitted in germination. gas might be emitted from the seed along with the carbonic acid gas, either by the decomposition of some of its gluten, or by absorbing a small quantity with the oxigen gas of the atmosphere. In the experiment No. 1 it will be seen, that the first tried gas left a considerable residue, owing no doubt to the casual introduction of atmospheric air in passing up the seeds: but the gas, as it formed, being transferred into other jars, this errour, after the first 24 hours, must have ceased to have any effect. Latterly the production of gas became more slow; and if the seeds had been suffered to remain, most likely it would in time have altogether ceased. The seeds after the first experiment acescent. When they were withdrawn and inspected in the first experiment, no sign of putrefaction appeared; they had an acescent smell, and distilled water poured upon them in a moment deeply reddened paper stained with litmus.

Theory of the production of gas in germination. Analysis has demonstrated the principal constituent parts of graminaceous or cereal seeds, to be a large proportion of fecula, a little ready formed saccharine matter, and a portion of gluten; which last has been proved to be the active agent in fermentation, and necessary for the conversion of sugar and fecula into alcohol. Therefore, to account for the production of gas in germination, as in seeds placed as in the above experiments, it appears, that, after imbibing a quantity of moisture, the fecula by the action of the gluten becomes gradually decomposed; the already formed saccharine matter is dissolved, and assists in the instant commencement of germination; water most probably is decomposed; its oxigen, uniting to the carbon of the seed, forms the carbonic acid evolved; while the hidrogen in its nascent state, by combining with another portion of carbon, assists the continued conversion of the fecula into saccharine matter; the oxigen gas of the atmosphere is absorbed for the purpose of restoring the equilibrium of the elementary parts, which the decomposition of the matter of the seed, while going on, has a tendency to destroy. But if germination be impeded or stopped, by the exclusion of oxigen gas, or otherwise, the regular composition and decomposition, and consequent changes in the substance of the seed, presently cease. Carbonic acid gas however still continues to be given out, in consequence of the action of the

the gluten on the saccharine matter formed by the germination. When the sugar is exhausted, the acescent first, and then the putrefactive phenomena commence; but only very partially, as I have found the seeds will remain for many months in the jars after the carbonic acid gas has nearly ceased to be produced, without undergoing much apparent alteration.

Exp. 4. To observe how far the same phenomena might take place in matters completely disorganised, and under what variety of circumstances this prolific gas (carbonic) would be produced, I mixed up a little flour, water, and yeast into a stiff paste, and passed a piece of it about the size of a walnut up an inverted jar filled with mercury. In three days I collected seven cubic inches of gas. The whole being submitted to lime water, an absorption ensued, leaving one tenth of an inch only, which appeared to be nitrogen. Paste gave 6·9 carbonic acid gas, 0·1 nitrogen.

Exp. 5. I also placed in the same situation a piece of paste made with flour and water only, about the same size, rolled very stiff. The gas here formed very slowly, not more than 3·50 cubic inches being collected in ten days. Of this lime water took up 94 per cent. In 8 days, after 4 cubic inches more had formed, and by the same test, 96 per cent were absorbed. Paste without yeast.

Exp. 6. Three pieces of the same paste were also placed in an inverted jar, containing 1·30 cubic inches of oxigen gas of the purity of 98 per cent. After the paste was in the jar, the whole indicated by the graduated scale 2·75 cubic inches. In three days, the usual allowance being made for difference of temperature and pressure, an absorption had evidently taken place, the volume being reduced to 2 cubic inches. In four days more it increased to 3·70 cubic inches; and in four days after to 7. A little of the air being now tried with limewater, 95 per cent were absorbed; evidently showing, that the greatest part of the oxigen gas had disappeared. To prove this still farther, it was suffered to remain till it had increased to 15 cubic inches, when the same test took up 99 per cent, which it could not have done, had any oxigen gas remained. Paste in oxigen gas.

Exp. 7. To be convinced no errour had ensued in Exp. 5, I repeated Exp. 5 repeated.

I repeated it with the utmost care. After some days, 9 cubic inches of gas were collected; and on being submitted to the usual test, 90 per cent disappeared. In ten days after 5 cubic inches more had formed, of which 99 per cent were absorbed; and 3·2 cubic inches being tried with caustic potash, only a bubble remained.

Germination not necessary to the production of carbonic acid.

These results prove beyond any doubt, with how much facility the particles of seeds act upon each other, even in a pulverized state, when moistened with water; and how uncertain, under any circumstances, must be the attempt to discover the precise time of the cessation of germination of seeds confined in oxigen gas, or what part of the carbonic acid gas is given out by that process, and what by the spontaneous decomposition of some portion of the seed. Hence it should seem, that such experiments, as may have been made with a view to establish the identity of quantity between the disappearing oxigen gas and the newly formed carbonic acid gas, must be supposed to be in a great measure fallacious, and consequently the conclusions drawn from them not to be depended upon.

The seeds lost weight by evaporation, not by germination.

In my first essays on this subject, rendered fruitless by the circumstance before mentioned, I was desirous of discovering whether seeds increased or decreased in weight during germination. For this purpose I weighed accurately several parcels of barley before placing them in the air, and after they were taken out, having previously well dried their surfaces with blotting paper. In every instance I found a deficiency of weight, but not beyond what may be easily accounted for by the evaporation of moisture from the seeds; as I could often, when the air was particularly dry (as oxigen gas prepared from oxigenated muriate of potash over mercury is), perceive some water condensed on the sides of the jars. It appears therefore impossible in this way to come at the truth: but from all I have been able to observe, I am persuaded a real increase takes place. The following statement gives an account of the loss these seeds sustained, while confined for some days in jars of atmospheric air.

200 grains of barley,	lost	8·00 grains	
120 grains	lost	6·40	
100 grains	lost	5·60	
40 grains	lost	2·30	
30 grains	lost	2·20	
30 grains	lost	2·10	

I merely give these results as means of preventing unnecessary trouble and waste of time in others, and not as of any other importance. The seeds were continued in the air, until the increase was considerable, and the oxigen gas was for the most part exhausted, as appeared by the accustomed tests of limewater, and impregnated sulphate of iron.

Experiments decisive of the absorption of oxigen gas.

In proceeding to detail the following experiments, which appear to me decisive of the absorption of oxigen gas, I am compelled to observe, I have found it impossible to vary and continue some of them to the extent I intended, having been often interrupted by the sudden intense coldness of the weather, occasional illness, and the indispensable concerns of business. In most of them, where it was at all necessary, the usual corrections, according to the calculations of Gay Lussac, for change of temperature and pressure were made, and for this purpose the barometer and thermometer at the beginning of every experiment and analysis were duly noted.

Mr. Pepys's eudiometer.

Sincerely wishing the little experience I may have acquired in this sort of manipulation should be serviceable to others just entering upon the same laudable pursuits, I take the liberty here of strongly recommending the eudiometrical apparatus of Mr. Pepys, as the easiest and most correct that can be used for the analysis of gasses. When accurately made, and the precautions and directions adopted as stated in your Journal, vol. XIX, p. 86, scarcely any obstacle intervenes to prevent its being managed with facility.

Precautions.

Great attention should however be paid, when filling the elastic gum bottle with the eudiometric liquor, to the expelling from it every bubble of air; which I have found can be effectually done no other way, than by frequently pressing the bottle in a vertical position, keeping the end of

of the bent tube in the liquor the whole time, and suffering it to resume its proper form very slowly. Care should also be taken during the operation, to hold the apparatus firmly at the junction of the tubes with one hand, or cautiously with both; as, when the greater part or the whole of the gas is likely to be absorbed, and it goes on rapidly, the graduated tube will, in consequence of the pressure, sometimes fly off violently from the other, and perhaps be broken. In making the impregnated solution of sulphate of iron with nitrous gas, I dissolve good soft iron in small pieces to saturation, in the purest sulphuric acid I can get, diluted, with about twice its weight of water. The nitric acid is more manageable than the nitrous, and preferable for procuring the nitric oxide to impregnate the iron sulphate, which may be easily done with a wide mouthed bottle in a common basin.

Eudiometric solution.

It sometimes happens, when analysing air containing but little oxigen gas, a great deal of nitric oxide is extricated; much more than can be contained in the graduated tube, so that some difficulty arises in attempting to transfer it. In such a case I suffer all the gas to ascend into the elastic bottle, then under mercury take out the tube, fill it with the sulphated solution of iron, replace it, and thus the nitric oxide is again separated, and the experiment completed; care being taken during the time to hold the bottle in such a position, as will prevent the escape of any air.

Caution when the proportion of oxigen in the air is small.

Exp. 8. The 14th of March, temp. 40°, press. 29·95. In these processes it may not be unnecessary to mention, that the jars used were graduated with the nicest accuracy into cubic inches and tenths, by putting into them repeatedly the weight of these measures in grains of quicksilver, and then drawing a line with the diamond. The internal diameter of the largest is not more than 2 inches, and of the others about an inch. A quantity of freshly germinating barley, weighing 760 grs., the radicles protruding about a quarter of an inch, were conveyed in a coarse gauze bag through the mercury into one of these jars inverted, containing 17·20 cubic inches of oxigen gas, prepared from the oxigenated muriate of potash, and of 97 per cent purity; the greatest pains being taken, when the seeds were under

Experiment with germinating barley in oxigen gas.

under the mercury, to exclude the atmospheric air from the bag as much as possible, by pressing and turning it round many times. After the seeds were in the jar, the bulk of the whole was increased to 20·11 cubic inches. I had no opportunity of making any observation for some days. On the 21st of March it stood at 19·59, and the next day at 19·98, the difference of temp. and press. being allowed. A part of the air being then conveyed to the eudiometer, and washed with limewater, 87 per cent disappeared, leaving a residue of 13 parts; evidently showing, that the whole of the oxigen gas was not expended. To corroborate this suspicion, I made several trials with the impregnated solution of iron, but owing to the test not being properly prepared, as I found that it acted on the quicksilver, which it should not have done, the results were so anomalous and contradictory, I forbear to state them.

Germinating beans in oxigen gas.

Exp. 9. The 18th of March, temp. 46°. press. 29·80. Eleven germinating beans, weighing 508·3 grs., were passed up a jar containing 6·20 cubic inches of oxigen gas of 99 per cent purity. After the beans were in, the scale indicated 7·66 cubic inches.

In 24 hours it had diminished to 6·80 cubic inches.
In 24 hours more to .... 6·50

On the 24th of March the gas had considerably increased, and upon trial with limewater 88·20 parts in 100 were absorbed. The beans were then taken out, and on being weighed were found to have lost 6·90 grs.

Germinating pease in oxigen gas.

Exp. 10. The 19th of March, temp. 48°, press. 29·72. Twenty germinating pease, weighing 125·5 grs., were placed in an inverted jar, containing 1·60 cubic inches of oxigen gas of 99 per cent purity. When the pease were in, the whole indicated by the scale 1·95 cubic inches.

In 24 hours it had diminished to .. 1·70
In 12 hours more to .... 1·67
In 24 hours more it had increased to 1·78

On the 24th of March it had increased some inches, and on a portion being examined with limewater, 94 per cent disappeared.

 These

These pease were then passed up a jar *filled with mercury*, and in three days produced 2 cubic inches of gas, 98 per cent of which were absorbed by the same test. Another portion, formed afterward, gave a similar result.

Barley just beginning to germinate in oxigen gas.

Exp. 11. The 19th of March, temp. 48°, press. 29·72. Some freshly germinating barley, weighing 1127 grains, radicles just bursting forth, were placed in a gauze bag, as in Exp. 8, in 24 cubic inches of oxigen gas of 99 per cent purity. When the barley was in, the scale indicated 27·10 cubic inches. In 24 hours it had diminished to 26·70, and in 12 hours more to 26·15. In transferring some of the gas for trial, an accident prevented the farther pursuit of the experiment; but that being exposed to limewater, 34·50 per cent only disappeared.

Germinating pease in oxigen gas, in the dark,

Exp. 12. The 24th of March, temp. 54°, p. 29·34. Some germinating pease, weighing 114·70 grs., were carefully passed up an inverted jar A, covered with brown paper, containing 3·75 cub. in. of oxigen gas quite pure (an inch of it being previously exposed to the test, only a very small bubble remained, hardly appreciable.) When the pease were in, the graduated scale indicated 4·10 cub. in.

In two days in jar A, it had decreased to	3·90
In three days more it had increased to ··	4·20
And the next day to ················	4·60

The gas being now exposed to lime water, 94 per cent were absorbed; and the pease, on being placed in the balance, had lost some grains in weight as before.

and in the light.

The same weight of pease was placed in jar B exposed to light in 3·77 cub. in. of oxigen gas. After the pease were in, it stood at 41·10 cub. in.

In two days jar B stood at ············	3·92
In three days it was increased to ······	4·20
And next day to ·····················	4·60

Being now tried with lime water, 93 per cent were absorbed; and the pease, being weighed, had lost two grains only.

Germinating beans in oxigen gas, in the dark,

Exp. 13. The 13th of April, temp. 46°, press. 28·90. In jar A, inverted in mercury, and covered with a wrap-

per

per of brown paper to exclude the light, containing 2·30 cub. in. of oxigen gas of purity 28 per cent, were placed 6 freshly germinating garden beans. The scale then indicated 3·20 cub. inches.

In jar B, in the same situation, but the light not excluded, the same number of beans were passed up into 2·35 cub. inches of oxigen gas of like purity. The scale then indicated 3·30 cubic inches.

and in the light.

In three days it had decreased in jar A, to	2·80
In the same time in jar B, to············	2·50

The air in jar A being now exposed to lime water, 66·30 per cent were taken up; and of that in jar B 55·50 per cent. The residues being afterward submitted to the impregnated sulphate of iron, the quantity absorbed in each was proportionate to the oxigen gas not consumed, both having about five per cent, which appeared to be nitrogen.

Germinating beans in oxigen gas in the dark & in the light.

Exp. 14. The 16th of April, temp. 50°, press. 28·90.

In jar A, covered as before, containing 4·60 cub. in. of pure oxigen gas, 10 germinating garden beans were placed. After they were in, the scale indicated 6·15. In jar B, exposed to light, were also put 9 beans, in 5 cub. in. of the same gas; the scale then indicating 6·85 cub. inches.

In three hours the scale of jar A indicated		6·06
	of jar B········	6·56
On the 18th of April··	jar A············	5·90
	jar B············	6·30
On the 21st····	jar A had increased to	6·10
	jar B to··············	6·80
On the 22d····	jar A to··············	6·40
	jar B to··············	7·20
On the 23rd····	jar A to··············	6·75
	jar B to··············	7·70
On the 24th····	jar A to··············	7·55
	jar B to··············	8·70

6·15 cub. in. being now taken out of jar A, and exposed to solution of caustic potash, 4·75 were absorbed; and of 7 cub. in. out of jar B, the same test took up 5·20 cub. in.

The residue of jar A being submitted to the usual test for oxigen gas, 12.04 out of 100 parts were absorbed: and the residue of jar B being also tried, 18·68 per cent disappeared.

No hidrogen found.

To discover whether any hidrogen gas were present, the portions left were attempted to be inflamed, but not the least sign of it appeared.

The beans were afterward sown, and though the weather proved very unfavourable, some of them continued to vegetate, and are now in blossom.

Nitrogen emitted in germination.

From the quantity of nitrogen left, I am still farther confirmed in the idea, that a little is emitted from the seed in germination, particularly with those of the pulse kind.

Germinating pease in oxigen gas.

Exp. 15. The 16th of April, temp. 50°, press. 28·90.

Fifty germinating pease were placed as above in 2·05 of the same oxigen gas.

The whole then indicated	2·80 cub. in.
In two hours it had decreased to	2·60
On the 18th April, to	2·00
——— 19th it had increased to	2·60
——— 20th	2·80
——— 21st	3·05
——— 22d	3·70
——— 24th	4·60
——— 25th	5·10

4·40 Cub. in. being exposed to caustic potash, only one tenth of a cubic inch remained, which, on being submitted to the test for oxigen gas, was not determined.

Exp. 16. The 19th of May, 1809, temp. 65°, p. 29·50.

Germinating pease in oxigen gas.

Thirty germinating pease, with radicles from half to three quarters of an inch long, were conveyed into an inverted jar containing five cubic inches of oxigen gas of the purity of 92 per cent.

After

After the pease were in, it stood at ...... 6·70
In four hours it had decreased to......... 6·00
In four hours more to.................. 5·50
In three hours more to................. 5·10
And in twelve hours more it had increased to 7·00

Six cubic inches of the air being now transferred for examination, 95 per cent were absorbed. The residue tested with impregnated sulphate of iron remained unaltered.

Oxigen gas always absorbed at the commencement,

From experiment 8 to this last it appears evident, that, when germinating seeds are first placed in oxigen gas, a considerable absorption takes place, the quantity of which is much influenced by the state of the seeds, and the temperature of the atmosphere. As all I wish to establish is this simple fact, I have not been anxious as to the minor particulars, or in entering into any tedious aud unnecessary calculations, only in instances where the difference of temperature and pressure made it unavoidable; and in such the proper allowances were made, as I have before stated.

In the last experiment it is most decisive, and to an extent beyond any thing to be accounted for by the condensation supposed to ensue from the conversion of oxigen gas and carbon into carbonic acid gas.

not to be accounted for by forming carbonic acid.

It is also demonstrated, that, if the seeds be suffered to remain sufficiently long, the whole of the oxigen gas disappears, and the carbonic acid gas notwithstanding still continues to be produced. But if the air be examined when arrived at the original quantity, after the decrease, a portion of the oxigen gas may still be discovered, contradicting at once the statement of the sameness in quantity of the carbonic acid gas formed, and the oxigen gas consumed.

The whole of the oxige. disappears, not to form carbonic acid.

In conducting experiments 12, 13, and 14, I thought it might not be superfluous to institute a comparison between the process of germination in the dark and in the light, all other circumstances being as nearly as possible the same: and from an attentive examination and consideration of the results I cannot find any material difference, but what may be readily accounted for by the difference of moisture in the seeds, or some other unknown trifling incident. Here the water was confined to the seeds; but when they are exposed in

Action of light unimportant.

Evaporation soon kills seeds.

in the open air, and in dry weather, the evaporation from them is rapid, they soon become corrugated, all vital action ceases, and they consequently die. In this manner only can the difference be satisfactorily accounted for; as it is self-evident that the evaporation must be quicker in the light than the shade, the temperature on account of reflected heat being generally much higher; and I have often seen barley seeds vegetate to a considerable height in the dark, when, if they had been thrown to the light, they would have been soon parched up.

Water shows the formation of carbonic acid, and absorption of oxigen.

By the results of the above experiments I am well aware, that if the seeds, be suffered to remain long enough in the oxigen gas, it at length is all absorbed. This is also easily shown by placing the jars containing the seeds over water: the carbonic acid gas is then gradually taken up by the water, which ascends in the jar, till no more oxigen gas remains. I have sometimes placed large quantities of germinating barley in narrow jars containing from one to three gallons of atmospheric air, and suffered them to remain over water many months. When the remaining air has been tried with the test for oxigen gas, none has been found, nor any trace of any other gas than nitrogen; and this method may be adopted for procuring this gas for experimental purposes, when not wanted in a hurry. It is certainly too a b[illegible] eudiometrical way [illegible] ascertaining the quantity of oxigen gas in atmospheric air, than that of absorption by water sometime since suggested.

Absorption of oxigen.

In referring particularly to experiment 15, it will be seen, that the absorption of oxigen gas in eleven hours was 1·60 cub. in., being nearly one third of the whole quantity employed. This evidence appears to be irresistible, and is beyond what I could have reasonably expected. I have already made a few trials to the same purpose in vegetation and respiration, and hitherto with similar results, which as soon as concluded I shall take the liberty of laying before you. I shall at the same time make some remarks on fermentation.

I remain,

Dear Sir, yours &c.

J. ACTON.

XII.

XII.

Analysis of the Kaneels'ein; by Professor LAMPADIUS *.

THE kaneelstein has always been considered as a species of jacinth. Its colour is orange, approaching that of cinnamon, whence Werner gave it this name. Its analysis by Prof. Lampadius leaves no doubt, that it is a variety of the jacinth. He obtained from it Analysis of kaneelstein.

Silex	42·3
Zircon	28·8
Alumine	8·6
Potash	6·0
Lime	3·8
Oxide of iron	3·0
Loss by calcination	2·6
Loss	4·4
	100·0

This analysis shows, that it does not contain much more than one fourth of zircon, while the jacinth contains 0·69.

XIII.

Observation of a Lunar Rainbow; by L. CORDIER, *Mine Engineer* †.

I Was lately witness of a pretty rare phenomenon, a rainbow in the night. The 13th of this month, August 1807, I was standing with several persons on an eminence, that commanded a view of the horizon. We had near us, to the north, the tail of a storm, that poured down a copious rain. At the same time the sky cleared up toward the south, and the moon, nearly at full, appeared. A fine luminous bow then appeared on the storm; but, though it was well defined, the seven primary colours were scarcely to be distinguished in it. They seemed as if drowned in a tint of pale yellow. What struck us particularly was, that the whole of the circle encompassed by the bow was luminous, and tinged with a similar yellow hue, though less intense. Lunar rainbow.

* Journal de Physique, vol. LXV, p. 32. † Ibid, p. 208.

XIV.

XIV.

On the Want of Tables of the Proportions of the constituent Principles of Salts, and on the Luminous Smoke from Lead Smelting-Houses. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Tables of the proportions of the constituent parts of salts would be highly useful.

THERE are few tables more useful to a chemical inquirer, than such as point out the proportions of the constituent parts of salts: not only the philosophic but the practical chemist also would be equally benefited, by having a collection of tables of this description to refer to; and it is I think a matter of surprise, that no person has attempted to publish such upon a scale sufficiently extensive, to answer the purpose of general reference. I was in hopes, that the last edition of your Dictionary would have contained, among its other valuable additions, tables of this kind*; and it may not perhaps be improper to suggest, that this omission may in some measure be supplied by inserting from time to time in your interesting journal, as opportunity of collecting the requisite materials may afford, an alphabetical list of salts, with the proportion of their ingredients agreeably to the latest researches. Such an addition, while it would render an essential service to many of your readers, would not a little increase the value of your Journal.

Luminous smoke from smelting lead ore.

I have observed, that the white smoke that arises from a lead furnace during the process of smelting the ore continues luminous at night for a great length of time after it has left the chimney: sometimes I have seen the smoke retain this luminous appearance until it has been quite dissipated. Your explanation of this phenomenon will oblige, Sir,

Your most humble Servant,

May 6*th*, 1809. J. S. K.

* In table II at the end of the Dictionary, that of Compounds consisting in general of more than two Principles, the proportions, where they had been ascertained with any accuracy, were given from the best authorities.

I am

I am inclined to think, that the luminous smoke arises from sulphur driven up in the first state of combustion. For sulphur, like phosphorus, may be burned with two kinds of flame, the first not visible in day-light, at less than 300°, as I conjecture, and not capable of setting fire to the smallest thread or vegetable fibre, and the latter much brighter, and generally known. From sulphur in its first state of combustion.

W. N.

SCIENTIFIC NEWS.

THE Russian minister for the home department has communicated to the Imperial Academy of Petersburg the following account of a meteoric stone, weighing about 160 lbs, that fell in the circle of Ichnow, in the government of Smolenske. Meteoric stone in Russia, 13 March, 1807.

In the afternoon of the 13th of March, 1807, a very violent clap of thunder was heard in that district. Two peasants in the village of Timochim, being in the fields at the time, say, that at the instant of this tremendous report they saw a large black stone fall about forty paces from them. They were stunned for a few minutes, but, as soon as they recovered themselves, ran toward the place where the stone fell. They could not discover it however, it had penetrated so deep into the snow. On their report several persons went to the spot, and got out the stone, which was above two feet beneath the surface of the snow. It was of an oblong shape, blackish like cast iron, very smooth on all parts, and on one side resembling a coffin. On its flat surfaces were very fine radii resembling brass wire. Its fracture was of an ashen gray. Being conveyed to the gymnasium of Smolensko, a professor of natural philosophy there considered it at once as ferruginous, from the simple observation of its being extremely friable, and staining the fingers. The particles of which

which it is composed contain a great deal of lime, and of sulphuric acid.

Several meteoric stones in Italy, 19th April, 1808.

On the 19th of April, 1808, at one o'clock in the afternoon, a great quantity of meteorolites fell in the commune of Pieve di Casignano, in the department of Taro (formerly the duchies of Parma and Placentia). The air was calm, and the sky serene, but with a few clouds. Two loud explosions were heard, followed by several less violent, after which several stones fell. A farmer, who was in the fields, saw one fall about fifty paces from him, and bury itself in the ground. It was burning hot. A fragment of one of these stones is deposited in the museum at Paris.

Peculiar claw in the beaver.

On the 17th of November, 1807, during an inundation of the Rhone, a beaver was killed in the island of la Barthalasse, opposite Avignon. Mr. Costaing has given a very particular description of the animal, and among other things remarks, that the fourth toe of each hind paw has a double nail, the parts of which close on each other, so as to form a sharp and cutting beak, opening and shutting like that of a bird of prey.

Bees poisoned by the effluvia of the rhus vernix.

A large swarm of bees, having settled on a branch of the poison ash, *rhus vernix*, in the county of West Chester in America, was taken into a hive of fir at three o'clock in the afternoon, and removed to the place where it was to remain at nine. About five the next morning the bees were found dead, swelled to double their natural size, and black, except a few, which appeared torpid and feeble, and soon died on exposure to the air.

Cotton tree introduced into France.

The cultivation of the cotton tree, as well as of the sweet potato from St. Domingo, has been introduced in the southern departments of France, it is said very successfully.

Paper from mountain flax.

Mrs. Lena Serpenti, of Como, to whom an honorary medal was decreed in 1806 for having improved the method of spinning amianthus, has fabricated paper from this fossil, that answers well either for writing or printing, and is capable of resisting the action of fire or water.

Metallic thermometer.

Mr. Urban Joergensen has presented to the Copenhagen Society of Rural Economy a metallic thermometer of his invention, in the shape of a watch. The scale, on a circle on the

the dial-plate, is graduated to 80° of heat and 40° of cold; and the temperature is pointed out by a hand from the centre.

Mr. Creve of Wisbaden has discovered a method of recovering wine that has turned sour. For this purpose he employs powdered charcoal. The inhabitants of the banks of the Rhine have bestowed on him a medal, as a reward for this discovery. Sour wine sweetened by coarcoal.

Mr. Ljung, a Swedish naturalist, has discovered a new species of mouse, which he has named *sorex caniculatus*. It is the smallest animal known of the mammiferous class, weighing only about half a drachm. Diminutive quadruped.

Mr. Lacepede has lately given a minute description of an oviparous quadruped, not hitherto noticedby any naturalist, but preserved in the Museum of Natural History. He classes it in the genus proteus, or that of salamander, distinguishing it by the name of tetradactylus from the number of its toes. New quadruped.

A German chemist is said to have discovered another new metal among the grains of platina, to which he gives the name of *vestium*. New metal.

Counsellor Koehler, of Moscow, is busily employed in cleaning the old coins he is continually receiving from the Crimea. He is publishing a collection of more than 600 kings or cities, all belonging to Grecian colonies, or kingdoms, that extended along the northern and western coasts of the Black Sea. Ancient coins.

The University of Leipsic has resolved, that the stars belonging to the belt and sword of Orion, as well as the intermediate stars, which have yet received no particular name, shall in future be called the Stars of Napoleon, or the Constellation Napoleon. New constellation.

A Voyage of Discovery to the Countries of the South, by Order of his Majesty the Emperor Napoleon, in the sloops Geographe and Naturaliste, and schooner Casuarina, during the years 1800—1804, compiled by M. F. Péron, Naturalist to the Expedition, is published conformably to a Decree of the Emperor, in 2 vols. 4to, with 41 plates, 28 of them coloured, and 3 large maps. In this work are described the least known parts of van Diemen's Land, the large strait that Voyage of discovery.

that separates it from New Holland, the discovery of the Great Land of Napoleon, the Great Archipelago of Bonaparte, &c.

Index to Buffon. Prof. Sue has published an Analytical and Systematic Index to Sonnini's new edition of Buffon, with an index to the names of authors. The index occupies 3 vols. 8vo., and was highly necessary to a work in 124 vols.

Index Mem. of French Academy. Mr. Demours published an Index to the Memoirs of the French Academy of Sciences in 9 vols. 4to, each volume including ten years of the Memoirs. Mr. Cotte is now employed on a tenth volume, which will make the index complete from the commencement to the year 1790, with which these Memoirs finished.

Capillary pen. A Mr. Baradelle has constructed a pen, which he terms capillary, capable of tracing 144 lines in the space of a French inch.

Dublin Society.

Dublin Society. AT a meeting of this Society, at their house in Hawkins Street, on the 11th of May, various resolutions were passed.

American fir to be compared with that of Europe. It having been suggested to the Society, that the timber imported from North America differs very materially in quality and strength from the timber, which has for many years past been used in this kingdom: it was resolved,

That a committe be appointed to inquire into the truth of the above suggestion; and to report to the Society on the comparative strength of Norway and Memel timber, with that of the timber of North America, in which the committee will distinguish the particular states of North America, whence the timber may have been imported, the comparative qualities of which with those of Memel and Norway shall be reported upon.

Mr. J. L. Foster presented the following report from the committee of chemistry:

Catalogues of Irish minerals. The committee of chemistry and mineralogy, to whom it was referred to report upon Mr. Higgins's manuscript catalogues of Irish minerals, have proceeded to take the same into consideration, and are decidedly of opinion, that it will

not

not be expedient to incur the expense of printing any catalogues, until the collections themselves shall have been rendered much more perfect than they are at present. They are farther of opinion, that the nature of these collections requires, that the catalogues should be arranged according to the topographical situation of the specimens, rather than by a systematic distribution into classes; but adverting to the great labour, which would attend the making a catalogue on so opposite a principle from that which has been adopted, they merely recommended for the present, that the professor of chemistry and mineralogy be directed to add a topographical index to the catalogue of each country, specifying under the names of the different places, that are mentioned in the catalogue, the numbers of the various specimens, which have been brought from it.

Geological and mineralogical survey.

The committee have farther taken into their consideration the resolution of the Society of the 7th day of July last, authorising this committee to offer a premium not exceeding two hundred pounds for the best geological and mineralogical survey of the county of Dublin, to be approved of by them, and sanctioned by the board. The committee find, that no person has become a candidate for executing the task that has been thus proposed; and they recommend, that the proposal itself be discontinued. The committee are further of opinion, that the division into counties is, in many instances, an inconvenient mode of assigning a district proposed for mineralogical survey; and they should recommend in preference an attention to the great lines of geological character, which have been traced out by nature. Of these they know of none more interesting than that which marks the coal district in the vicinity of Kilkenny, comprising some portion of each of the three countries of Carlow, Kilkenny, and the Queens-county.

Coal district of Kilkenny.

The committee are of opinion, that no measure would conduce more eminently to the advancement of the agriculture, manufactures, and general commerce of this country, than a complete and scientific survey of its mineral productions; but such a survey as the committee allude to would require a degree of geological science and practical knowledge, such as is possessed by very few, and, if extend-

ed

ed to the whole of Ireland, would demand an expe
beyond the means of the Society.

Still, however, they think it an object well worthy o
tion, to make a beginning, to choose some limited d
to give it in charge to some person of undisputed s
to request from him a map on a large scale, drawn
view to represent the mineralogical characters of t
trict, accompanied with sections of the strata, parti
in the vicinity of mines, elucidated by a copious m
and accompanied with collections of specimens of th
cipal substances referred to. If such a beginning we
obtained, printed, and circulated by the Society, it
serve as a useful pattern for further undertakings;
executed with that degree of science, which the com
flatter themselves with being able to obtain, might p
appear of such national importance, as to obtain for
ciety more ample funds for its further prosecution.

The committee have thought it their duty to c
which of their present funds are more particularly a
ble for the purpose, and in the first place they prop
application of the £200, which had been appropri
the execution of the survey, which they have alre
commended should be relinquished. A more ampl
seems to be available in a part of the £1300 reserved
estimate toward the completing the statistical surveys
County surveys. thirteen counties which remain to be undertaken. O
they understand but two or three are in any forwa
and unless the execution of the remainder should b
superior to that of many of those which have alread
obtained, the committee are of opinion, that such a
as they now propose would be an application of the f
the Society incalculably more beneficial.

In selecting a person for the undertaking, the ch
necessarily confined among very few. The committ
of opinion, that Mr. Richard Griffith Jun. is eminent
lified for the undertaking; and to him (subject to
probation of the Society) they proposed to und
it. The committee could have wished to make an a
ment with Mr. Griffith with respect to the amount
muneration, which he should finally receive; but on s

ing to him, (subject to the approbation of the Society) to accept of the £200 above alluded to, as soon as his map and memoir should be executed and approved of. Mr. Griffith on one hand setting a higher value on his time as a professional man, than the Society could at present afford to give, but on the other hand not desiring to make this undertaking an object of emolument, prefers to submit to the Society, at the completion of the work, an account of the mere expenses incurred in its prosecution, proposing that the Society should discharge the amount on accepting of his work; and your committee, considering the great liberality of the proceeding on the part of Mr. Griffith, and the great advantages which may be expected from its execution, earnestly recommend it to the adoption of the Society.

Mr. Leslie Foster, who made the report, stated, that he conceived the committee were fully justified in selecting Mr. Griffith for this undertaking, as he had heard the late Mr. Greville, one of the first mineralogists in Europe, who was a patron of the Geological Society in London, and Vice-president of the Royal Society, declare, that Mr. Griffith's professional acquirements, as a mineralogical engineer, rendered him fitter than any other man in Great Britain or Ireland, that he was acquainted with, to make a mineralogical survey; as such an undertaking required an intimate knowledge of the most approved methods of working mines, as well as of the sciences of geology and mineralogy. The survey undertaken by Mr. R. Griffith.

This report of the committee was adopted accordingly, and confirmed by the society at large.

Mr. Davy intends to visit Dublin next winter, and give a course of electio-chemical lectures on his late discoveries. Mr. Davy.

ERRATA.

Page.	Line.	
139,	24,	*For* p. 133, *read* pages 133 and 158.
158,	16	—— of —— by.
179,	28	—— muriatic—— muriate.
180,	note	*before* the proportions *add* reversed.
181,	lines 1 & 3 from bot.	*for* sulphate of *read* sulphuric.

METEOROLOGICAL JOURNAL

***For JUNE,* 1809,**

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

MAY Day of	THERMOMETER.				BAROMETER, 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.		Day.	Night.
27	56	58	62	57	29·60	Fair	Fair
28	58	57	61	53	29·63	Rain	Ditto
29	57	58	65	47	29·56	Ditto	Ditto
30	51	51	61	53	29·82	Ditto	Cloudy
31	49	56	60	51	29·78	Rain	Ditto
JUNE							
1	60	58	72	50	29·50	Fair	Fair
2	51	51	58	50	29·36	Rain *	Cloudy
3	51	56	62	46	30·02	Fair	Fair
4	58	57	71	53	29·68	Rain	Ditto
5	57	53	62	51	29·33	Ditto	Rain †
6	56	55	60	50	29·61	Ditto	Fair
7	54	55	60	50	29·73	Ditto	Ditto
8	55	56	60	51	29·78	Ditto	Cloudy
9	55	56	60	51	29·55	Ditto	Ditto
10	55	56	60	49	29·57	Ditto	Ditto
11	54	55	57	50	29·73	Ditto	Ditto
12	58	56	61	50	30·08	Fair	Ditto
13	58	57	62	55	30 00	Ditto	Fair
14	59	58	67	56	29·97	Ditto	Ditto
15	58	58	64	52	29·88	Ditto	Ditto
16	59	58	66	54	29·95	Ditto	Ditto
17	58	59	69	54	29·82	Ditto	Ditto
18	58	56	63	49	29·82	Ditto	Ditto
19	58	62	67	57	29·90	Ditto	Ditto ‡
20	62	67	73	58	30·02	Ditto	Ditto §
21	62	65	72	59	30.22	Ditto	Cloudy
22	63	63	66	57	30·30	Rain ‖	Fair
23	64	64	73	58	30·31	Fair	Ditto
24	64	64	72	58	30·33	Ditto	Ditto
25	61	58	68	50	30·43	Ditto	Ditto ¶

* Rain, boisterous, and cold, all the forenoon.
† Evening boisterous and very cold.
‡ The Moon obscured at intervals; at 12 dark and windy.
§ Afternoon sultry; in the evening refreshing breezes.
‖ Heat drops.
¶ Great change to cold.

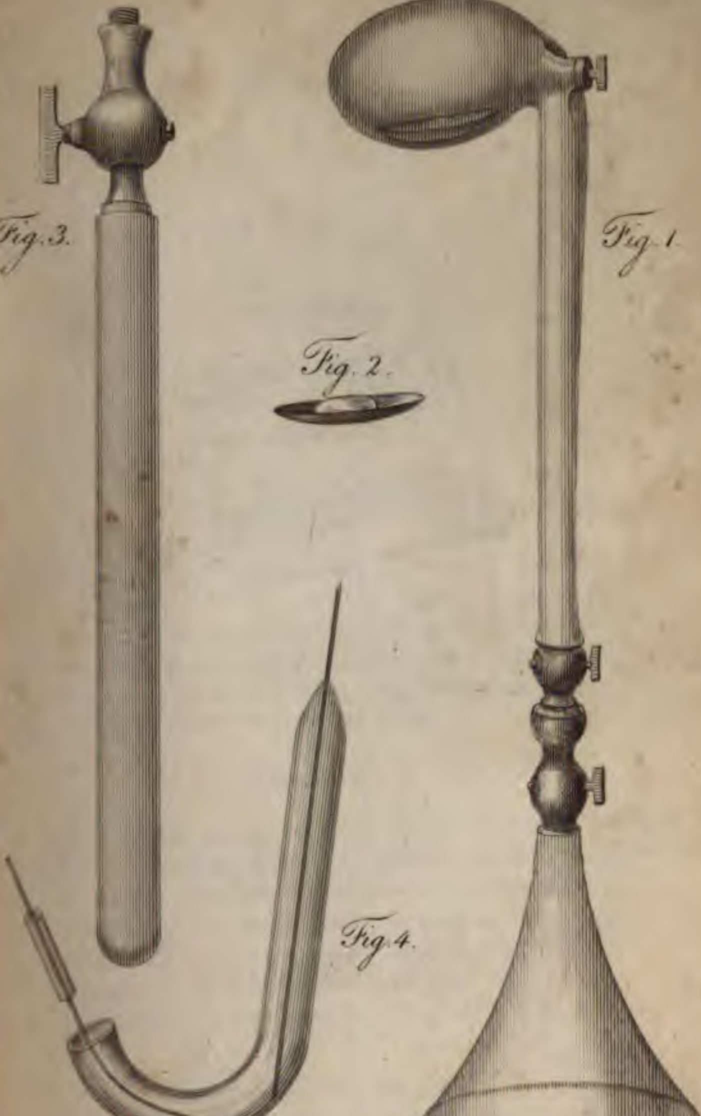


Fig. 3.

Fig. 1.

Fig. 2.

Fig. 4.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY

AND

THE ARTS.

AUGUST, 1809.

ARTICLE I.

The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory. By HUMPHRY DAVY, *Esq. Sec. R.S. F.R.S. Ed. and M.R.I.A.* *

1. *Introduction.*

IN the following pages, I shall do myself the honour of laying before the Royal Society an account of the results of the different experiments, made with the hopes of extending our knowledge of the principles of bodies by the new powers and methods arising from the applications of electricity to chemistry, some of which have been long in progress, and others of which have been instituted since their last session. Object of the experiments.

* Philos. Trans. for 1809, Part I. p. 39.

Their subjects.

The objects which have principally occupied my attention are the elementary matter of ammonia, the nature of phosphorus, sulphur, charcoal, and the diamond, and the constituents of the boracic, fluoric, and muriatic acids.

Among the numerous processes of decomposition, which I have attempted, many have been successful; and from those which have failed some new phenomena have usually resulted, which may possibly serve as guides in future inquiries. On this account, I hall keep back no part of the investigation, and I shall trust to the candour of the Society for an excuse for its imperfection.

The more approaches are made in chemical inquiries towards the refined analysis of bodies, the greater are the obstacles which present themselves, and the less perfect the results.

Pure substances seldom obtained.

All the difficulties, which occur in analysing a body, are direct proofs of the energy of attraction of its constituent parts. In the play of affinities with respect to secondary compounds even, it rarely occurs, that any perfectly pure or unmixed substance is obtained; and the principle applies still more strongly to primary combinations.

First methods imperfect.

The first methods of experimenting on new objects likewise are necessarily imperfect; novel instruments are demanded, the use of which is only gradually acquired, and a number of experiments of the same kind must be made, before one is obtained, from which correct data for conclusions can be drawn.

2. *Experiments on the Action of Potassium on Ammonia, and Observations on the Nature of these two Bodies.*

Oxigen in ammonia.

In the Bakerian lecture, which I had the honour of reading before the Society, November 19, 1807, I mentioned, that in heating potassium strongly in ammonia, I found that there was a considerable increase of volume of the gas, that hidrogen and nitrogen were produced, and that the potassium appeared to be oxidated; but this experiment, as I had not been able to examine the residuum with accuracy, I did not publish. I stated it as an evidence, which I intended to pursue more fully, of the existence of oxigen in ammonia.

In

In a paper read before the Royal Society last June, which they have done me the honour of printing, I have given an account of various experiments on the amalgam from ammonia, discovered by Messrs. Berzelius and Pontin, and in a note attached to this communication, I ventured to controvert an opinion of M. M. Gay Lussac and Thenard with respect to the agency of potassium and ammonia, even on their own statement of facts, as detailed in the Moniteur for May 27, 1808.

Opinions of Gay Lussac and Thenard erroneous.

The general obscurity belonging to these refined objects of research, their importance and connection with the whole of chemical theory, have induced me since that time to apply to them no inconsiderable degree of labour and attention; and the results of my inquiries will, I trust, be found not only to confirm my former conclusions; but likewise to offer some novel views.

Apparatus.

In the first of these series of operations on the action of potassium on ammonia, I used retorts of green glass; I then, suspecting oxigen might be derived from the metallic oxides in the green glass, employed retorts of plate glass, and last of all, I fastened the potassium upon trays of platina, or iron, which were introduced into the glass retorts furnished with stop cocks. These retorts were exhausted by an excellent air pump, they were filled with hidrogen, exhausted a second time, and then filled with ammonia from an appropriate mercurial gas holder*. In this way the gas was operated upon in a high degree of purity, which was always ascertained; and all the operations performed out of the contact of mercury, water, or any substances that could interfere with the results.

Potassium used.

I at first employed potassium procured by electricity; but I soon substituted for it the metal obtained by the action of

* A representation of the instruments and apparatus is annexed.

Pl. VII. fig. 1. The retort of plate glass for heating potassium in gasses.

Fig. 2. The tray of platina for receiving the potassium.

Fig. 3. The platina tube for receiving the tray in experiments of distillation.

Fig. 4. The apparatus for taking the voltaic spark in sulphur and phosphorus.

ignited iron upon potash, in the happy method discovered by M. M. Gay Lussac and Thenard, finding that it gave the same results, and could be obtained of a uniform quality *, and infinitely larger quantities, and with much less labour and expense.

Potassium brought into contact with ammonia.

When ammonia is brought into contact with about twice its weight of potassium at common temperatures, the metal loses its lustre and becomes white, there is a slight diminution in the volume of the gas; but no other effects are produced. The white crust examined proves to be potash, and the ammonia is found to contain a small quantity of hidrogen, usually not more than equal in volume to the metal.

Heated in the gas.

On heating the potassium in the gas, by means of a spirit lamp applied to the bottom of the retort, the colour of the crust is seen to change from white to a bright azure, and this gradually passes through shades of bright blue and green into dark olive. The crust and the metal then fuse together; there is a considerable effervescence, and the crust, passing off to the sides, suffers the brilliant surface of the potassium to appear.

Cooled.

When the potassium is cooled in this state it is again covered with the white crust.

Heated a second time.

By heating a second time, it swells considerably, becomes porous, and appears crystallized, and of a beautiful azure tint; the same series of phenomena, as those before described, occur in a continuation of the process; and it is finally entirely converted into the dark olive coloured substance.

Hidrogen? evolved, and ammonia disappears.

In this operation, as has been stated by M. M. Gay Lussac and Thenard, a gas, which gives the same diminution by detonation with oxigen as hidrogen, is evolved, and ammonia disappears.

The proportion of the ammonia, which looses its elastic form,

The potassium probably contaminated with a little iron.

* When the potash used for procuring potassium in this operation was very pure, and the iron turnings likewise very pure and clean, and the whole apparatus free from any foreign matters, the metal produced differed very little, in its properties, from that obtained by the Voltaic battery. Its lustre, ductility, and inflammability were similar. Its point of fusion and specific gravity were, however, a little higher, it requiring nearly 130° of Fahrenheit to render it perfectly fluid, and being to water as 7960 to 10000, at 60° Fahrenheit. This I am inclined to attribute to its containing a minute proportion of iron.

form, as I have found by numerous trials, varies according as the gas employed contains more or less moisture.

Thus eight grains of potassium, during its conversion into the olive coloured substance, in ammonia saturated with water at 63° Fahrenheit, and under a pressure equal to that of 29·8 inches of mercury, had caused the disappearance of twelve cubical inches and a half of ammonia; but the same quantity of metal acted upon under similar circumstances, except that the ammonia had been deprived of as much moisture as possible by exposure for two days to potash that had been ignited, occasioned a disappearance of sixteen cubical inches of the volatile alkali. Less of the gas disappears when moist.

Whatever be the degree of moisture of the gas, the quantities of inflammable gas generated have always appeared to me to be equal for equal quantities of metal. M. M. Gay Lussac and Thenard are said to have stated, that the proportions in their experiment were the same as would have resulted from the action of water upon potassium. In my trials, they have been rather less. Thus, in an experiment conducted with every possible attention to accuracy of manipulation, eight grains of potassium generated, by their operation upon water, eight cubical inches and a half of hidrogen gas: and eight grains from the same mass, by their action upon ammonia, produced eight cubical inches and one eighth of inflammable gas. This difference is inconsiderable, yet I have always found it to exist, even in cases where the ammonia has been in great excess, and every part of the metal apparently converted into the olive coloured substance. The inflammable gas always in proportion to the metal, and less than results from the action of water.

No other account of the experiments of M. M. Gay Lussac and Thenard has, I believe, as yet been received in this country, except that in the Moniteur already referred to; and in this no mention is made of the properties of the substance produced by the action of ammonia on potassium. Having examined them minutely and found them curious, I shall generally describe them. Properties of the substance produced by the action of ammonia on potassium.

1. It is crystallized, and presents irregular facets, which are extremely dark, and in colour and lustre not unlike the protoxide of iron; it is opaque when examined in large masses,

masses, but is semitransparent in thin films, and appears of a bright brown colour by transmitted light.

2. It is fusible at a heat a little above that of boiling water, and if heated much higher, emits globules of gas.

3. It appears to be considerably heavier than water, for it sinks rapidly in oil of sassafras.

4. It is a nonconductor of electricity.

5. When it is melted in oxigen gas, it burns with great vividness, emitting bright sparks. Oxigen is absorbed, nitrogen is emitted, and potash, which from its great fusibility seems to contain water, is formed.

6. When brought into contact with water, it acts upon it with much energy, produces heat, and often inflammation, and evolves ammonia. When thrown upon water, it disappears with a hissing noise, and globules from it often move in a state of ignition upon the surface of the water. It rapidly effervesces and deliquesces in air, but can be preserved under naphtha, in which, however, it softens slowly, and seems partially to dissolve. When it is plunged under water filling an inverted jar, by means of a proper tube, it disappears instantly with effervescence, and the nonabsorbable elastic fluid liberated is found to be hidrogen gas.

The ponderable matter of the ammonia exists in this product

By far the greatest part of the ponderable matter of the ammonia, that disappears in the experiment of its action upon potassium, evidently exists in the dark fusible product. On weighing a tray containing six grains of potassium, before and after the process, the volatile alkali employed having been very dry, I found that it had increased more than two grains; the rapidity with which the product acts upon moisture prevented me from determining the point with great minuteness; but I doubt not, that the weight of the olive coloured substance and of the hidrogen disengaged precisely equals the weight of the potassium, and ammonia consumed.

Results of Gay Lussac and Thenard.

M. M. Gay Lussac and Thenard * are said to have procured

* No notice is taken of the apparatus used by M. M. Gay Lussac and Thenard in the Moniteur; but from the tenour of the details, it seems that they must have operated in glass vessels in the way heretofore adopted over mercury.

cured from the fusible substance, by the application of a strong heat, two fifths of the quantity of ammonia that had disappeared in their first process, and a quantity of hidrogen and nitrogen in the proportions in which they exist in ammonia, equal to one fifth more.

My results have been very different, and the reasons will, I trust, be immediately obvious. Different from Mr. Davy's.

When the retort containing the fusible substance is exhausted, filled with hidrogen and exhausted a second time, and heat gradually applied, the substance soon fuses, effervesces, and, as the heat increases, gives off a considerable quantity of elastic fluid, and becomes at length, when the temperature approaches nearly to dull redness, a dark gray solid, which by a continuance of this degree of heat does not undergo any alteration. The fusible substance heated in hidrogen.

In an experiment, in which eight grains of potassium had absorbed sixteen cubical inches of well dried ammonia in a glass retort, the fusible substance gave off twelve cubical inches and half of gas, by being heated nearly to redness; and this gass analysed was found to consist of three quarters of a cubical inch of ammonia, and the remainder of elastic fluids, which when mixed with oxigen gas in the proportion of $6\frac{1}{2}$ to 6, and acted upon by the electric spark, diminished to $5\frac{1}{2}$. The temperature of the atmosphere, in this process, was 57° Fahrenheit, and the pressure equalled that of 30·1 inches of mercury. Gas expelled from it by heat.

In a similar experiment, in which the platina tray containing the fusible substance was heated in a polished iron tube filled with hidrogen gas, and connected with a pneumatic apparatus containing very dry mercury, the quantity of elastic fluid given off, all the corrections being made, equalled thirteen cubical inches and three quarters, and of these a cubical inch was ammonia; and the residual gas, and the gas introduced into the tube being accounted for, it appeared, that the elastic fluid generated, destructible by detonation with oxigen, was to the indestructible elastic fluid, as 2·5 to 1. Heated in a polished iron tube filled with hidrogen.

In this process, the heat applied approached to the dull red heat. The mercury, in the thermometer, stood at 62° Fahrenheit, and that in the barometer at 30·3 inches.

In

Similar results in different experiments.

In various experiments on different quantities of the fusible substance, in some of which the heat was applied to the tray in the green glass retort, and in others, after it had been introduced into the iron tube; and in which the temperature was sometimes raised slowly and sometimes quickly, the comparative results were so near these, that I have detailed, as to render any statement of them superfluous.

Difference between the iron tube and green glass retort.

A little more ammonia, and rather a larger proportion of inflammable gas *, were in all instances evolved when the iron tube was used, which I am inclined to attribute to the following circumstances. When the tray was brought through the atmosphere to be introduced into the iron tube, the fusible substance absorbed a small quantity of moisture from the air, which is connected with the production of ammonia. And in the process of heating in the retort, the green glass was blackened, and I found that it contained a very small quantity of the oxides of lead and iron, which must have caused the disappearance of a small quantity of hidrogen.

Effects of moisture.

M. M. Gay Lussac and Thenard, it appears from the statement, had brought the fusible substance into contact with mercury, which must have given to it some moisture: and whenever this is the case, it furnishes by heat variable quantities of ammonia. In one instance, in which I heated the fusible substance from nine grains of potassium, in a retort that had been filled with mercury in its common state of dryness, I obtained seven cubical inches of ammonia as the first product; and in another experiment which had been made with eight grains, and in which moisture was purposely introduced, I obtained nearly nine cubical inches of ammonia, and only four of the mixed gasses.

With a proper quantity of moisture, the original quantity of ammonia would be regenerated.

I am inclined to believe, that if moisture could be introduced only in the proper proportion, the quantity of ammonia generated would be exactly equal to that which disappeared in the first process.

* The average of six experiments made in a tube of iron is 2·4 of inflammable gas to 1 of uninflammable The average of three made in green glass retorts is 2·3 to 1.

This

This idea is confirmed by the trials which I have made, by heating the fusible substance with potash containing its water of crystallization, and muriate of lime partially dried*.

In both these cases, ammonia was generated with great rapidity, and no other gas, but a minute quantity of inflammable gas, evolved, which was condensed by detonation with oxigen with the same phenomena as pure hidrogen.

In one instance, in which thirteen cubical inches of ammonia had disappeared, I obtained nearly eleven and three quarters by the agency of the water of the potash; the quantity of inflammable gas generated was less than four tenths of a cubical inch.

In another, in which fourteen cubical inches had been absorbed, I procured by the operation of the moisture of muriate of lime nearly eleven cubical inches of volatile alkali, and half a cubical inch of inflammable gas; and the differences, there is every reason to believe, were owing to an excess of water in the salts, by which some of the gas was absorbed.

The fusible substance does not produce ammonia by heat alone.

Whenever, in experiments on the fusible substance, it has been procured from ammonia saturated with moisture, I have always found that more ammonia is generated from it by mere heat; and the general tenour of the experiments inclines me to believe, that the small quantity, produced in experiments performed in vacuo, is owing to the small quantity of moisture furnished by the hidrogen gas introduced, and that the fusible substance, heated out of the presence of moisture, is incapable of producing volatile alkali.

Gay Lussac and Thenard.

M. M. Gay Lussac and Thenard, it is stated, after having obtained three fifths of the ammonia or its elements that had disappeared in their experiment, by heating the product;

* If water, in its common form, is brought into contact with the fusible substance, it is impossible to regulate the quantity, so as to gain conclusive results, and a very slight *excess* of water causes the disappearance of a very large quantity of the ammonia generated. In potash and muriate of lime, in certain states of dryness, the water is too strongly attracted by the saline matter to be given off, except for the purpose of *generating* the ammonia.

procured

procured the remaining two fifths, by adding water to the residuum, which after this operation was found to be potash. No notice is taken of the properties of this residuum, which, as the details seem to relate to a single experiment, probably was not examined; nor as moisture was present at the beginning of their operations could any accurate knowledge of its nature have been gained.

Properties of the residuum of the fusible substance after exposure to heat.

I have made the residuum of the fusible substance after it has been exposed to a dull red heat, out of the contact of moisture, an object of particular study, and I shall detail its general properties.

It was examined under naphtha, as it is instantly destroyed by the contact of air.

1. Its colour is black, and its lustre not much inferior to that of plumbago.

2. It is opaque even in the thinnest films.

3. It is very brittle, and affords a deep gray powder.

4. It is a conductor of electricity.

5. It does not fuse at a low red heat, and when raised to this temperature, in contact with plate glass, it blackens the glass, and a grayish sublimate rises from it, which likewise blackens the glass.

6. When exposed to air at common temperatures, it usually takes fire immediately, and burns with a deep red light.

7. When it is acted upon by water, it heats, effervesces most violently, and evolves volatile alkali, leaving behind nothing but potash. When the process is conducted under water, a little inflammable gas is found to be generated. A residuum of eight grains giving in all cases about $\frac{30}{100}$ of a cubical inch.

8. It has no action upon quicksilver.

9. It combines with sulphur and phosphorus by heat, without any vividness of effect, and the compounds are highly inflammable, and emit ammonia, and the one phosphuretted and the other sulphuretted hidrogen gas, by the action of water.

A compound of nitrogen with suboxide of potassium.

As an inflammable gas alone, having the obvious properties of hidrogen, is given off during the action of potassium upon ammonia, and as nothing but gasses apparently the same

same as hidrogen and nitrogen, nearly in the proportions in which they exist in volatile alkali, are evolved during the exposure of the compound to the degree of heat which I have specified; and as the residual substance produces ammonia with a little hidrogen by the action of water, it occurred to me, that, on the principles of the antiphlogistic theory, it ought to be a compound of potassium, a little oxigen and nitrogen, or a combination of a suboxide of potassium and nitrogen; for the hidrogen disengaged in the operations of which it was the result nearly equalled the whole quantity contained in the ammonia employed; and it was easy to explain the fact of the reproduction of the ammonia by water, on the supposition, that by combination with one portion of the oxigen of the water, the oxide of potassium became potash, and by combination with another portion and its hidrogen, the nitrogen was converted into volatile alkali.

With a view to ascertain this point, I made several experiments on various residuums, procured in the way that I have just stated, from the action of equal quantities of potassium on dry ammonia in platina trays, each portion of metal equalling six grains. **Experiments to prove this.**

In the first trials, I endeavoured to ascertain the quantity of ammonia generated by the action of water upon a residuum, by heating it with muriate of lime or potash partially deprived of moisture; and after several trials, many of which failed, I succeeded in obtaining four cubical inches and a half of ammonia. In three other cases, where there was reason to suspect a small excess of water, the quantities of ammonia were three cubical inches and a half, three and eight tenths, and four and two tenths. **Quantity of ammonia produced.**

These experiments were performed in the iron tube used for the former process; the tray was not withdrawn; but the salt introduced in powder, and the apparatus exhausted as before, then filled with hidrogen, and then gently heated in a small portable forge.

Having ascertained what quantity of ammonia was given off from the residuum, I endeavoured to discover what quantity of nitrogen it produced in combustion, and what quantity of oxigen it absorbed. The methods that I employed, **The compound introduced into oxigen gas.**

ployed, were by introducing the trays into vessels filled with oxigen gas over mercury. The product often inflamed spontaneously, and could always be made to burn by a slight degree of heat.

Oxigen absorbed and nitrogen evolved.

In the trial that I regard as the most accurate, two cubical inches and a half of oxigen were absorbed, and only a cubical inch and one tenth of nitrogen evolved.

Surprised at the smallness of the quantity of the nitrogen, I sought for ammonia in the products of these operations; but various trials convinced me, that none was formed. I examined the solid substances produced, expecting nitrous acid; but the matter proved to be dry potash, apparently pure, and not affording the slightest traces of acid.

The quantity of nitrogen existing in the ammonia, which this residuum would have produced by the action of *water*, supposing the volatile alkali decomposed by electricity, would have equalled at least two cubical inches and a quarter.

Exposed to nascent oxigen, still the quantity of nitrogen small.

I heated the same proportions of residuum with the red oxide of mercury, and the red oxide of lead in vacuo, expecting that when oxigen was supplied in a gradual way, the result might be different from that of combustion; but in neither of these cases did the quantity of nitrogen exceed a cubical inch and a half.

But on what could this loss of nitrogen depend; had it entered into any unknown form with oxigen, or did it not really exist in the residuum in the same quantity, as in the *ammonia* produced from it?

Residuum exposed to intense heat.

I hoped that an experiment of exposing the residuum to intense heat might enlighten the inquiry. I distilled one of the portions which had been covered with naphtha, in a tube of wrought platina made for the purpose. The tube had been exhausted and filled with hidrogen, and exhausted again, and was then connected with a pneumatic mercurial apparatus. Heat was at first slowly applied, till the naphtha had been driven over. It was then raised rapidly by an excellent forge. When the tube became cherry red, gas was developed; it continued to be generated for some minutes. When the tube had received the most intense heat, that

that could be applied, the operation was stopped. The quantity of gas collected, making the proper corrections and reductions, would have been three cubical inches and a half at the mean temperature and pressure. Twelve measures of it were mixed with six of oxigen gas, the electrical spark was passed through the mixture; a strong inflammation took place, the diminution was to three measures and a half, and the residuum contained oxigen. This experiment was repeated upon different quantities with the same comparative results.

The gas detonated.

In examining the platina tube, which had a screw adapted to it at the lower extremity, by means of which it could be opened, the lower part was found to contain potash, which had all the properties of the pure alkali, and in the upper part there was a quantity of potassium. Water poured into the tube produced a violent heat and inflammation; but no smell of ammonia.

In the tube, potash and potassium.

This result was so unexpected and so extraordinary, that I at first supposed there was some source of errour. I had calculated upon procuring nitrogen as the only aeriform product; I obtained an elastic fluid, which gave much more diminution by detonation with oxigen, than that produced from ammonia by electricity.

I now made the experiment, by heating the entire fusible substance from six grains of potassium, which had absorbed twelve cubical inches of ammonia, in the iron tube, in the manner before described. The heat was gradually raised to whiteness, and the gas collected in two portions. The whole quantity generated, making the usual corrections for temperature and pressure, and the portion of hidrogen originally in the tube, and the residuum, would have been fourteen cubical inches and a half at the mean degree of the barometer and thermometer. Of these, nearly a cubical inch was ammonia, and the remainder a gas, of which the portion destructible by detonation with oxigen was to the indestructible portion, as 2·7 to 1.

Gas from the whole of the fusible substance heated.

The lower part of the tube, where the heat had been intense, was found surrounded with potash in a vitreous form; the upper part contained a considerable quantity of potassium.

Solid results.

In

More than one third of the potassium revived.

In another similar experiment, made expressly for the purposes of ascertaining the quantity of potassium recovered, the same elastic products were evolved. The tube was suffered to cool, the stop-cock being open in contact with mercury, it was filled with mercury, and the mercury displaced by water; when two cubical inches and three quarters of hidrogen gas were generated, which proved, that at least two grains and a half of potassium had been revived.

Calculation of the results.

Now, if a calculation be made upon the products in these operations, considering them as nitrogen and hidrogen, and taking the common standard of temperature and pressure, it will be found, that, by the decomposition of 11 cubical inches of ammonia equal to 2·05 grains, there are generated 3·6 cubical inches of nitrogen equal to 1·06 grains, and 9·9 cubical inches of hidrogen, which, added to that disengaged in the first operation equal to about 6·1 cubical inches, are together equal to ·382 of a grain; and the oxigen added to 3·5 grains of potassium would be ·6 of a grain, and the whole amount is 2·04 grains; and 2·05 — 2·04 = ·01. But the same quantity of ammonia, decomposed by electricity, would have given 5·5 cubical inches of nitrogen equal to 1·6 grain, and only 14 cubical inches of hidrogen * equal to ·33, and allowing the separation of oxigen in this process in water, it cannot be estimated at more than ·11 or ·12.

Nitrogen lost and oxigen and hidrogen produced.

So that, if the analysis of ammonia by electricity at all approaches towards accuracy; in the process just described, there is a considerable loss of nitrogen, and a production of oxigen and inflammable gas.

Nitrogen generated when water employed.

And in the action of water upon the residuum, in the experiment page 252, there is an apparent generation of nitrogen.

How can these extraordinary results be explained?

Suppositions to explain this.

The decomposition and composition of nitrogen seem proved, allowing the correctness of the data; and one of its elements appears to be oxigen; but what is its other elementary matter?

* See Phil. Trans. 1808, p. 40, or Journal, vol. xx, p. 328.

Is

Is the gas, that appears to possess the properties of hidrogen, a new species of inflammable aeriform substance?

Or has nitrogen a metallic basis, which alloys with the iron or platina?

Or is water alike the *ponderable* matter of nitrogen, hidrogen, and oxigen?

Or is nitrogen a compound of hidrogen with a larger proportion of oxigen than exists in water?

These important questions, the two first of which seem the least likely to be answered in the affirmative, from the correspondence between the weight of the ammonia decomposed and the products, supposing them to be known substances, I shall use every effort to solve by new labours, and I hope soon to be able to communicate the results of farther experiments on the subject to the Society.

Ammonia decomposed in the experiment, and potassium not a compound of hidrogen and potash.

As the inquiry now stands, it is however sufficiently demonstrative, that the opinion, which I had ventured to form respecting the decomposition of ammonia in this experiment, is correct; and that M. M. Gay Lussac's and Thenard's idea of the decomposition of the potassium, and their theory of its being compounded of hidrogen and potash, are unfounded.

For a considerable part of the potassium is recovered unaltered, and in the entire decomposition of the fusible substance, there is only a small excess of hidrogen above that existing in the ammonia acted upon.

The mere phenomena of the process likewise, if minutely examined, prove the same thing.

After the first slight effervescence, owing to the water absorbed by the potash formed upon the potassium during its exposure to the air, the operation proceeds with the greatest tranquillity. No elastic fluid is given off from the potassium; it often appears covered with the olive coloured substance, and, if it were evolving hidrogen, this must pass through the fluid; but even to the end of the operation, no such appearance occurs.

The crystallized and spongy substance, formed in the first part of the process, I am inclined to consider as a combination of ammonia and potassium, for it emits a smell of ammonia

ammonia when exposed to air, and is considerably lighter than potassium.

Potassium does not absorb hidrogen, but is soluble in it.

I at first thought, that a solid compound of hidrogen and potassium might be generated in the first part of this operation: but experiments on the immediate action of potassium and hidrogen did not favour this opinion. Potassium, as I ventured to conclude in the Bakerian Lecture for 1807*, is

Hidrogen said to be absorbed by potassium.

* M. M. Gay Lussac and Thenard seem to be of a different opinion. In the Moniteur, to which I have so often referred, it is related, that these distinguished chemists, by exposing hidrogen to potassium at a high temperature, found that the hidrogen was absorbed, and that it formed a compound with the potassium of a light gray colour, from which hidrogen was capable of being obtained by the action of water or mercury.

Not in Mr. Davy's experiments.

After a number of trials, I have not been able to witness this result. In an experiment which I made in the presence of Mr. Pepys, and which I have often repeated, and twice before a numerous assembly, in retorts of plate glass, four grains of potassium were heated in fourteen cubical inches of pure hidrogen. At first, white fumes arose and precipitated themselves in the neck of the retort. When a considerable film of the precipitate had collected, its colour appeared a bright gray, and after the first two or three minutes, it ceased to be formed.

The bottom of the retort was heated to redness, when the potassium began to sublime and condense on the sides.

The process was stopped, and the retort suffered to cool. The absorption was not equal to a quarter of a cubical inch. When the retort was *broken*, the gas, in passing into the atmosphere, produced an explosion with most vivid light, and white fumes. The potassium remaining in the retort, and that which had sublimed, seemed unaltered in their properties.

The grayish substance inflamed by the action of water, but did not seem to be combined with mercury. I am inclined to attribute its formation to the agency of moisture suspended in the hidrogen, and to consider it as a triple compound of potassium, oxigen, and hidrogen.

Potassium heated in hidrogen.

When potassium is heated in a gas containing hidrogen, and from $\frac{1}{12}$ to $\frac{1}{30}$ of common air, it is formed in greater quantities, and a crust of it covers the metal, and in the process there is an absorption both of hidrogen and oxigen. It is likewise produced in experiments on the generation of potassium by exposing potash to ignited iron, at the time (I believe) that common air is admitted, during the cooling of the tube.

It is nonconducting, inflames spontaneously in air, and produces potash and aqueous vapour by its combustion.

When

is very soluble in hidrogen; but, under common circumstances, hidrogen does not seem to be absorbable by potassium.

When potassium is heated in hidrogen in a flint glass retort, or even for a great length of time in a green glass retort, there is an absorption of the gas; but this is independent of the presence of potassium, and is owing to the action of the metallic oxides in the glass upon the hidrogen. Hidrogen absorbed by the oxides in the glass.

If a solid compound of hidrogen and potassium could be formed, we might expect its existence in the experiment with the gun barrel, in which potassium is exposed to hidrogen at almost every temperature; but the metal formed in this process, when proper precautions are taken to exclude carbonaceous matters, is uniform in its properties, and generates, for equal quantities, equal proportions of hidrogen by the action of water.

The general phenomena of this operation show indeed, that the solution of potassium in hidrogen is intimately connected with the general principle of the decomposition, and confirm my first idea of the action of the two bodies.

Hidrogen dissolves a large quantity of potassium by heat, but the greater portion is precipitated on cooling. The attractions which determine the chemical change seem to be that of iron for oxigen, of iron for potassium, and of hidrogen for potassium; and in experiments, in which a very intense heat is used for the production of potassium by iron, I have often found, that the gas which comes over, though it has passed through a tube cooled by ice, inflames spontaneously in the atmosphere, and burns with a most brilliant light, which is purple at the edges, and throws off a dense vapour containing potash.

Sodium appears to be almost insoluble in hidrogen, and this seems to be one reason why it cannot be obtained, except in very minute quantities, in the experiment with the gun barrel. Sodium nearly insoluble in hidrogen.

Sodium, though scarcely capable of being dissolved in hidrogen alone, seems to be soluble in the compound of hidrogen and potassium. By exposing mixtures of potash and soda to ignited iron I have obtained some very curious alloys; which, whether the potassium or the sodium was in excess, were fluid at common temperatures. The compound containing an excess of potassium was even light than potassium (probably from its fluidity). All these alloys were in the highest degree inflammable. When a globule of the fluid alloy was touched by a globule of mercury, they combined with a heat that singed the paper upon which the experiment was made, and formed, when cool, a solid so hard, as not to be cut by a knife. Curious alloys.

(To be continued in our next.)

II.

On the Production of an Acid and an Alkali from pure Water by Galvanism. In a Letter from Mr. Charles Sylvester.

To Mr. NICHOLSON.

SIR,

Soda and muriatic acid produced from water by galvanism.

IT is now a long time since I had the pleasure of communicating any thing to your valuable periodical work, although I was under a promise to send you something decisive on the subject of the production of soda and muriatic acid, from pure water, by galvanism. I should not at present have ventured to have offered any thing on this subject, knowing, that the tide of opinion must have gone with the decisions of Mr. Davy, who has said, that the acid and alkali are produced from foreign matter in the water, or in the vessels employed; had not the truth and consequent reasonings of my experiments been strongly supported by many recent facts, brought forward by Mr. D. himself.

Mr. Davy's experiments made on oxides of hidrogen.

All the experiments, in which Mr. Davy has produced the apparent base of an alkali, an earth, or even acid, are nothing more than degrees of the same process, by which the alkali is produced when pure water is exposed to the galvanic influence; and it is equally evident, that all the bodies he has, in these experiments, operated upon, are oxides of hidrogen. I have not the least hesitation in saying, that the acid and alkali can be produced, from pure water, in such abundance as not to admit a doubt of their being derived from the water, or the apparatus. The importance of the electrical agency in chemical processes appears principally to consist in hidrogen and oxigen being furnished in their nascent and pure form; for it will be recollected, that in all experiments, in which the alkalis and the earths have appeared to be decomposed, the presence of water has always been essential to the changes produced.

Acid and alkali produced in abundance.

Electrical agency in chemical processes.

Water with oxigen forms acids; with

It is therefore probable, that water with different portions of oxigen forms acid products; and with hidrogen,

alkalis, earths, and metals. In the experiment, where pure water is exposed to the galvanic influence, separated into two portions by some moist conductor, the oxigen is presented in its nascent form, and an acid is produced, from that substance combining with the water; and at the point where the hidrogen is presented, an alkali is formed, by a similar fixation of hidrogen. In the pretended decomposition of potash, the alkali combines with an extra dose of hidrogen, forming the metallic globulus. And when a metal was said to be produced from ammonia, forming an alloy with potassium remarkable for its little specific gravity, the effect could only be attributed to that metal combining with a still greater portion of hidrogen. Hidrogen, alkalis, earths, and metals.

The electrical doctrine of Mr. Davy is so replete with truth and consistency, that I am every day more pleased with it. It would seem, that we have only two kinds of simple matter; one something like oxigen, possessing the effects of negative electricity in the greatest degree; the other a general inflammable substance of the nature of hidrogen, endowed with positive electricity: that each of these bodies has a constant repulsion between their homogeneous particles, and hence is permanently elastic; that equal portions of these bodies combined would constitute a body of the greatest possible density, from the attraction being at a maximum: and that, as one of them predominates, the attraction becomes less. Hence it appears, that the particles of simple matter are repellent of each other, and that no solid body can be considered a simple body. Mr. Davy's doctrine true and consistent. Only two kinds of simple matter. No solid a simple body.

A friend of mine intends soon to favour you with a more extensive essay on this subject.

If you think the above observations will at all interest the readers of your work, their insertion in your next will much oblige,

Sir,

Your humble servant,

Derby, June 23, 1809. CHARLES SYLVESTER.

This letter came too late for insertion last month. It seems proper to notice, that Mr. Davy states the decompo-

 sition

sition of potash &c., where no water was present. With regard to theories, there must always be great difficulty when inductions are made and generalized beyond the support afforded by the facts. Specific facts duly arranged in support of each other are the great desiderata of science. We possess many, the happy acquisition of our own time, but we are in waut of many more.

W. N.

III.

Account of the Decomposition and Recomposition of Boracic Acid. By Messrs. GAY LUSSAC *and* THENARD *.

Decomposition of boracic acid announced.

ON the 21st of June last we announced in a note read at the Institute, and we published in the *Bulletin de la Société Philomatique* for July, that by treating the fluoric and boracic acids with the metal of potash we obtained results, which could only be explained by admitting these acids to be compounds of a combustible substance and oxigen. However, as we had not recomposed them, we added, that we did not give this composition as completely demonstrated. Since that time we have continued and varied our researches, and are now able to assert, that the composition of the boracic acid is no longer problematical. In fact, we can decompose this acid and recompose it at pleasure.

Method in which it was decomposed.

To decompose it, we put equal parts of the metal and very pure and well vitrified boracic acid into a copper tube, to which a curved glass tube is fitted. The tube of copper is placed in a small furnace, and the extremity of the glass tube in a jar filled with mercury. The apparatus being thus arranged, the copper tube is heated gradually, till it is slightly red hot. In this state it is kept for some minutes. The operation being then finished, it is cooled, and the

* Journal de Physique for November, 1808, Vol LXVII, p. 393. Mr. Davy's experiments on the boracic acid will appear in the course of the paper, of which the commencement is given in our present number. See also Journal, Vol. XX, p. 331, and Vol. XXI, p. 375.

matters

matter taken out. The following are the phenomena observed in this experiment.

When the temperature is about 150° [302° F.], the mixture on a sudden grows highly red, as may be seen in a striking manner by using a glass tube. There is even so much heat produced, that the glass tube partly melts, and sometimes breaks, and the air of the vessels is almost always expelled with force. From the beginning of the experiment to the end, nothing is disengaged but atmospheric air, and a few bubbles of hidrogen gas, not answering to a fiftieth part of what the metal employed would give out by means of water. All the metal constantly disappears in decomposing part of the boracic acid; and the two substances are converted by their reciprocal action into an olive gray matter, which is a mixture of potash and the radical of the boracic acid. This mixture is extracted from the tube by pouring in water, and heating it gently; and the boracic radical is separated by repeated washing with warm or cold water. Before this washing it is advisable to saturate the alkali contained in the mixture with muriatic acid: for it appears, that the boracic radical can become oxided, and then dissolve in the alkali, to which it gives a very deep colour. What does not dissolve is the radical itself, which possesses the following properties.

Phenomena observed in the experiment.

It is of a greenish brown colour, fixed, and insoluble in water. It has no taste; and no action on infusion of litmus or sirup of violets. Mixed with oximurate of potash, or nitrate of potash, and projected into a red hot crucible, a vivid combustion ensues, one of the products of which is the boracic acid. When it is treated with nitric acid, a great effervescence takes place, even in the cold: and when the fluid is evaporated, a great deal of the boracic acid is obtained. But of all the phenomena produced by the boracic radical in its contact with different substances, the most curious and most important are those it exhibits with oxigen.

Properties of the base of boracic acid.

On projecting 3 decig. [$4\frac{1}{2}$ grs.] of boracic radical into a silver crucible scarcely at a dull red heat; and covering the crucible with a jar holding about a litre [a wine quart], filled wish oxigen gas, and placed over mercury; a combustion

This base, heated in oxigen gas,

tion of the most instantaneous kind takes place, and the mercury rises with such rapidity half way up the jar, as to raise it forcibly. In this experiment however, the combus-

first oxided,

tion of the boracic radical is far from complete. What prevents this is, that the radical is at once converted entirely into the state of a black oxide, the existence of which we

then converted into boracic acid.

think we have perceived; and the external parts of this oxide passing afterwards to the state of boracic acid, they melt, and thus defend the interior parts from the contact of the oxigen. Accordingly to burn them completely it is necessary, to wash them, and place them afresh in contact with oxigen gas, still at a cherry red heat, but then they burn with less violence, and absorb less oxigen, than the first time, because they are already oxided: and still the external parts, passing to the state of boracic acid, which melts, prevent the combustion of the interior parts; so that to convert them all into boracic acid, they must be subjected to a great number of successive combinations, and as many washings.

Oxigen fixed, but no gas evolved.

In all these combustions a fixation of oxigen constantly takes place, without any gas being disengaged; and they all afford products so acid, that, in treating these products with boiling water, boracic acid is obtained after suitable evaporation and refrigeration, a specimen of which we present to the Institute.

Burns less vividly in common air.

Lastly, the boracic radical comports itself in air precisely as in oxigen, with this difference only, that the combustion is less vivid.

The base a combustible substance, not metallic.

From these experiments it follows, that the boracic acid is composed of oxigen and a combustible substance. Every thing convinces us, that this substance, for which we propose the name of *bore*, is of a peculiar nature, and ought to be ranked with phosphorus, carbon, and sulphur: and we presume, that, to acquire the state of boracic acid, it demands a large quantity of oxigen; but, before attaining this state, it becomes a black oxide.

Note, by the Authors.

Former trials

Several chemists have made experiments on the decompo- sition

sition of boracic acid, whence they have deduced different consequences. to decompose boracic acid.

Fabroni asserted, that this acid was only a modification of the muriatic. *See* Fourcroy's Chemistry, art. Boracic Acid.

In the Annales de Chimie, vol. XXXV, p. 202, we find a long series of experiments on the phenomena exhibited by boracic acid on treating it with oximuriatic acid. These experiments are by Crell, who inferred from them, that carbon was one of the elements of this acid. Supposed to contain carbon.

Lastly, Mr. Davy, subjecting moistened boracic acid to the Voltaic pile, observed traces* of a black combustible matter at the negative pole; but he says, that being occupied in researches upon the alkalis, he was unable to follow up this observation. *See* Mr. Davy's paper, which arrived in France two months ago, and an abstract of which was inserted in the *Bulletin de la Société Philomatique* for the month of November.

Thus hitherto the principles of the boracic acid were not known. It is true, we had announced to the Institute, that this acid contained oxigen, and consequently a combustible substance; but, as we had not recomposed it, we did not consider its nature as determined.

IV.

On the Influence of Galvanic Electricity on the Transition of Minerals; read at the Meeting of the Mathematical and Physical Class of the Institute, the 13th of July, 1807; by Mr. GUYTON†.

On examining five years ago a native oxide of antimony found in the province of Gallicia, which had been sent me by Mr. Angulo, inspector general of the mines of Andalusia, Native oxide of antimony from a sulphuret.

* Mr. Davy's own expressions are: "I find, that a dark coloured combustible matter is evolved at the negative surface." *See* Journal, vol. XX, p. 231.

† Annales de Chimie, vol. LXIII, p. 113.

sia, I was led to consider this mineral as a transition from the state of a sulphuret to that of almost a pure oxide, which could have been effected only by the decomposition of water, determined by a subterranean electricity precisely similar to that we obtain in Volta's apparatus.

This shown by its appearance.

The external appearance of this mineral, which still evidently exhibits the structure of native crystallized sulphuret of antimony, and even in some parts the remains of a metallic lustre, leaves no doubt, that its entire mass was originally a sulphuret of antimony, the particles of which had undergone the slow and successive action of some agent, that had altered their composition, without disturbing their respective arrangement, precisely as we see in petrified wood, that retains its organization.

The principle applicable to other fossils.

The proofs on which I grounded this explanation, and the applications I have made of this principle to the formation of other fossils, as the pyrites termed hepatic, gray copper ore, &c., have been detailed in a paper inserted in the Journal of the Polytechnic School, for July, 1802, p. 308.

Mr. Davy's ideas similar.

Mr. Davy's views on the same subject, given at the end of his excellent Bakerian lecture read to the Royal Society on the 20th of November, 1806, where he speaks of the slow and silent operations of natural electricity even on the mineral system*, inspired me with the idea of endeavouring to corroborate the inferences drawn from my former results, by performing the experiments with the more powerful apparatus, which we at present possess.

Experiment to confirm the fact.

Messrs. Hachette and Clement were so good as to assist me in this undertaking. We formed a battery of 64 plates of copper and zinc, 15 cent. [near 6 inches] long by 10 cent. [near 4 inches] broad, affording a surface of 9600 cent. or about 1260 French inches square.

Apparatus.

This battery was arranged in Mr. van Marum's manner, that is, distributed in four piles, the first two of which were placed in a plate of copper with its edges turned up, and supported by an insulator. The pasteboards placed between pairs of metal were wetted with a strong solution of soda.

Sulphuret of

A piece of sulphuret of antimony was placed in a small glass

* See Journal, vol. XIX, p. 62.

glass two thirds full of distilled water, and a communication was established between the water and the two poles of the battery by means of two slips of platina. antimony exposed to it.

As soon as the bubbles began to announce the decomposition of the water, a slight smell of sulphuretted hidrogen was perceptible. In two hours this smell was very strong, the water had assumed a yellow tint, and the surface of the sulphuret of antimony appeared of a deeper yellow, and as it were iridescent. Sulphuretted hidrogen evolved.

The slips of platina from the two poles were first fixed at some distance from the sulphuret; afterward they were brought near enough to touch it, and the acceleration of the disengagement of bubbles showed, that the activity of the battery had not slackened. Silver tarnished by it.

After the expiration of four hours, the smell of sulphuretted hidrogen was perceptible at a distance. A slip of silver, well cleaned, being placed on the edge of the glass without touching the water, was in a few minutes covered with a deep black coating. A drop of the water in the glass immediately formed a white precipitate in a solution of acetate of lead. Acetate of lead precipitated.

That part of the slip of platina, which was connected with the negative pole and immersed in the water, was black: and that which communicated with the positive pole had a slight yellow incrustation. The platina tarnished.

The battery having lost almost the whole of its activity at the expiration of eight hours, we attempted to take the piece of sulphuret out of the water; but the motion separating part of the yellow powder that covered it, to collect this we were obliged to throw the whole upon a filter. The sulphuret covered with yellow powder,

This powder, dried in the air, exhibited the same reddish yellow tint as the native oxide of the province of Gallicia; and the fragment still retained evident traces of it on several points of its surface, when scarcely any remains of metallic lustre were distinguishable. resembling the native oxide,

Hence we may presume to give this product of our imitation of the processes of nature as differing from the models she presents us only because the portion decomposed had not reached the same depth, and acquired the same consistency; in other words, because the result of an operation and differing only from the difference of the operations.

tion of a few hours cannot be perfectly similar to that of another, the duration of which depends on a uniform succession of agents, and the slowness of which prevents all possibility of its being disturbed.

Nothing but galvanic electricity could have wrought the change.

If now we consider, that no one of the substances, which we may reasonably presume to exist in the bowels of the Earth, acts in a similar manner, or produces the same changes in sulphuret of antimony when once formed, as I have shown in the paper printed in the Journal of the Polytechnic school, there appears to me no doubt, that the transition of the sulphuret of antimony of the province of Gallicia to the state of native oxide (in which it loses more than 0·17 of sulphur, and acquires 0·18 of oxigen) results directly from the decomposition of water by galvanic electricity. If it be strictly possible for the same effect to be produced by a different cause, it is certain, that the instances are much more rare, than is commonly supposed; and that the greater number appears to belong to this class only because we confound the remote with the immediate cause, the process with the chemical action, the form of the agent with the nature, and, if I may be allowed the expression, the handle with the tool. But when the effect is characterised, as in this particular case, by circumstances that imprint on it the seal of a distinct cause, and excludes any other known cause, we have not a probability only, the certainty of the cause is equal to the certainty of the effect.

Farther proved,

We have since sought for new proofs of this conclusion, extending our experiments to other minerals, where the signs of a transition of this kind were most manifest,

on sulphuret of iron,

Sulphuret of iron, poor in metal, hard, compact, and of great lustre, merited attention in this point of view more particularly, because the pyrites of Berezoff, which is found in the same state of alteration, in its primitive state resists the action of the most powerful solvents, yielding only to the nitric acid and the nitro-muriatic.

and gray silver ore.

On a pyrites of this kind, and the gray silver ore (crystallized *fahlerz)*, we endeavoured to produce analogous alterations.

These exposed in water to a battery, were

Being exposed in distilled water to the action of the same battery, and communications established in a similar manner,

manner, the smell of sulphuretted hidrogen was perceived, and the water rendered turbid; the slips of platina were coloured, as in the former experiment, black on the negative sides, and brownish yellow on the positive; the water, which was strongly acid, precipitated acetate of lead; and the fragments of the sulphurets were left in a state of division, almost pulverulent, and covered with pellicles of a dull colour and without lustre. acted upon in a similar manner.

The sulphuret of iron in particular exhibited a very striking alteration on its surface. Having attached the conductors before water was put into the glass, the sulphuret was vividly inflamed; which astonished us the more because in a preceding experiment a fragment of transparent native sulphur did not exhibit the least sign of inflammation, when touched with the platina exciter under a jar filled with oxigen gas, though the battery was powerful enough to burn the iron wire. The sulphuret of iron inflamed.

It even appeared to us, in the last experiment, that the inflammation of the sulphuret took place instantaneously after it had been covered with water; but the effect was so rapid, that we dare not assert this as a certainty.

We purpose to pursue these experiments, and in the mean time I think I may conclude, that those of which I have just given an account, while they confirm my explanation of the transition of brilliant crystallized sulphuret of antimony to the state of an earthy yellow oxide, without losing its configuration, afford a new mean for interrogating nature respecting the composition of bodies, the proportions of their component parts, and the succession of changes effected in their combinations. The desulphuration of ores is one of the most important points in metallurgy; and if, in the present state of our knowledge, we can scarcely discern any possibility of availing ourselves of this mean in processes in the large way, those of assaying cannot fail to derive more certain results from its application. These experiments to be pursued.

V.

V.

On Artificial Sandstones, that have undergone a regular Contraction in the Fire; by Mr. ALLUAU*.

Artificial sandstone separated into prisms.

ON examining with Mr. Leopold Chevaillers the scoriæ produced in the operation of parting bell metal, which was performed under his direction at Limoges, I found masses of artificial sandstone, which by a regular contraction had divided itself into prisms, precisely resembling the basaltic columns, that exist in all volcanic countries.

Composition of the stone.

These sandstones, which served as a cupel to the melting furnace, are composed of a fine grained sand, the remains of granites; the other component parts of which had been decomposed. To separate them, and obtain the purest siliceous grains, they were carefully washed and decanted, they were afterward mixed with water loaded with clay, to impart to them the body requisite for their use; and a little charcoal powder was added, which, diminishing the points of contact between the siliceous particles and the metallic oxides, rendered them less vitrescible, and thus prolonged the duration of the cupel of the furnace.

Manner in which it was formed.

To form it, a stratum of this mixture 15 to 20 cent. [6 or 8 inches] thick was placed on the floor of the furnace, and strongly beaten down as it was gradually dried by a gentle heat. After being used a certain time, it was necessary to renew the whole stratum, and all the sandstones arising from it had experienced the same contraction.

Its texture.

The upper part of these prismatic sandstones is covered with a scorified metallic stratum 4 or 5 cent. [about 1½ or 2 inches] thick, that serves to hold together all the prisms, which are notwithstanding easily separable. The degree of heat has been more intense near this stratum, than in the inferior part: accordingly the sandstone there is harder and more compact, being difficult to penetrate; while the other extremity of the prism is easily crumbled by the fingers. The fire however has been sufficiently violent through-

* Abridged from the Journal de Physique, vol. LXV, p. 228.

out

out the whole thickness of the mass, to vitrify the metallic fragments disseminated through the sand.

These prisms extend to the length of 15 cent. [5·9 inch.] and from 1 to 2 or even 3 [0·39, to 0·78, or 1·18 of an inch] in diameter. They are parallel to each other, and have their axis constantly perpendicular to the metallic stratum that covers them. Though the number of their sides is not constant, they are most frequently six. Their edges are sharp, and pretty straight. Their faces are not strictly planes, but a little concave; and, what is remarkable, they appear to have been more powerfully heated than the interior of the prism, a circumstance I conceive to be ascribed to the last molecules of caloric, which have escaped as through so many channels by the clefts or intervals, that were formed between the adjacent faces of the prisms. Figure.

When these sandstones have not been so strongly heated, the aggregation and prismatic division is not so well characterised. Then too the charcoal, deprived of the air necessary for its combustion, has arranged itself in longitudinal zones parallel with the axis of the prism, and so as to leave between them intervals of 2 or 3 millim. [0·787 or 1·18 of a line]. This singular phenomenon appears to me occasioned by the caloric, which, absorbed by the metallic stratum, taking the shortest course to reach it, and finding itself stopped in its progress by the particles of charcoal equally disseminated through the mass of sand, pushed them aside to the right and left by imperceptible degrees to open itself a passage, and has thus dispersed them in little parallel strata or threads, as if they had yielded to the laws of affinity, which always tend to bring together homogeneal particles, when suspended by a fluid in a suitable state of rest. In some cases the charcoal arranged in parallel strata.

If we invert one of these masses of sandstone, it is a very good representation of the bottom of a basaltic stratum. In short it is impossible to have a more perfect model of its mechanical division*. The sandstone resembles basaltes,

Naturalists have already remarked clays, that have undergone a reguler contraction in the fire: but, beside that silex more than any thing before observed.

* The piece I preserve in my collection is about 4 dec. [15 ¾ inches long,] and 2 dec. [7 inches and three quarters] broad.

forms

forms more than nine tenths of the mass of this sandstone of Mr. Chevaillers, this effect had not been observed in such a constant manner on such large blocks, as Dolomieu said in speaking of the configuration of basaltes. An effect so frequently repeated must have its causes.

Reflections on the configuration of basaltes.

Basaltes first supposed a crystallization.

At a time when men were ignorant of the first principles of crystallography, and but few crystals were known, it was difficult certainly, to avoid confounding with them solids that exhibited some external appearance of their regularity. Thus Cronstedt, Wallerius, and other celebrated naturalists thought basaltic prisms were the direct products of crystallization.

Romé de Lisle corrected this mistake.

Romé de Lisle at first adopted the mistake of his predecessors: but he had scarcely raised the veil, that enveloped the mechanism of crystallization, when he sought for another cause of the prismatic division of basaltes, and then the happy idea of a contraction offered itself to his mind.

Haüy.

But since the genius of Haüy has developed the theory of crystallization in such a learned manner, may we not be astonished still to find naturalists, who are desirous of assimilating basaltic columns with the productions of regular aggregation?

Objection to basaltic prisms being formed by cooling,

Setting aside therefore every idea of the crystallization of basaltes, I shall confine myself to the refutation of a slight objection of its partizans to the numerous proofs of its contraction, and I shall attempt to follow its mechanism, in examining the laws to which bodies are subjected in cooling.

from their regularity.

They admit, that the cooling of the basaltes must have occasioned divisions, that would naturally give rise to some forms; but they add, that these forms, resulting from chance, and a thousand different accidents, could only be very irregular, and not produce vast columns, as remarkable for their regularity as for the uniform arrangement that characterises their extensive masses.

But they ought to be regular.

But why should these forms depend on accidents guided by the hand of chance? I can conceive no reason for this; since, if the cause of the contraction be constant, and if the manner in which it operates be always the same, ought not

not their results to bear the stamp of this uniformity of circumstances? Do not the cracks of clay dried by a scorching sun sometimes exhibit polygons nearly regular? Do not the cracks in the glaze of pottery, which superficially examined appear destitute of symmetry, resemble on closer inspection a kind of mosaic issuing from the hand of a single artist?

Regular cracks in enamel not from crystallization.

Mr. Patrin even mentions a piece of ancient enamel in the collection of Mr. Dolomieu, the surface of which exhibits throughout hexagonal figures, representing in miniature a horizontal section of a basaltic causeway. But who can conceive with him, that these hexagons are the effect of crystallization? Is it not evident, that the metallic base, on which the enamel rests, being capable of greater dilatation, may under various circumstances have occasioned cracks, the unusual regularity of which gives at first sight an erroneous idea of their causes?

The cooling of basaltes accelerated by something that quickly absorbs heat.

The basaltic prisms then are the result of a regular contraction, and the hypothesis of Dolomieu, which ascribes it to a refrigeration accelerated by the contact of a body that quickly imbibes caloric, agrees perfectly with the division of the sandstone of Mr. Chevaillers, the surface of which is covered with a scorified metallic stratum serving as a conductor to the caloric.

Caloric an agent in its formation.

If geologists be not agreed on the formation of basaltes, they cannot refuse to admit, that caloric performs one of the principal parts in it; whether it act alone, or in concert with other solvent gasses, known or unknown to naturalists: and the latter, as they are extricated, may furnish analogous results.

Effects of a sudden cessation of its agency.

When a body is strongly heated, if the action of caloric come to cease suddenly, the body experiences the most intense degree of heat at the instant when the caloric escapes. In fact, the caloric, rushing rapidly toward the body that absorbs it in proportion to the strength of its affinity, accumulates on the parts which it traverses as a powder does a sieve, and sets in motion the particles of the body, which almost at the same instant are briskly separated and left to the attraction of cohesion, that tends to unite them. Spheres of attraction are then established between the particles of caloric

caloric that are flying off, and those of the substance itself, which tend to unite.

Metal gradually cooled.

If this substance be a good conductor of heat, and the attractive power of its particles equal the expansive power of the caloric, it will return to its natural state without change of form. Such is a metal in fusion, which is left to cool gradually.

Cakes separated from cast iron.

If, under the same circumstances, the caloric, rapidly absorbed, is separated in successive strata, this substance will separate into planes, which will be perpendicular to the direction the caloric takes to escape. Such is the case with cast iron in fusion, the surface of which is wetted to separate thin cakes from it: a cause analogous to what may have divided basaltes into leaves, or thin strata, either perpendicular to the axes of the prisms, or around a globular mass.

Tempering of steel.

If, the motion of the caloric being uniform, the attraction of cohesion do not equal the expansile power, the particles of the substance will remain dilated; and then, if it be a good conductor of heat, they will maintain their situation without experiencing any division. Such is the effect of tempering steel, where the cohesive power of the particles of iron is broken by the interposition of carbon. But if the substance be not a good conductor of heat, it will fall to powder; as quartz strongly heated and immersed in water. The first of these circumstances has perhaps never occurred in volcanic productions, but the other must have been very frequent.

Quartz broken to powder.

Prismatic divisions.

To obtain prismatic divisions, let us suppose a basaltic mass still in its pasty state covering a considerable plain; and which, yielding to the laws of gravity, adheres strongly to the base that supports it. Then, if the expansive force of the igneous or aqueous gasses happen to cease in consequence of their sudden or accelerated extrication, the particles, losing their fluid state, will tend to approach each other, yielding to the laws of gravitation, and also obeying the attraction of cohesion that they exert toward each other; and they cannot contract, but by following the diagonal direction resulting from these two powers. But the extent of this mass, its gravity, and the inequality of surfaces, opposing a general contraction like that which is experienced by

by a cake of clay exposed to the fire on a support, there will necessarily be a vibration, and cracks that will determine spheres of attraction, round which the particles will agglomerate; and the centres will be so much more numerous, and the radii less, as the attractive force is more considerable.

VI.

Observations on the Oxigenized Muriatic Acid. By Mr. JOSEPH MOJON, *Professor of Pharmaceutic Chemistry in the Medical School of the Imperial University of Genoa, &c.*[*]

IN making oxigenized muriatic acid, I have several times had occasion to observe, after having emptied the receiver, into which I had distilled the acid, and left it a few hours exposed to the light, that the little portion of acid, which commonly adheres to the inside of the receiver, lost entirely its peculiar suffocating smell, and acquired an aromatic odour perfectly analogous to that of muriatic ether. I remarked besides, that the oxigenized muriatic acid, though retained in bottles well stopped, and luted so that the gas cannot exhale, yet, if it remain some time exposed to the action of the sun, not only ceases to fume, but also acquires an ethereal smell, similar to that of muriatic alcohol or ether.

Oximuriatic acid acquires the smell of ether:

This transmutation of oxigenized into simple muriatic acid, without the excess of oxigen being able to escape, as also the ethereal smell it acquires by simple exposure to light, led me more than once to suspect, that the oxigen in this case, instead of being extricated in the form of gas, entered into fresh combinations, and formed ether.

changed into muriatic acid without any oxigen escaping.

To convince myself whether ether were really formed, I took a bottle filled with oxigenized muriatic acid, which had been left exposed to light almost two years, and had

Ether obtained from some.

[*] Annales de Chimie, Vol. LXIV. p. 264.

acquired the ethereal smell. I have mentioned, I saturated it with magnesia; and distilled the whole in a glass retort with a very gentle heat, till I had obtained a few ounces of a fluid, which I rectified afresh in a small retort over a lamp. This afforded me a perfectly limpid, colourless liquor, of a very penetrating ethereal smell, and a taste resembling that of muriatic ether diluted with water. It did not change the colour of infusion of mallow flowers; and it did not take fire at the flame of a candle, being still very dilute.

Farther experiments promised.

The small quantity of liquor obtained by this process not allowing me to proceed to a fresh rectification, to deprive it entirely of the superabundant water it contained, I mean to make new trials with a larger quantity of acid.

From the observations I have thus briefly given, and which no doubt deserve to be repeated and confirmed by farther experiments, I am far from pretending to explain by vague hypotheses the formation of acohol, or of ether, by oximuriatic acid, and to point out whence it derives its component parts. We may suppose, however, that a portion of ether is formed at the time of distilling the oximuriatic acid, and that the potent and suffocating smell of this acid prevents that of the ether from being perceived. In fact the celebrated Giobert of Turin, in distilling oximuriatic acid sixteen years ago, observed a volatile oil similar to that which Mr. Westrumb had discovered some time before him. Mr. Giobert tells us, that this oil is of a yellowish brown colour, very clear, and analogous to the *oleum vini*; but that it is difficult to determine its precise quantity, since when once separated it dissolves anew very readily in the aqueous vapours, that fall into the receiver. This chemist imagined he might estimate the quantity of oil obtained from a mixture of a pound of sulphuric acid with eighteen ounces of muriate of soda at 30 or 35 grains.

Perhaps ether formed in the distillation,

as oleum vini found to be.

VII.

VII.

Extract of a Letter from Mr. RESAL, *Apothecary at Remirement, to Mr.* CADET, *Apothecary to the Emperor, on the Conversion of Malt Spirit into Vinegar, and on the Red Colour of Oil of Hempseed* *.

I TAKE the liberty of imparting to you an observation respecting the article of Mr. Hebert of Berlin, whose process you could not verify without it. I communicated it to Mr. Parmentier a twelvemonth ago, with several other notes, part of which was inserted in the month of May, 1806. One of these was on the vinegar of brandy, which chance threw in my way. I had mixed some malt spirit (*alcool de bierre*) with an equal quantity of water, and added to it some beech charcoal. Being set aside and forgotten, I was surprised at the end of a twelvemonth to find it converted into a very strong vinegar, and the unpleasant taste of the beer still subsisting. Malt spirit converted into vinegar.

With your permission I will add an observation respecting the property of liquids to absorb different solar rays.

It is known, that various substances absorb this or that luminous ray, but I do not believe that any one has mentioned the property, that oil of hempseed, *cannabis sativa* L., has to absorb the red rays when they are direct only, and to appear of a fine blood-red colour; so that, being lighter than rape or linseed oil, as it returns to the upper part of the vessel it appears equally red, without changing the colour of the oil it floats on. Its use in the arts, since it offers more resistance to the air than linseed oil, and does not skin [*ne se crispe pas*] like it; and its mixture with oils for the lamp being very common from its low price, while it yields a thick smoke; require a method of detecting it. This that I have mentioned perhaps would answer, and even show the effect of the solar rays on different substances. Oil of hempseed grows red in the sun.

* Annales de Chimie, vol. LXIV, p. 261.

VIII.

Remarks on some Points of Hydrography, by Mr. LEBLANC, *Officer in the French Navy* *.

Errour of longitude in the Gulf of Florida.

THE gulf of Florida, or new Bahama Channel, is greatly frequented by ships of all nations, that trade to or cruise in the Gulf of Mexico; yet the latitudes and longitudes of the principal points in it have not been fixed. They are not mentioned in the Tables inserted in our Connoissance des Temps, or in the English collection entitled "Tables requisite &c." Accordingly we are obliged to have recourse to the most modern charts. French navigators use the General Chart of the Atlantic Ocean published in 1791, and revised and corrected in 1792. I think I can show, that there exists an errour in longitude of 52′ with respect to all the points of the gulf. I was led to notice this on the following occasion.

corrected.

On the 25th of January, 1807, in the afternoon, on board the Foudroyant, we saw waves and breakers on the North of the Great Bahama. At 4 o'clock we set, at a small distance, Lena Key N 80° E, and that of Azena N 45° E by compass. The longitude given by our time-keepers No. 40 and 76, reduced to that hour, was only 80° 17′ 30″, while that by the chart was nearly 82° 16′. Whence it follows, that the whole course of the gulf is too far west about 52′ of a degree †, a considerable errour in those latitudes. The going and state of the two timekeepers had been carefully observed during our long stay at the Havannah. Their errours were almost nothing after we had been at sea eight days, when we had soundings abreast of Cape Henry. The results given by the observations taken with the reflecting circle gave us no reason to suspect any thing incorrect in the longitudes: and when we entered Brest the absolute errour of No. 40 was only 7′ of a degree after a voyage of thirty-five days.

Old Bahama channel.

Green Key is one of the principal marks of the Old Bahama

* Journal de Physique, vol. LXV, p. 55.

† I give the difference as in the original, not knowing where the errour is.

hama Channel. The English call it Chesterfield. There is a small errour in the latitude of this Key, as given in our Connoissance des Temps. In our voyage I ascertained it to be 22° 7′, instead of 21° 55′. The want of tolerable charts of this dangerous part, and the necessity of comparing the ship's place on the chart with sure data, render this observation interesting for those who sail without a pilot on board. As to the longitude, it was agreeable to what I obtained by the timekeepers. This key must not be confounded with another of the same name on the south of the Great Bank of Bahama, and almost in the same latitude.

The accuracy of both of the observations here given I have verified by comparison with two Spanish charts published in 1779 under the ministry of Mr. Langara, and derived from the Hydrographer's Office at the Havannah.

I know not where the latitude and longitude of San Salvador, one of the principal cities of Brazil, in the Bay of All Saints, are to be found. When we anchored in that bay, Mr. Fonsera, Captain in the Portuguese navy, and superintendant of that harbour, told me, that its latitude was 13°. and its longitude 42° 25′. An English work, in the hands of all the navigators of that country, gives them 12° 46′ and 41° 5′. So considerable a difference led me, to pay as much attention to the subject as our short stay would permit; and I had an opportunity of finding both by lunar observations and the timekeepers, that its true longitude is about 41° 5′. The latitude of point St. Antony I ascertained by several observations to be 12° 59·8′. The time of high water is twenty minutes after three, mean time. The variation of the needle there in 1806 was 10° 20′ E.

San Salvador in Brazil.

Variation of the needle.

IX.

On the Spontaneous Ignition of Charcoal: by B. G. SAGE, *Member of the Institute, Founder and Director of the first School of Mines*.*

Charcoal fired in grinding.

MR. de Caussigni appears to have been the first who observed, that charcoal was capable of being set on fire by the pressure of millstones.

* Journal de Physique, vol. LXV, p. 423.

Mr.

In fine powder ignites spontaneously. Mr. Robin, commissary of the powder mills of Essonne, has given an account in the Annales de Chimie, No. 35, p. 93, of the spontaneous inflammation of charcoal from the black berry bearing alder, that took place the 23d of May, 1801, in the box of the bolter, into which it had been sifted. This charcoal, made two days before, had been ground in the mill without showing any signs of ignition. The coarse powder, that remained in the bolter, experienced no alteration. The light undulating flame, unextinguishable by water, that appeared on the surface of the sifted charcoal, was of the nature of inflammable gas, which is equally unextinguishable.

Moisture promotes this. The moisture of the atmosphere, of which fresh made charcoal is very greedy, appears to me to have concurred in the developement of the inflammable gas, and the combustion of the charcoal.

In heaps heats strongly, It has been observed, that charcoal powdered and laid in large heaps heats strongly.

and takes fire. Alder charcoal has been seen to take fire in the warehouses, in which it has been stored.

About thirty years ago I saw the roof of one of the low wings of the Mint set on fire by the spontaneous combustion of a large quantity of charcoal, that had been laid in the garrets.

Fired in pounding. Mr. Malet, commissary of gunpowder at Pontailler, near Dijon, has seen charcoal take fire under the pestle. He also found, that when pieces of saltpetre and brimstone were put into the charcoal mortar, the explosion took place between the fifth and sixth strokes of the pestle. The weight of the pestles is 80 pounds each, half of this belonging to the box of rounded bell metal, in which they terminate. The pestles are raised only one foot, and make 45 strokes in a minute.

Ingredients for gunpowder ground separately. In consequence of the precaution now taken, to pound the charcoal, brimstone, and saltpetre separately, no explosions take place; and time is gained in the fabrication, since the paste is made in eight hours, that formerly required four and twenty.

Manufacture. Every wooden mortar contains twenty pounds of the mixture, to which two pounds of water are added gradually,

The

The paste is first corned: it is then glazed, that is the corns are rounded, by subjecting them to the rotatory motion of a barrel, through which an axis passes: and lastly it is dried in the sun, or in a kind of stove.

Experience has shown, that brimstone is not essential to the preparation of gunpowder; but that which is made without it falls to powder in the air, and will not bear carriage. There is reason to believe, that the brimstone forms a coat on the surface of the powder, and prevents the charcoal from attracting the moisture of the air.

Sulphur useful, not indispensable.

The goodness of the powder depends on the excellence of the charcoal; and there is but one mode of obtaining this in perfection, which is distillation in close vessels, as practised by the English.

Goodness of charcoal important.

The charcoal of our powder manufactories is at present prepared in pots, where the wood receives the immediate action of the air, which occasions the charcoal to undergo a particular alteration.

X.

Theory of the Detonation and Explosion of Gunpowder. By the same.*

THESE two phenomena, which take place simultaneously, arise from different causes. The detonation is the noise, that is produced by the combustion of two parts of inflammable and one of oxigen gas.

Cause of the detonation of gunpowder,

The explosion, or discharge, is produced by the water of the nitre, and that which results from the decomposition of the two gasses, which, being expanded by the fire, occupies fourteen thousand times the space it did before; and acts in the same manner as compressed air, to which its elasticity is restored, and the explosive effect of which is produced without detonation.

and its explosion.

The inflammation of gunpowder by means of a spark arises from the ignition of the nitre and brimstone.

Its ignition.

* Ibid. p. 425.

The

Inflammable gas.

The inflammable gas is produced by the decomposition of the charcoal *; and the oxygen gas arises from part of the nitre, which is decomposed by the fire.

Foulness in gun barrels.

After the explosion of gunpowder, we find the inside of the gunbarrel coated with a mixture of alkaline sulphuret and charcoal not decomposed. This alkaline mixture attracts the moisture of the air, and forms a greasy coating within the barrel. If it be loaded in this state, part of the powder adheres to the sides of the barrel; and on discharging the piece, it catches, and produces what is termed *hanging fire*. The barrel of a fowling piece therefore should never be used a second day without cleaning.

XI.

On the Sulphates of Lime, Barytes, and Lead.

Mr. Thompson's analysis confirmed by Mr. Berthier.

IN our last number, p. 174, we gave an analysis of two of these salts by Mr. James Thomson, who was led to the inquiry by the want of agreement between chemists respecting the proportions of the principles of the sulphate of barytes. A similar reason had led Mr. Berthier, mine engineer, to an investigation, which he has inserted in the *Journal des Mines*, for April, 1807, that has but lately reached this country. His analysis corroborates that of Mr. Thomson, after whose paper it would be superfluous to give Mr. Berthier's, I shall therefore simply quote the results he obtained.

Component parts of

"From the experiments I have above described it follows:

gypsum,

"1. That pure common gypsum, in whatever state of mechanical division it may be, contains 21 or 22 per cent of pure water.

Charcoal of hard woods best.

* In France charcoal of alder, poplar, willow, &c. is always used for making gunpowder. The intensity of the fire produced by such charcoal is less than of that from harder wood. The former, being more porous, would require more care in charring than the latter; and they cannot be said to be in the state of charcoal, unless they have been distilled: for when prepared by smothering the fire, there is always a portion reduced to the state of ashes [*braise*].

"2. That

"2. That the anhydrous sulphate of lime, whether natural or artificial, and the nonanhydrous sulphate calcined, contain the same proportions of lime and sulphuric acid; namely, 0·42 or 0·43 of lime, and 0·58 or 0·57 of acid, nearly as determined by Bergman. sulphate of lime,

"3. That the sulphate of barytes is composed of at least 0·33 sulphuric acid, and at most 0·67 of barytes. sulphate of barytes,

"4. That the mean proportions of these two salts are: 0·425 of lime, and 0·575 of acid, for the sulphate of lime, and 0·665 of barytes, and 0·335 of acid, for the sulphate of barytes. mean of both these,

"5. And lastly, that in pure calcined sulphate of lead there are 0·69 of metal, 0·26 of sulphuric acid, and 0·05 of oxigen." sulphate of lead.

XII.

Extract from a Letter of Mr. Gehlen *to Mr.* Descotils, *on the Igneous Fusion of Barytes*.*

It appears to me, that the French chemists are yet unacquainted with the fusibility of pure barytes by fire, which Mr. Bucholz discovered, and described in 1800, in the 2d number of his *Beitraege zur Erweiterung and Berichtigung des Chimie.* Igneous fusion of barytes

If pure barytes be heated in a platina or silver crucible, it liquefies in its water of crystallization. After this water is evaporated, it enters into fusion at a bright cherry red heat, and flows like an oil. On cooling, it becomes a gray mass, radiated in its fracture, which, when powdered, redissolves in water, heating more strongly than lime, and recrystallizes in cooling. succeeds the aqueous.

Mr. Bucholz, having hitherto prepared his pure barytes only in Pelletier's method, did not know by experience, that barytes did not melt when it has been prepared by the decomposition of the nitrate by fire; which it might have been expected to do, but which I have never seen take place, Does not take place with barytes obtained by decomposition of the nitrate.

* Annales de Chemie, Vol. LXIV. p. 168.

place, even with the strongest heat. Mr. Bucholz and I have made some experiments; to ascertain the cause of this; but we have not yet attained our object. Neither an excess of carbonic acid, nor the solution of part of the substance of the crucible, appears to be the occasion of this difference, since, on dissolving the residuum of the decomposition of the nitrate in water, very little insoluble matter remains in proportion to the quantity of barytes; and on adding this insoluble matter to pure barytes in much larger proportion the latter is not prevented from entering into fusion.

Perhaps previous crystallization necessary.

We know not whether the previous crystallization of barytes be necessary to the fusion, and consequently whether water do not act some part in it. This might be solved, by decomposing the nitrate in a crucible of some material not acted upon either by the nitrate or barytes. We made our experiment in a silver crucible, but obtained no decisive result, on account of the large quantity of silver, which the nitrate detached from the crucible by cohesion. As we have not a crucible of gold, or of platina, we cannot pursue our experiment. These observations, if inserted in your Annals, may perhaps tend to an elucidation of the subject.

Note by Mr. Descotils.

Proportions of the elements of the sulphate determined with fused barytes.

The French chemists have long known the igneous fusion of barytes, and it was with barytes thus fused, that Mr. Thenard determined the proportions of sulphate of barytes, which he gave in his Memoir on Antimony, published in 1800. It was likewise with fused barytes, that Mr. Berthollet has since determined the proportions of the principles of the same salt. As to the difference in fusibility of crystallized barytes and that which is obtained from the decomposition of the nitrate, Mr. Berthollet will make known the cause in a paper, which will be inserted in the 2d volume of the *Memoires d'Arcueil.* His experiments relating to barytes were already finished, when I received Mr. Gehlen's letter; and they had given occasion to a series of researches, that are now concluded. In Mr. Berthollet's paper it will appear, that water is the cause of the fusibility of barytes, as the two celebrated chemists of Erfurt have suspected; and that it

Water necessary to this fusion.

it is likewise the cause of the difference of the proportions of the principles of sulphate of barytes given by the chemists, who have attempted at different times to determine its composition.

XIII.

Note on a Species of Manna, or concrete Sugar, produced by the Rhododendron Ponticum.*

A Few years ago Messrs. Fourcroy and Vauquelin remarked, that a concrete sugar, or manna, exuded from the receptacle of the flowers of the pontic dwarf rosebay. **Concrete sugar on the rosebay,**

Mr. Bosc has lately observed it afresh, and presented to the Institute some grains of this substance collected by him from the receptacle of the fruit, several of which were upward of 2 mill. [0·79 of a line] in diameter. Their taste and appearance do not differ perceptibly from the purest sugarcandy; but it is necessary to be on our guard against this appearance, on account of the deleterious properties suspected in the plant. Mr. Deyeux has even found, that they leave an acerb smatch on the palate. **described.**

The manna of the rosebay, according to Mr. Bosc, is dissolved during the night by the moisture of the atmosphere, melted in the day by the heat of the sun, and does not exude from plants that vegetate vigorously. These are the reasons why it is so seldom seen. Plants growing in pots, and sheltered from the dew as well as from the sun, are most likely to furnish it. The grains above mentioned were collected from a plant, in which all these circumstances united. **Reasons why seldom seen.**

Mr. Bosc intends, if possible, to collect a sufficient quantity to analyse.

* Annales de Chimie, vol. LXIII, p. 102.

XIV.

XIV.

An Essay on Manures. By ARTHUR YOUNG, *F. R. S.*

(Concluded from p. 196.)

7. *Yard and Stable Dung.*

Dung usually collected in heaps.

IT has been a common notion, till very lately, both with farmers and writers on agriculture, that dung is to be accumulated on hills or receptacles for a longer or shorter time, till fermentation and putrefaction have brought it, after few or many months, and few or many operations of turning or mixing, to a certain state, in which it is ready and proper for applying to land.

But it is sometimes used fresh:

But there is another system of management, which of late has attracted a good deal of attention: and this is, to use it fresh as made. If this method be right, no instructions for the management of dunghills are necessary, since we ought to have no dunghills.

and this is preferable, according to the ablest chemists,

Hassenfratz observes, "The management of the farmers "in Picardy is highly advantageous, in continually carrying "their dung to their land, rather than leaving it to be exhausted in their farm yard, in order to be carried out at "a fixed period. By applying the dung quite fresh to the "land, its first fermentation is employed in heating the "soil. The little alkali it contains, instead of being dissolved in the farm yard, and carried off by rain, remains "in the land, and improves it, if alkali be useful to vegetation. The straw, yet entire, better divides the soil; its "fermentation proceeds less rapidly, and is less advanced "when the seed is sown; and consequently the dung is in "a better state for furnishing a great quantity of carbonic "acid, which hitherto appears to be, with water, the principle aliment of plants."

Dr. Darwin asks a very interesting question. "Do the "recrements of vegetable and animal bodies, buried a few "inches beneath the soil, undergo the same decomposition, "as when laid on heaps in farm yards?" He conceives they do,

do; and adds: "Though this is accomplished more slowly, "yet it is attended with less loss of carbonic acid, of vola- "tile alkali, of hidrogen, and of the fluid matter of heat; "all which are emitted in great quantity during the rapid "fermentations of large heaps of manure, and are wasted "in the atmosphere, or on unprolific ground. By using "dung in a less decomposed state, though it will require "some time before it will be perfectly decomposed and re- "duced to carbonic earth, it will in the end totally decay, "and give the same quantity of nutriment to the roots, "but more gradually applied."

The testimonies of Kirwan, Sennebier, and Dr. Pearson, are equally in favour of carrying dung fresh to the field.

What is still more to the purpose, the theory of these able chemists is supported by the authority of many of the most skilful and judicious farmers founded on extensive experiments. *and the practice of the best farmers.*

As dung is a compound of animal and vegetable matters, but chiefly the latter, it must be resolvable into the principles of which they are composed. *Nature of dung.*

These principles, thus separated by decomposition, will be ready again to enter into the composition of the growing vegetables. The grand property of dung therefore is, to yield immediate food to plants. Farther, it opens the soil, if this be strong; it attracts moisture; and by the fermentation, which it excites in the soil, promotes the decomposition of whatever vegetable particles may be already in the land. Its effects have powerful progressive influence; for the production of a great crop of leaf, root and stalk, by its shade and fermentation leaves the land in better order to produce succeeding crops. *Its properties.*

The circumstances to be considered in the receptacles of yard and stable dung are few but important. *Collecting.*

The first object to be attended to is to spread a layer of earth over the surface of the yard. Peat is the best for this purpose, with a portion of marle, or chalk. In want of this, turf, rich mould, scourings of ditches, and some marles, or chalk; but not so much of either as to prevent the penetration of the fluids, which should enter sufficiently, to give a black colour to the whole. There is no necessity for remov-

ing

ing this every time the dung is removed. As there are no advantages from fermentation in the mass till carried on to the land, no attention should be paid to prevent treading and pressing the mass. But as it is beneficial to have the whole as equal as possible, it is very useful, that the stable dung should be spread over the surface, and not left to accumulate at the door. The same observation is applicable to the riddance of the fat bullock stalls, and the hogsties. As heavy rains will at times, in spite of every precaution, cause some water to run from the yard, this should be received into a covered reservoir, and pumped up on heaps of earth prepared to receive it. In summer weeds of every kind, that do not propagate from the root, should be early collected and spread over the surface, as well as leaves in autumn; and the foddering with straw, if any, and the soiling on green food, should both be upon it for all loose cattle.

Preparation. From what has been said it is obvious, that dung requires no preparation; but if the richness or quantity of the dung, or state of the weather, excite too much fermentation, or this be apprehended, scatter every now and then over the surface some of the same earth with which the yard was bedded, but not in layers.

State in which applied. As soon as circumstances of crops and convenience will permit, the dung should be carried to the land. In a business of any extent this cannot be done exactly when the absorption of animal matter is enough to secure a due fermentation in the soil, but must be directed by other circumstances. The farmer however is not to lose sight of those principles, which govern the operation.

Application. All dung should be applied to hoeing crops, to leys, or to grass land, and never to white corn. This is more essential with fresh long dung, than with short; as there will be many more seeds of weeds in it, several sorts of which are destroyed by a strong fermentation. The proper crops for which to apply yard and stable dung are turnips, cabbages, potatoes, beans, and tares for soiling; and the seasons for putting in these crops are spring, midsummer, and September. But the farmer is not confined to carry on his dung at the time of sowing or planting: it is, on the contrary, much better, especially with long dung, to have it previously deposited

deposited in the land. The dung made in the depth of winter may be spread in March or April for potatoes: the next made, and what is not wanted for potatoes, may be taken out in succession through April, May, and June, as convenience suits, for turnips and cabbages: that made in July and August will be ready for tares: and what is produced in September, October, and part of November, is ready for beans. The best time for manuring grass is immediately after hay is cleared from the field.

It is proper to remark, that the use of the skim coulter is essential to ploughing in long dung. By means of this admirable addition to any common plough, every atom may be buried*. Skim coulter.

8. *The Sheepfold.*

The immediate application of dung and urine to all soils, and of treading too loose ones, is well know to be productive of great benefit. Every one knows, sheep's dung and urine are so far from wanting fermentation previous to their being applied, that the sooner the seed is sown after folding, the greater is the effect: and this tends to confirm the principles laid down in the preceding section. Folding sheep.

9. *Pigeon's Dung.*

This manure is esteemed by farmers to be hot and powerful. Forty or fifty bushels per acre are commonly applied. While in the house it does not run into those stages of fermentation, that reduce a body to mucilage; and yet has an extraordinary effect when spread. This is another argument in favour of fresh dung. Pigeon's dung.

10. *Pond and River Mud.*

The quality of this must be affected by various circumstances. In proportion as it is resorted to by cattle and waterfowl, and receives the washing of towns, houses, farm yards, or privies, the mud must be good. In other cases the mud may be tried experimentally in small quantities, or chemically analyzed. It generally pays well, but seldom or never very considerably. Pond and river mud.

* See Journal, p. 82, on the utility of burying dung deep.

11. *Sea Weeds.**

Sea weeds.

Wherever these are to be had, they are used with uniform success. The best and most durable sort is cut from rocks at low water. One load used fresh is more service than two, that have been left in a heap to ferment. This is the case with nine substances out of ten.

12. *Pond and River Weeds.*

Pond and river weeds.

Great advantage has been found from cutting these weeds just before the last ploughing for turnips, and spreading them as a manure for that crop. Some value them load for load equal to dung, and have imagined the following barley superior to that after dunging for turnips.

13. *Hemp and Flax Water.*

Hemp and flax water.

In Yorkshire they observe, that the grass grows doubly where flax is grassed. Mr. Billingsley carted flax water on his land, and found it superior to animal urine. Where there are convenient ponds on a farm, one at least should be half filled in summer with green weeds for the putrid water, which would soon be the result.

14. *Burnt Vegetables.*

Burned vegetables.

In some parts of Lincolnshire it is usual, to spread evenly over land, just before sowing turnip seed, from three to 4 tuns of straw per acre, and set fire to it. A similar practice prevails in the Pyrenees. It is said to be superior to common dunging. In Cambridgeshire and other places very stout oat stubble, reaped high, is burned as a preparation for wheat, both cleaning and improving the land.

15. *Ploughing in Green Crops.*

Ploughing in green crops.

This husbandry has been practised for ages, though many have found little advantage from it. The benefit certainly depends on the crop being completely buried. The only way of proceeding is, to roll down the crop with a barley roller, and add a skimcoulter to the plough, going in the same direction as the roller, to plough six inches deep. There should be no other successive tillage than scuffling

* See Journal, p. 79.

shallow

shallow on the surface. It usually answers better for a summer's sowing, as of turnips, or early winter tares, than for late autumnal sowings.

General Remark.

On all arable farms the dung of the farm yard may manure from a sixth to a fourth of it; by a proper course of crops and layers a certain portion may be pared and burned; and at least one tenth may be manured by ploughing in green vegetables. By these three exertions a good manager may manure more than one third of his arable land every year, which, with a right application of calcareous manures, will keep any land in heart, and regularly in a state of improvement. General remark:

The preceding manures are usually to be procured on most farms. Under the second head, or such as are to be purchased, we have in the first class, or animal manures,

1. *Night Soil.*

This is the best of all manures, and, if dry, the cheapest. Night soil. It answers on all soils, and for all crops; but the most profitable application of it is on grass lands, spread after clearing away the hay; though it may be used in all seasons. It is very durable in effect. The common quantity used is about 200 bushels an acre. In the state of powder it is excellent for delivering by drill cups with turnip seed.

2. *Bones.*

These do best on strong soils, and their duration exceeds Bones, and that of all other manures. The effect has been seen for above thirty years. For potatoes they are excellent. Five or six loads of fifty bushels each are commonly employed on an acre, after they have been broken and boiled for the grease.

The refuse dust of bone manufacturers is also good. bonedust.

3. *Sheep's Trotters.*

These are a powerful manure, and usually sold by the Sheep's trotters. quarter with feltmongers cuttings. Four or five quarters an acre are a common dressing, but eight have been spread.

 They

They should be ploughed in not less than 6 inches with a skim coulter.

4. *Hair.*

Hair.

Hog's hair is sold in great cities from 1s. to 1s. 6d. per bushel, pretty well squeezed down. From 16 to 25 bushels an acre are commonly used.

5. *Feathers.*

Feathers.

These are a powerful manure. Twenty-five bushels an acre have been spread with much success: but land, which unmanured yielded but 28 bushels of white wheat, with ten bushels of feathers produced 48.

6. *Fish.*

Fish.

Every sort of refuse fish is one of the most effective manures that can be carried into our fields.

7. *Graves.*

Graves.

These appear to produce remarkable effects in turnip crops on poor sandy soils.

8. *Woollen Rags.*

Woollen rags.

These do best on dry and sandy lands. From five to twelve hundred weight an acre, chopped small, are used.

Refuse of leather.

9. *Curriers Shavings and Furriers Clippings*

do best on dry soils. Thirty bushels an acre are a common dressing.

10. *Horn Shavings.*

These are applicable to all soils, but do best in wet seasons. The coarser sorts are cheaper, but inferior in effect, though more durable.

Nature and properties of animal substances.

Nature & properties of animal substances.

All animal substances will fertilize the soil being resolved into their first principles, but this takes place much sooner with some, than with others. Urine begins to act immediately, bones will last twenty years. All of them should be laid on the field as soon as may be after collecting. Night soil, dry and in powder, is the only one properly applicable

plicable as a top dressing; the rest should be ploughed in as soon as spread.

In the second class Mr. Young includes

1. *Wood Ashes.*

Mr. Hassenfratz having questioned whether alkalis were a manure, Mr. Young made many experiments on the subject, which convinced him, that pearlash was in a very powerful degree; and that it also had the property of acting on charcoal by mere mixture and solution in water. Alkalis act on charcoal by mixture and solution in water.

Woodashes, wherever tried, have proved a valuable manure. Mr. Young has used them on gravel and loams, both dry and wet, and never without good effect. The spring is the proper season, and succeeding rain of much importance. Forty bushels an acre the common quantity. Woodashes.

2. *Peat Ashes.*

The value of these usually depends on the blackness and density of the peat that is burned. Those of the Newbury peat are most celebrated, and ten or twelve bushels an acre are a common quantity, while in other countries from twenty to forty are usually applied. According to Mr. Davy their component parts are Peat ashes.

Oxide of iron	48
Gypsum	32
Muriate of sulphur and of potash	20
	100

Some uncommonly ferruginous peat ashes are used with great success on the chalk hills of Dunstable.

3. *Coal Ashes.*

All sorts of ashes are found most effective when spread on clover, sainfoin, or other *seeds* in the spring. They are also good on grass lands, and are by many used on green wheat. The quantity from fifty to two hundred bushels an acre. The effect of fifty or sixty bushels on dry chalk lands is considerable. They answer best on dry, sound, rich loams; but on clays, and wet gravels or loams, they make a Coal ashes.

 poor

poor return. Coarse ashes and cinders are better than those that are finely sifted.

4. *Soot.*

Soot.

This is a very powerful manure on most soils; but least upon strong or wet clay. Twenty bushels an acre are the common quantity applied on green wheat or clover in the spring.

5. *Peat Dust.*

Peat dust.

From its abounding in hidrogen this should operate as a strong manure. Commonly too it contains much iron. Having a great attraction for humidity, it is very advantageous on dry sandy soils. Mr. Farey asserts it to be the best possible dressing for onions.

6. *Potash Waste.*

Potash waste.

The alkali having been extracted, this is not a powerful manure, but does good in low meadows, and on grass lands in general. Ten loads an acre, or 350 bushels, are a common quantity.

7. *Sugar-bakers Waste.*

Sugar bakers waste.

Some say this is a powerful manure.

8. *Tanners Bark.*

Tanners bark.

The tanning principle is probably in all cases hostile to vegetation. If this bark be ufeful any where, it should be on calcareous soils. Sometimes it appears to have diminished a crop of corn very considerably.

9. *Malt Dust.*

Malt dust.

Eighty bushels an acre have exceeded dung on clay land for wheat. From twenty to forty bushels are commonly used, and with success on various soils.

10. *Rape Cake.*

Rape cake.

About half a tun an acre is an excellent manure, but since the price has risen less is used. Mr. Coke, by drilling it in *powder* with turnip seed, makes a tun do for five or six acres.

Of

Of the fossil manures lime was included in the first division, and coal ashes were classed with those of wood and peat, so that only two remain.

1. *Salt.*

Salt.

Little is known of this at present. In too large a quantity it is injurious. It is certainly beneficial when properly applied. Perhaps it is best when mixed with dung or compost.

2. *Gypsum.*

Many persons assert, that this is no manure; others, that it is almost uniformly advantageous. It is said, to act as an immediate manure to grass, and afterward in an equal degree to grain: to continue in force for several succeeding crops: to produce an increase of vegetation on stiff clay soil, but not sufficient to pay the expense: to be beneficial to flax on poor dry sandy land: to be particularly adapted to clover in all dry soils, or even on wet soils in a dry season: and to have no effect in the vicinity of the sea.

Of Composts.

Composts.

These Mr. Young considers in the same light with dunghills: he is of opinion, that the materials composing them would produce at least equal if not superior effect when applied to the land directly.

XV.

On the Formation of the Winter Leaf Bud, and of Leaves. By Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Use of the bud not yet known.

YOUR obliging notice of my former papers has emboldened me, to trouble you again. There is no part of a plant or tree more various in its formation, and in its consequences more astonishing, than the *gemma*, or *bud*. In spite of

of the abilities of a Malpighi, a Grew, and many others, its real use is not yet *perhaps* known. So defective were our magnifying glasses *at that time*, so impossible was it to render an opake object luminous and c a, that we cannot wonder they did not attempt to search further into the formation of the bud: for there is hardly any study, that requires the objects being so much magnified, and opake specimens so clearly delineated. What follows I offer as the result of many years study; I offer it with the *greatest diffidence*, but with the most thorough conviction of its truth: nor have I trusted wholly to my own sight, many have seen the specimens on which I first founded my opinion, and drawn from them the same conclusions; which, though from their novelty they may surprise, will on farther examination in *very young* buds and leaves soon give conviction.

Method in which leaves are formed.

This opinion is, "That leaves are formed or woven by the "vessels or cotton, that is generally supposed by botanists "placed there to defend the bud from the severities of "winter. That these vessels are a continuation of those "of the bark and inner bark in the stem of the plant, "That these vessels compose the various interlacing "branches of the leaf, which are soon filled up by the concentrated and thickened juices of the inner bark, which "form the pabulum of the leaf."

This apparent on dissecting very early buds.

The truth of this assertion is easily seen by dissecting very early buds, where except two or three scales, nothing but these vessels will be found. What then could be the use of them?—to put them within *the bud* to keep the outside warm is against nature, for it is against reason. I shall begin with the anatomy of the bud from its first appearance; which will explain the whole process, as far as constant attention could give me an insight into it. The gemma or bud grows on the extremity of the young branches. It is a small round or pointed body; and is fixed on the young shoot, and along the branches on a sort of bracket. There are three sorts. The leaf bud, the flower bud; and the leaf and flower bud. It is the leaf bud alone I mean here to dissect: for their natures are totally different, as are the purposes for which they are intended. As I look on the leaf bud to be formed

Buds of three kinds.

almost

almost wholly of the bark and inner bark, so the flower bud is a composition of every *part* and *juice* of *the tree*.

The leaf bud. The leaf bud is generally smaller than the other two; in its first state it consists of two or three scales, enclosing a parcel of vessels, which have the appearance of a coarse kind of cotton, very moist; but when drawn out, and placed in the solar microscope, they show themselves to be merely the vessels of the bark and inner bark elongated and curling up in various forms. They are generally of three sorts, like the bark, &c. First three or four short thick ones that appear to grow from the larger vessels of the inner bark, and through which the thickened juice flows, but with this difference, that the holes are not there. Then there are two smaller sized vessels, that exactly resemble the smaller vessels of the bark. The former I have ever found to be the midrib of the leaves; the latter the interlacing of the smaller vessels: and I have so often taken a leaf and dissected it to compare it with the vessels which I the next winter found in the leaf bud of the same tree, that I cannot but feel the most *thorough conviction*, that I have in *the bud* traced its origin; though certainly much enlarged in the full grown leaf. The pabulum of the leaf, or that which lies between the vessels, is (as I have before said,) composed of that thick juice which runs in the bark or inner bark of the tree, and is to be found in no other part. It differs essentially from the sap, and may be called the blood of the tree, as it possesses its peculiar virtues, is gum in one, resin in another, oil in a third, according to the nature of the plant. Whether it flows both *forward* and *retrograde* I have not yet been able to discover; indeed, finding the subject in the hands of a gentleman of such abilities as Mr. Knight, I waited his decision: but that the greatest part is taken up in *forming* the *leaves* I feel the most perfect conviction. The pabulum of the leaf, after the vessels are arranged and crossed, grows over in bladders, making alternate layers with the smaller pipes, and with the branches of the leaf. But I have found, and shall give, many specimens before this part of the process is begun.

Mr. Knight.

Formation of the leaf of the horsechestnut. I know not any tree that gives a more convincing proof of the manner of forming leaves in the bud than the horse-chestnut:

chestnut: but it should be taken in November or December. Several different midribs may be taken at once from the same leaf bud, with an innumerable number of silken vessels extremely fine, fastened, or growing up each side the midrib. When these have interlaced each other sufficiently, the pabulum will begin to grow over them, in small bladders full of a watery juice. The next process is the larger vessels crossing over them, and then another row of bladders; this continuing till the leaf is at its proper thickness. The leaves thus formed are very small, but when once their shape is completed, they then continue growing all together.

A drawing will so much better explain this than any description, that I shall beg leave to refer to the sketch of the several specimens of beginning or half formed leaves taken out of the buds of various trees.

Mode of arranging the leaves in the bud.

When the leaves are so far completed, the rolling and folding begins. Each tree has its peculiar mode of arranging its leaves in the bud, as Linneus beautifully exemplifies, some double their leaves, and then roll them round one midrib; some round several, each of which has its own middle vessel; some plait, some fold the leaf. The variety is prodigious; but it must not be supposed, that once is sufficient to complete the *process*; I have had the most thorough conviction, that it is repeated several times, immersed all the while in the glutinous liquor, that runs in the bark, and forms the *pabulum*. During this arrangement, the pressure of the leaves is very great; and it is this and the rolling, that completes them; for if a leaf is taken from the bud, before this process, it will be like a piece of cloth before it is dressed; that is, with all the ends and knots to it; thus the back of the leaf will be obscured by the ends of vessels, which are at last all rubbed off, the hairs excepted, which remain to many plants.

Formation of the edge of the leaf.

The next process is the forming the edges of the leaves, the most curious and the most beautiful of all. The bud, if opened, will appear full of that glutinous liquor, and the leaves folded according to the order to which they belong. Take out one of them, and the edges, folded as it is, will exhibit a perfect double row of bubbles following the scollop of the leaf's edge, and appearing as if set with brilliants.

I hardly

I hardly know a more admirable spectacle in the microscope; it requires but trifling powers to show it well.

The last process, and completion of the leaf, is the forming of the pores. Whether it is, that the young leaf being thicker and more hairy than it is afterward, the pores are obscured and hidden, or that the upper net grows last, I cannot say; but in the many hundred forming leaves I have exposed to the solar microscope, I have never once been able to view the pores, as I have often done after the leaves had completely quitted the bud. I must not forget to mention, that there are two sorts of pores in the leaf; the large ones are those which receive the dew drops and rain, the smaller are those which appear in the day to give out the oxigen, and at night to inhale the carbonic gas. I mentioned, that I suspected these smaller pores of yielding a sort of insensible perspiration; as I find, when out of doors, a scurf only to be seen with a microscope; and under a glass this seems to rise as water, to bedew the glass. But to place an object in an unnatural situation, in order to judge of its secretions, is something like putting a human being into a warm bath, to judge how fast the blood flows. We know not what unnatural secretions we may cause in that confined air, or how much it may alter the nature of the plant, as I shall show at a future time with respect to melons and grapes.

Formation of the pores.

Two sorts of them.

Unnatural situations may occasion unnatural secretions.

The two cuticles of leaves differ in most plants: for in the under one I have hardly ever found the large pores into which the dew or rain enters; and but little oxigen is given out also from the under part of most leaves; while this part has a number of very small apertures, formed I suppose for the reception of the carbonic gas.

Upper and under cuticle.

I cannot but notice here how strange is the contradictory account of the leaves now generally received. They are supposed to perspire 17 times more than a man: water must therefore be yielded from each pore. They at the same time give out oxigen, and receive carbonic gas. Is this credible, or is it not contradictory? That they give out oxigen in the day, and inhale carbonic gas in *the night*, I am *convinced*, and I think it requires but the simple experiment of keeping a plant in the window, and examining it with a microscope 8 *or* 10 *times in a day*, to convince a person, that there

Contradiction in the received notions.

there is no perspiration worthy being so called. Bu turn to my subject.

Completion of the edge of the leaf.

While the upper and under cuticles are growing, tl of the leaf is completing; the bubbles generally divi partly dry up, and horny points appear in their stead. this is complete, the leaves burst from the bud; bu are few that will not show for a long time the manner formation; the planes for more than a month remain ed with the ends of vessels, some attached to the leaf loose: and most leaves have a bunch of vessels faste the outside to the corner of each side rib.

Two sorts of vessels.

The vessels of the leaves (I mean those confined in the midribs and side ribs of the leaf) *are of tw the spiral* and *nourishing vessels*. The spiral vessel corkscrew wire, that surrounds the two last rows of t vessels (as I shall show when I describe the division into the stem should, *I conceive* be separated). The nou vessels are the only part formed of the wood, and con sap necessary for the support of the leaf, and run o side of the spiral ones; which are generally divide little bundles of 3, 5, or 7 sets. It is impossible for a lineation to be more exact, than that given in the Phil. by Mr. Knight, of the entrance of these vessels into th rib of the leaf. That these spirals vessels are the c

Motive vessels.

motion in leaves, and that they are perfectly solid and pable of carrying moisture, I hope to prove in my next

Use of the hairs on leaves

Many leaves have a number of hairs fastened to the cuticle of the leaf, and some to the upper. On the they appear designed to divide the rain drop to the size pore it is fitted for, and those at the back of the lea intended to guard it from moisture, that the wet not prevent the entrance of the carbonic gas at which it probably would do, without this preca by resting on the apertures. But it is watching nat her natural state, that her laws are to be understood.

Why leaves turn from the wind & to the sun.

the wind blows with violence, the leaves turn their ba the wind; and when the sun shines, they present face to it: guarding by the first means the oxigen fro *persing*, and in the latter case procuring a greater qu from the heat of the sun shining on the leaves. Wh

leaves are very young, they are pressed together, their backs exposed to the heat; probably to dry them, and clear the pores for the reception of the carbonic gas; and as young leaves give out hardly any oxigen, the shade in which the other side is immersed is of little consequence.

To prove, that in forming the leaf I have given it no features, but what it really possesses, I shall finish by showing all the parts of a full grown leaf. The colour of leaves is not to be found in their substance, but in the liquid with which it is filled. The darkest green leaf that can be *taken*, has a perfect white cuticle, both *above* and *below it*. In this cuticle are the pores. It is rather a thicker net below than above; but not enough to account for the difference of the tints; but the under one lies not near so close to the pabulum of the leaf as the upper one; which may account for the colour not piercing so much through. When these two nets are taken off, the pabulum of the leaf appears. It is formed of little bladders, filled with a dark green liquid, and interlaced with vessels. Take this off, and a bed of larger vessels presents itself; then a collection of bladders, which is followed by the larger lines of the leaf; and then a bed of bladders repeated, which the under cuticle covers. Though the bladders differ in *size* and *colour* in different leaves, and in thickness also, yet the general arrangement is the same. I mean not however to include either the *firs*, the *grasses*, or those *grassy leaves* of early spring, the iris, crocus, snowdrop, &c., which are all of a different nature, as I shall show hereafter.

Description of the leaf.

I cannot quit the subject without adverting to the different sorts of hairs, that are found on the back and face of the leaf. I have before mentioned some on the former part, intended to preserve the dryness; but on the face of the leaf there appear often many filled with moisture, as a kind of reservoir for the cuticle, and these are replete or not, according to the dryness of the atmosphere.

Different sorts of hairs.

There is also an innumerable multitude of things, that are truly parasite plants, that grow on leaves, forming groves and orchards for the various tribes of insects, that live and breed under them. As I do not wish to mix the different subjects, I shall conclude this letter, but mean to trouble you with

Microscopic parasitical plants.

with another on the division of the stem of plants, without which I cannot well explain the discoveries I think I have made with respect to the motion and general formation of plants, or the effect that grafting and budding of every kind have on trees; a study which is now occupying every moment of my time, and from which I hope to draw many useful hints.

The mistake made by my directing my letters to be sent to Mr. I. has led you into an errour. It is Mrs. Agnes Ibbetson, who has the honour of being your correspondent.

Dear Sir,

Your obliged servant,

Belleveu, June 8. A. IBBETSON.

Explanation of the Figures.

Explanation of the plate.

Plate VII, figs. 1, 2, 3, 4. Commencement of the growth of leaves, exhibited in different stages. *a, a, a, a,* the midrib. *b, b, b,* the young vessels appearing like cotton. *c, c,* the spiral nerves. *d,* the smaller vessels crossing each other.

Fig. 4. The formation of the pabulum. *e, e,* the fine vessels growing up each side of the midrib. *f,* the pabulum.

Fig. 5. Leaf-bud of the limetree.

Fig. 6. Leaf-bud of the horse chestnut about January.

Figs. 7, 8, and, 9, with some others, belong to two papers, which will appear next month.

XVI.

A Letter on a Canal in the Medulla Spinalis of some Quadrupeds. In a Letter from Mr. WILLIAM SEWELL, *to* EVERARD HOME, *Esq. F.R.S.**

SIR,

Canal in the spinal marrow

ACCORDING to your request, I send you an account of the facts I have ascertained, respecting a canal I disco-

* Philos. Trans. for 1809, Part I. p. 146.

vered

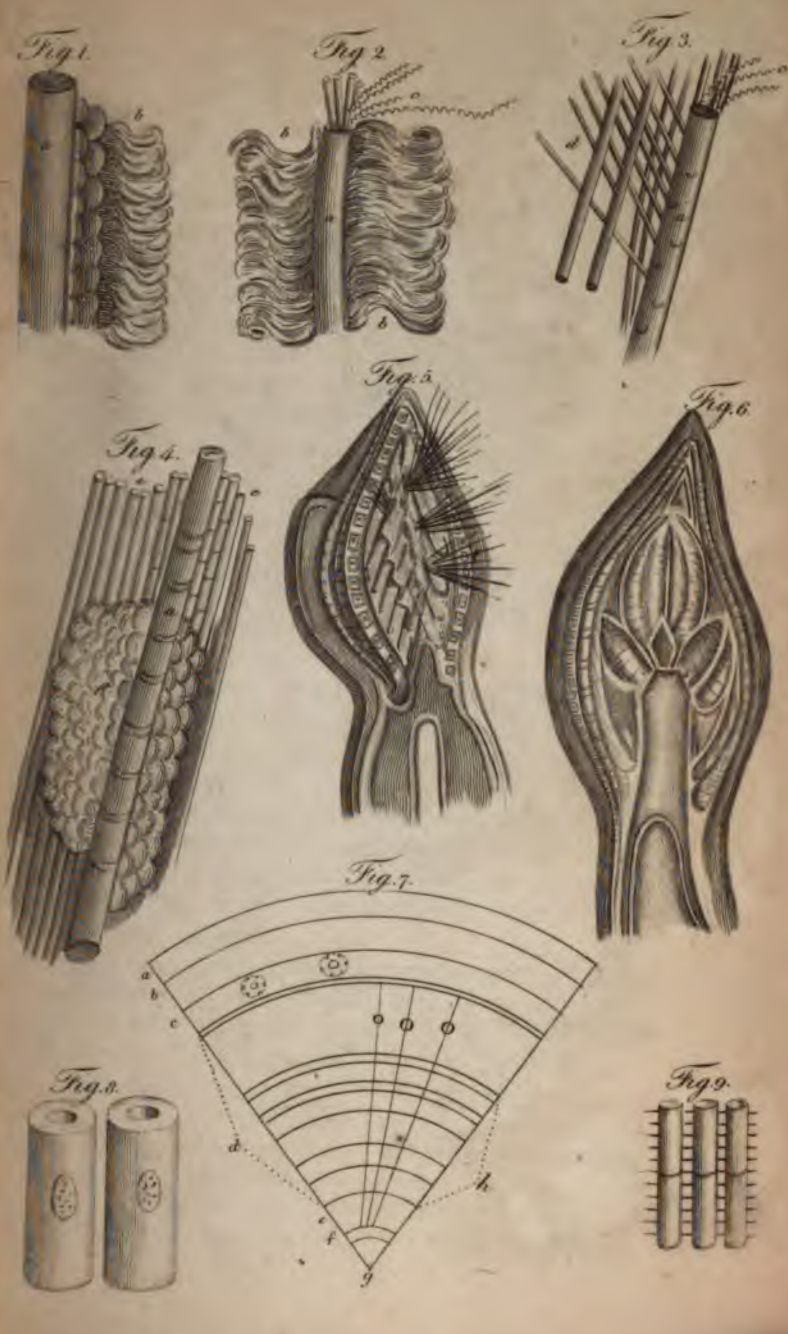


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
a
b
c
d
e
f
g
h

vered in the year 1803, in the medulla spinalis of the horse, bullock, sheep, hog, and dog; and should it appear to you deserving of being laid before the Royal Society, I shall feel myself particularly obliged, by having such an honour conferred upon me.

Upon tracing the sixth ventricle of the brain, which corresponds to the fourth in the human subject, to its apparent termination, the calamus scriptorius, I perceived the appearance of a canal, continuing by a direct course into the centre of the spinal marrow. To ascertain with accuracy whether such structure existed throughout its whole length, I made sections of the spinal marrow at different distances from the brain, and found that each divided portion exhibited an orifice with a diameter sufficient to admit a large sized pin; from which a small quantity of transparent colourless fluid issued, like that contained in the ventricles of the brain. The canal is lined by a membrane resembling the tunica arachnoidea, and is situate above the fissure of the medulla, being separated by a medullary layer: it is most easily distinguished where the large nerves are given off in the bend of the neck and sacrum, imperceptibly terminating in the cauda equina. communicating with one of the ventricles of the brain, and containing a fluid.

Having satisfactorily ascertained its existence through the whole length of the spinal marrow, my next object was to discover whether it was a continued tube from one extremity to the other: this was most decidedly proved, by dividing the spinal marrow through the middle, and pouring mercury into the orifice where the canal was cut across, it passed in a small stream with equal facility towards the brain (into which it entered), or in a contrary direction to where the spinal marrow terminates. A continuate tube the whole length of the spinal marrow.

By many similar experiments, I have since proved, that a free communication of the limpid fluid, which the canal contains, is kept up between the brain and whole extent of spinal marrow. I have consulted the most celebrated authors on comparative anatomy, but do not find any such structure of those parts described; and as it is not known to you, I may presume, that it has not been before taken notice of. I have the honour to be, The fluid has a free communication with the brain.

Sir, your obedient faithful servant,

Veterinary College, Nov. 26, 1808. WM. SEWELL.

XVII.

Note on the Alteration, that Air and Water produce in Flesh. By Mr. C. L. BERTHOLLET*.

Beef boiled in water repeatedly, exposed to the action of air,

I Boiled some beef, renewing the water from time to time, till the water no longer afforded a precipitate with tannin. I then suspended it in a glass cylinder filled with atmospheric air, which rested on a plate filled with water. After a few days the oxigen was found to be converted into carbonic acid: the interior of the cylinder was filled with a putrid smell: the beef, subjected to ebullition, again afforded a pretty copious precipitate with tannin: the boiling was repeated, till tannin ceased to render the water turbid: and the beef, having almost entirely lost its smell, was replaced in the same apparatus.

and again boiled.

This done repeatedly.

The operation was repeated several times, and the following were the results.

Results.

The alteration of the atmospherie air, and the emission of the putrid smell, gradually slackened: the quantity of gelatine formed progressively diminished: the water on which the vessel rested gave but slight indications of ammonia throughout the whole process: when I terminated it, no putrid smell was perceptible, but a smell resembling that of cheese: and in fact the animal substance, which scarcely retained any fibrous appearance, had not only the smell, but precisely the taste of old cheese.

Beef and cheese separately distilled.

I distilled separately equal weights of beef and Gruyere cheese, employing two glass bodies, each of which communicated with a tube opening under water. The operation was conducted so as to decompose the two substances as far as possible, and retain all the ammonia, that was evolved. On comparing the quantities of ammonia, that afforded by the cheese was to that of the beef nearly as 19 to 24: whence it appears, that a distinguishing characteristic of the caseous substance is to contain less nitrogen than flesh.

Less ammonia from the cheese.

* Journal de Physique, Vol. LXV, p. 466.

If

If any inference may be drawn from experiments so incomplete as the preceding, it would appear: Conclusion.

1. That the gelatine obtainable from an animal substance does not exist completely formed in it; but that, when this substance has been exhausted by the action of water, more may be formed by the action of the air, the oxigen of which combines with the carbon, while a portion of the substance, that was before solid, becomes gelatinous, as a solid part of a vegetable becomes solid by the action of the air. Gelatine not wholly formed in animal substances.

It must be remarked however, that the property of precipitating with tannin belongs to substances, that have very different qualities in other respects. I have found, that the decoction of Gruyere cheese formed a copious precipitate with tannin. Tannin affects different substances.

2. That nitrogen enters into the composition of the putrid gas, forming no doubt with hidrogen a combination less stable than ammonia, or perhaps taking an intermediate state; but, when its proportion is diminished to a certain degree, it is more strongly retained by the substance, and ceases to produce putrid gas. This substance, which is characterized by the putrid smell, appears to be rather a very evaporable compound, that unites with all gasses, like other elastic vapours, than a permanent gas. Putrid gas.

3. Since the caseous part has less nitrogen than most other animal substances, we may conjecture, that this part becomes more and more animalized during life, acquiring a greater proportion of nitrogen and hidrogen; which may be explained by the more intimate combination of the oxigen and hidrogen, that enter into its composition, and by the separation of carbon in the act of respiration; so that the last term of chemical action during life is the production of uree, agreeably to the opinion of Mr. Fourcroy*. Caseous matter. Uree.

* Syst. des Connoiss. Chim. tom. 10, p. 165; or English ed. Vol. X, p. 231.

XVIII.

XVIII.

Analysis of a Schist in the Environs of Cherbourg, taken from the Excavations made in Bonaparte Harbour. By Mr. Berthier, *Mine Engineer**.

The rock described.

CONSIDERED separately, and in small masses, this rock has all the characters of the primitive formation. It is of a dirty green colour, and has the greasiness and lustre of talc, though in a very slight degree. Its texture is slaty, and a multitude of little grains of crystalline quartz, disseminated between its laminæ, are visible to the naked eye. Some have a laminated fracture, and are probably feldspar: we may unquestionably however consider it as of intermediate formation from its situation. In fact Mr. Descotils has observed, that it contains blocks of granite, frequently pretty large and rounded; and that it alternates with ancient breccias well characterized, talky and argillaceous schists, &c.

It would have been impossible to separate the quartz mixed with it, whatever pains were taken. Besides, the person who sent it to the laboratory desired, that it should be analysed as it was.

Analysis.

Five grammes [77 grs.] were fused with double their weight of caustic potash, dissolved in pure muriatic acid, evaporated to dryness, and the silex separated. The liquor being filtered, and tested with sulphuric acid and sulphuretted hidrogen, gave no precipitate. Hidrosulphuret of ammonia formed in it a black precipitate. Being filtered, oxalate of ammonia, afterward poured into the liquor, scarcely rendered it turbid; and potash precipitated a small quantity of magnesia. The sulphurets having been redissolved in nitromuriatic acid, the whole was precipitated afresh by saturated carbonate of potash. Nothing remained in the liquor, which proved the absence of manganese. Lastly the alumine and iron were separated by caustic potash.

* Journal des Mines, vol. XXI, p. 315.

The

The results of the different operations were

Silex	68
Alumine	15
Oxide of iron	5
Magnesia	2
Lime (at most)	1
	91

consequently there was a loss of 9 per cent. I knew indeed, that the whole of the silex was not collected; but what might be supposed to have been left was far from answering to this deficiency. Great deficiency.

Accordingly I took one hundred decig. of the substance reduced to powder, and calcined them strongly in a platina crucible. They lost 3 dec., and were slightly agglutinated. Six still remained to be accounted for, and, suspecting the presence of an alkali, I sought for it after Mr. Davy's method. Water expelled.

5 grs. were fused in a silver crucible with 10 of boracic acid. The whole was diluted in water; muriatic acid was added in excess; it was evaporated to dryness; an excess of acid was added afresh; and the silex was separated by filtration. The liquor, when sufficiently evaporated, deposited a great deal of boracic acid, which was removed. The whole was then precipitated by carbonate of ammonia, boiled, and filtered. The liquor, rendered again acid, and evaporated to a pellicle, deposited boracic acid, which was removed: and, the evaporation being continued, the residuum was calcined, to drive off the ammoniacal salts. What remained still contained boracic acid; and, whatever precautions were taken, it was impossible to separate it by evaporation. Hence this method, though very convenient for detecting the presence of an alkali, appears to me not well calculated for finding its proportion. Into the liquor, reduced to a few grammes, muriate of platina was poured, which occasioned a considerable precipitate, that was found to be the triple muriate of platina and potash, as will be related in the third analysis, that was made of the same schist. Second analysis. Not well adapted to ascertain the quantity of alkali.

 This

This was undertaken for the purpose of finding the quantity of the potash, the presence of which was certain, and of the silex, which had not been obtained with certainty.

Third analysis. 10 gram. [154 grs.] of the fossil were kept a long time at a red heat with five times their weight of caustic barytes. The mixture having grown pasty, it was diluted with water and pure muriatic acid. Being evaporated to dryness, the silex was collected. It weighed 7·05 gr. It was fused again with potash, and diluted in water and a little sulphuric acid. There was a residuum of 0·4 of a gr., to which muriate of silver gave a violet colour. It was heated red hot with carbonate of potash, and washed with distilled water. The liquor contained sulphuric acid. Great part of the residuum dissolved in muriatic acid. It contained barytes and silver. The 0·4 of a gr. therefore consisted of barytes, muriate of silver, and a little silex; so that we may reckon the whole of the silex at 7·1 gr.

The barytes was precipitated from the muriatic solution by sulphuric acid; the earths, and oxide of iron, by carbonate of ammonia. The filtered liquor having been evaporated to dryness, a residuum was obtained, which, being calcined with sulphuric acid, was reduced to 0·65 of a gr. It was redissolved in a very small quantity of water, and concentrated muriate of platina was added to the solution. A precipitate took place, which was collected. The supernatant liquor, decomposed by hidrosulphuret of ammonia, filtered, and evaporated afresh, left a residuum of 0·2 of a gr., consisting entirely of lime and magnesia. The least trace of soda was not to be found. There remained then 0·45 of sulphate of potash, containing about 0·25 of alkali.

Method of distinguishing the trisule of platina with potash from that with ammonia. I satisfied myself, that the basis of this sulphate was potash by a very convenient method, which Mr. Descotils has made public, and which serves immediately to distinguish the triple muriate of platina and potash from that of platina and ammonia. It consists in boiling the precipitate in nitromuriatic acid. If it be the ammoniacal salt, it is decomposed, the ammonia is burned, and the platina dissolved. On the contrary, if it be the trisule with potash, it remains untouched; unless the quantity of the liquor be too great, in

in which case it dissolves, but reappears entirely by evaporation.

From the experiments that have been described it appears, that the schist analysed contains

Component parts of the schist.

Silex	71
Alumine	16
Oxide of iron	5
Magnesia	2
Lime (at most)	0·5
Potash	2·5
Water	3
	99
Loss	1
	100

It is possible, that the potash found in this schist comes from the feldspar, which I suspect to be in it. It would be interesting to ascertain, whether the alkali be inherent in the rock, by the analysis of a more homogeneous fragment.

The potash might be in the feldspar.

XIX.

Method of rendering common Alum as good for Dyeing as Roman Alum; by Mr. SEGUIN, *Corresponding Member of the Institute**.

TO the means that have been suggested for improving common alum, by freeing it from the iron it contains, Mr. Seguin has added a new one, founded on the different solubility of pure alum and alum contaminated with iron. He dissolves sixteen parts of common alum in twenty-four parts of water; crystallizes; and thus obtains fourteen parts of alum equal to the Roman, and two nearly equal to that of Liege.

Method of purifying alum.

This process might be employed in the manufacture of the alum, so as to obtain at first an alum worth one third more than in its impure state.

May be adopted in the manufacture.

* Sonnini's Bibliothèque Physico-économique, August 1807, p. 182.

SCIENTIFIC NEWS.

French National Institute.

French Institute.

MR. Delambre, perpetual secretary, has given an analysis of the labours of the mathematical division of the class of mathematical and physical sciences for the year 1807, of which the following is a brief account.

New construction of telescopes.

Mr. Burckhardt has proposed a mode of constructing telescopes, which he conceives will render their use more easy and convenient, than any yet adopted. His smaller mirror is plane, like Newton's, but placed perpendicular to the axis of the large concave mirror, and at half its focal distance. In this place the section of the reflected cone of light is a circle, the diameter of which is just half that of the large mirror. Accordingly the small mirror intercepts a fourth of the direct rays, but Mr. Burckhardt compensates this loss, by increasing the dimensions of the large mirror a little. The cone thus intercepted takes an inverted direction; and the rays, instead of proceeding to their focus behind the small mirror, unite at an equal distance in front of it, passing through an aperture in the centre of the large mirror. The telescope, thus reduced to half its length, will have four times as much light as a common reflecting telescope of the same length. Many objections were made to this construction, which Mr. B. answered, and it was agreed, that one should be made for trial.

Borda's circle.

The astronomers, who have lately measured the meridian line between Dunkirk and Barcelona, have employed Borda's circle to determine the time for correcting their clocks. They presume, that in an interval of four or six minutes, during which four or six observations may be taken, the altitude of the sun or a star increases with sufficient uniformity in proportion to the interval, so that a mean between the observations may be taken, and employed safely as a single observation.

Formulæ for altitudes.

Mr. Delambre and Mr. Burckhardt give several useful formulæ for taking altitudes of the stars, and likewise the moon,

moon, with precision. Mr. B. likewise proposes a new method of determining the moon's node.

Refraction of the atmosphere not affected by aqueous vapour.

Mr. Biot, before his first journey into Spain, had determined by nice experiments the refracting power of the air and of gasses, which he found to differ very little from what Mr. Delambre had inferred from his astronomical observations combined with those of Mr. Piazzi. It is well known, that refraction varies with the state and temperature of the atmosphere; and astronomers have long applied two corrections, one from the height of the barometer, the other from that of the thermometer. Since the introduction of the hygrometer, it has been questioned, whether this ought not to be employed for a third correction. During near a month, that Mr. Delambre spent in the steeple of Boiscommun, at a time when severe frosts more than once succeeded very damp mists, he endeavoured to ascertain, whether the variation of the hygrometer were attended with any change in terrestrial refraction, and found not the least indication of such a change. Mr. Laplace had made the important remark, that the refractive powers of air and the vapour of water, at equal degrees of elasticity, differed very little; but the question was of sufficient importance in astronomy, to be brought to the test of direct experiments. This Mr. Biot has undertaken. He first ascertained the effect of vapour alone. By means of potash he dried the warm air included in his prism, while that without was loaded with all the natural moisture of the atmosphere. The pressure of these two airs, indicated by a barometer within, and another without, was not the same; the difference being equal to the tension of the atmospheric vapour. The deviation of the luminous ray in the prism then gave the refraction produced by the vapour; and this never differed from what would have been produced by air alone at a similar temperature more than a few tenths of a second. The mean was 0·15″. Hence Mr. Biot infers, that the refraction produced by vapour in the atmosphere may safely be neglected in astronomy.

This doubted.

[Certain observations by some of the members of the Asiatic Society at Calcutta however lead to a different conclusion.]

Mr.

Nebula in Orion. Mr. Messier has given a beautiful delineation of the nebula in Orion, to which he has added that of Legentil, and another much more difficult to perceive, which he himself discovered in 1773.

Violent storms. He has likewise collected all the particulars of the thunder storm, that burst over Paris on the 21st of October, 1807; and the not less extraordinary gale of wind, that occurred the next day. In the observations he has registered for fifty years he finds nothing similar to it. The church of Moutivillers was struck by lightning during a storm equally violent, that took place on the 3d of November following.

Comet. On the 21st of October Mr. Pons discovered the comet at Marseilles. It was then austral, near the horizon, and set soon after the sun. It was seen a few days after by different astronomers in France and Germany, and at Madrid. Mr. Burckhardt has calculated its orbit.

Other comets. Mr. Burckhardt has found in the archives of the Imperial Observatory some unpublished observations of the comet of 1701, seen at Pau by Father Pallu. He suspects it is the same as was seen at sea in February following. Having found an important observation of the comet of 1672, he has calculated its elements afresh, and finds its perihelion distance greater than was before assigned; whence he infers, that it could not be the same with that of 1805, which some had supposed.

Tables of Jupiter and Saturn. Mr. Bouvard has accomplished a more important and more generally useful task, corrections of the tables of Jupiter and Saturn; and Mr. Delambre has availed himself of these in the ecliptic tables of Jupiter's satellites, which he has entirely reconstructed, and will shortly publish.

Adhesion of water. The only paper in physico-mathematics mentioned is Count Rumford's, printed in our Journal, Vol. XV, p. 52, from his communication.

Measure on the meridian. Beside the Memoirs of the Institute, the second volume of the "Base of the Decimal System of Measures" has been published. It contains the remainder of the observations of all kinds, and the calculation of the triangles from Dunkirk to Barcelona; the heights of the signals above the surface of the two seas; the azimuths and the latitudes

latitudes of the five principal stations. The third and last volume is in the press.

Mr. Berthoud, who died in August 1807, had published a few days before his death a supplement to his treatise on Timekeepers, with an account of his researches from 1752 to 1807. Treatise on timekeepers.

Mr. Betancourt presented to the class a model of a lock on the same principle as that invented by Mr. Huddleston. [See Journal, Vol. IV, p. 236.] He has likewise given a mathematical discussion of the principles, on which it ought to be constructed, so as to be manageable by the strength of one man. Lock for canals.

Mr. Lancret has considerably extended Mr. Monge's theory of evolutes. Evolutes.

Mr. Malus, of the corps of engineers, has deduced from a uniform and general analysis the various circumstances of the propagation of light, and a solution of the fundamental problems of optics. By a theory entirely new, founded on the properties of the intersections of a series of right lines, drawn, according to a constant law, to all the points of a given surface, Mr. Malus has determined the course of refracted and reflected rays; the intensity of light, in all cases, at any given distance from the luminous point; and the place, form, and magnitude of images. He shows, that in certain cases, and with certain surfaces, reflection and refraction produce images, that are erect in one of their dimensions, and inverted in the other, a circumstance never before noticed*. Propagation of light.

The propagation and reflection of sound have some re- Propagation

* The plane mirror, or common looking-glass, in fact shows objects erect in the perpendicular, and inverted with respect to right and left. But this is not what the reporter means, though he does not inform us, what the construction of the mirror of Mr. Malus is. It would be found however, that a mirror, which is a section of a concave cylinder, will represent the horizontal dimension of an object the reverse of what a plane mirror would do, without affecting the perpendicular; in other words, the spectator would see the image of himself, or any other object in it, exactly in the same position, as if he stood facing the object, that occasioned the image: and this no doubt is the mirror alluded to, which is of a kind, that I do not recollect to have seen mentioned. C. Mirror of a new kind.

semblance

and reflection of sound. semblance to those of light, but their theory is attended with more difficulty. As the velocity of sound is very small, it might be questioned how far it depended on a simple law. Messrs. Lagrange and Euler, who first treated this problem, supposed it in a particular case to depend only on its distance from the centre of motion. Mr. Poisson has just demonstrated generally, in a very ingenious manner, that the law is always the same: that the movement is propagated by spherical undulations with the same velocity in every direction; but that the vibration of particles situate at the same moment in the sonorous wave are made with unequal rapidity, according to a law depending on the nature of the primary agitation; and consequently, that the intensity of the sound, which depends on the velocity of these vibrations, is thus found to be different in different parts of the sonorous wave. The velocity in a given radius decreases in the ratio of the distance; whence it follows, if the intensity be proportional to the square of the velocity, it must decrease in the proportion of the square of the distance.

Only two determinate roots of the general equation had been found, but the formulæ of Mr. Poisson comprise an infinite number, by which may be verified all the theorems he has obtained in the general case, to which he first paid attention. He afterward considers the case where there are several causes of a simultaneous vibration; and without affecting the generality of the root, he decomposes it so, that the different parts answer to the different centres; which leads him to give in a novel and ingenious manner the theory of the reflexion of sound, and production of echoes; and to show what would take place between opposite and parallel planes. By a similar method he explains what must occur in the far more difficult case, where the mass of air set in motion is included in an ellipsoid. He demonstrates, that the sound, which originates in one of the foci, is reflected toward the other, making the angle of reflection equal to that of incidence, and following the same laws as light. These results are conformable with what we have learned by experience of elliptical vaults, but it was very difficult to demonstrate them mathematically, which Mr. Poisson has done in a new and ingenious manner.

It

Velocity of sound.

It has long been remarked, that the observed velocity of sound is superior to what is deduced from algebraical calculations. It may be conceived, that the density and temperature of the air have some influence in this; but Mr. Poisson demonstrates, that they are insufficient to explain the observations. Having examined successively the causes supposed by Newton and other geometricians, he finds them incompatible with the results of sound philosophy. Mr. Laplace attributes the acceleration of sound to the change of temperature experienced by the particles of air in their condensation and dilatation, which cannot take place without a successive evolution and absorption of heat, Calculation applied to this hypothesis, or rather incontestable fact, shows, from experiments made by the Academy of Sciences in 1738, that a dilatation or condensation of $\frac{1}{116}$ produces a change of temperature equal to a degree of the centesimal thermometer [1·8 Fah.].

The labours of the physical division of the class have been analysed by Mr. Cuvier, perpetual secretary.

Hibernation of animals.

In 1804 the class had awarded a prize to Doctors Herholdt and Rafn, of Copenhagen, for a paper on the winter sleep of animals; and, in 1807, another to Dr. Saissy, of Lyons. Prof. Prunelle, of Montpellier, has since sent a paper, that may rank with the best on the subject. Still however, notwithstanding their researches, and those of Spallanzani, Mangili, and Carlisle, we are ignorant of the causes, by which certain animals are disposed to this sleep, and not others; as well as of those that enable them to endure this suspension of their functions.

Comparative osteology.

Mr. Geoffroy-Saint-Hilaire, Prof. at the Museum of Natural History, elected to succeed the late Mr. Broussonnet, presented to the class some fragments of a great work, which he has undertaken on comparative osteology. His object is to investigate more minutely the analogies between the corresponding parts of various animals with vertebræ. In fact those parts of organs, that are always found more or less similar in number and position, notwithstanding their difference in size and use, and contradictoriness to all apparent final causes, must necessarily depend on efficient and formative causes. As these must be connected with the primary means

means employed by nature, if we may flatter ourselves with ever throwing any light on the origin of organized bodies, the most obscure and mysterious point of natural history, it seems to us the first sparks must be derived from these analogies of structure.

Mechanism of respiration in fishes.

Mr. Dumeril, prof. of anatomy at the Medical School; presented three papers. In the first he treated on the mechanism of respiration in fishes, and pointed out some interesting singularities. Those that from having their mouths sometimes affixed to stones, or buried in mud or sand, cannot always use them for taking in water, are provided with apertures for admitting the water on dilating the cavity of the mouth, and these apertures are furnished with valves internally, to prevent the water from returning by them, so that it has no exit but by the gills.

Organ of taste in fishes.

The second was on the smell and taste of fishes. Mr. D. supposes, that the tongue, from the dryness and hardness of its integuments, and the constant passage of water over it, must be insensible to flavours; and that the pituitary membrane, not being exposed to the impulse of elastic vapour, cannot be the seat of smell like ours. This membrane therefore he conceives to be the organ of taste.

Reptiles,

The third is a comparison of the various vital and animal functions in the order of reptiles termed *batrachian*, which justifies its division into two families.

Crocodiles,

Several other papers on reptiles have been produced, particularly on crocodiles, of which Mr. Cuvier has shown no less than twelve distinct species exist in the old and new world.

Amphibia.

The same naturalist has endeavoured to remove by dissection the doubts entertained respecting some reptiles of a singular form, which truly deserve the name of amphibia, because they breathe both with gills and lungs. One of these is the siren lacertina, another the proteus anguinus*, and a third the proteus pisciformis. The two former of these at least have the skeleton too firmly ossified, and too different from those of any other reptile of their native abodes, and besides their organs are too perfect, to admit of their being

* See Journal, Vol. XVIII, p. 91.

considered

considered as tadpoles, that have a change to undergo. The last inhabits the lakes of Mexico, where it is used as food, and resembles a water lizard, except in having gills. It is called there *axolotl*, and was brought over by Humboldt. Axolotl.

Air-bladders of fishes. Mr. Biot, while employed in measuring an arc of the meridian at the Balearic Islands, thinks he has observed, that part of the intestines of fishes caught by a hook and line at great depths, and drawn up suddenly, issue out of their mouths, which he attributes to the expansion of the air-bladder. He has likewise examined the nature of the air in this bladder, and found it to vary from pure nitrogen to a mixture of this gas with 0·87 oxigen, but he discovered no hidrogen. It appeared to him, that, the deeper the fish lived under water, the more oxigen the air contained.

Entomology. Mr. Jurine is extending his new method of classing insects*, which is found to be more natural than could have been expected, to the diptera.

Nerves of the lungs necessary in breathing. Mr. Dupuytren, head of the anatomical department of the Medical School, has shown, that the concurrence of the nerves of the lungs in the act of respiration is necessary to the conversion of the venous blood into arterial.

The science of botany has been sedulously pursued. Mr. de Labillardiere has finished his Flora of New Holland.

Growth of vegetables. Mr. Dupetit-Thouars continues his researches on the growth of vegetables. He still thinks, that the trunk of trees has the principle of its increase in the buds; and that the fibres composing the annual layers of wood are in some sort the roots of the buds, while the little medullary thread terminating each bud performs the functions of cotyledons. He has endeavoured to answer objections, and brought forward many interesting facts. Among these is the germination of the *lecythis*. The evolution of the seed of this tree, which is dicotyledonous, cannot be referred to either of the three modes hitherto adopted. Its cotyledon is interior, and serves as a base to the pith, which Mr. D.T. thinks a proof of the justice of his opinion. The cuttings of the

* See Journal, Vol. XVIII. p. 218.

willow,

willow, that take root though deprived of their buds, seem to furnish a strong objection to it; but he has found, that in this case little subsidiary buds are unfolded opposite points that were occupied by the stipules of the leaves.

Carbon of plants.

There is no subject of more general importance in the vegetable economy than the origin of the carbon of plants. Mr. Crell, the celebrated chemist of Helmstadt, has this year communicated to the class some experiments, that seem to give a very high notion of the power of vegetation. He asserts, that he has made plants grow and produce seed in pure sand, watering them only with distilled water, and supplying them with a given quantity of air, in which the carbonic acid must be almost as nothing in proportion to the carbon produced. It is to be observed however, that, though the plants were covered with a glass, he could not prevent the access of the external air through the sand.

Philosophical and chemical Society of Arcueil.

Messrs. Laplace, C. L. Berthollet, Biot, Gay Lussac, von Humboldt, Thenard, Decandolle, Collet-Descotils, and A. B. Berthollet, have formed a society under the name of Philosophical and Chemical at the village of Arcueil, near Paris, which meets once a fortnight, and published the first vol. of its Memoirs in 1807.

Berlin Society.

At the Royal Academy of Sciences at Berlin, the sixth of August last, a paper on the resistance of the air was read by Mr. Burja; one on the advantages and disadvantages of national prejudices by Mr. Klein; and a fragment on the great cataracts of the river Oronoko by Mr. von Humboldt. The following prize subject is proposed for 1810. "To "give a complete theory of the hydraulic ram, paying regard "to the adhesion of water*."

Prize question.

Dr. Gauss has sent to the Royal Society of Gottingen the following observations of two of the new planets.

1st. Observations of Pallas.

Observations of Pallas.

1806.		Mean time. *hours.*			Apparent right ascens.			Apparent declination.		
Feb.	14.	8	11′	16″	70°	16′	31″	19°	59′	13″ S.
	16.	7	32	28	70	42	39	19	20	44
	17.	6	52	38	70	56	44	19	1	8
	20.	7	49	35	71	39	2	18	5	0

* For a description of its mechanism, and some remarks on it, see Journal, vol. XIV, p. 98.

2d. Obser-

2d. *Observations of Juno.*

Of Juno.

1806.	Mean time. hrs.	Apparent right ascens.	Apparent declin.
Feb. 17.	9 42′ 0″	173° 46′ 45″	0° 28′ 32″ N.
20.	10 49 47		0 54 18
	10 59 2	173 15 57	
	13 12 18	173 15 15	

The following observations of Juno were made at Gottingen.

1806.	Mean time. hrs.	Apparent right ascension.	Apt. declin.
March 10	9 53′ 56·3″	169° 46′ 54·5″	3° 41′ 50·3″
11.	10 32 22·7	169 34 18	3 51 56·5

Dr. Gauss has likewise sent new elements of the orbit of Ceres, deduced from the last opposition observed by prof. Pasquich, which the doctor means to render more correct, when he has observations of this opposition on which he can better rely.

Elements of Ceres.

Epoch of the longitude, meridian of Seeberg..........	108°	19′	34·7″
Diurnal tropical motion			770″ 85′ 84
Annual	78	9	23
Aphelion, 1806	326	37	59
Annual motion	+	2	1·2
Ascending node, 1806......	80	53	23
Annual motion............	+		1·5
Inclination of the orbit, 1806	10	37	34
Annual diminution			0·4
Eccentricity, 1806	0·0783486		
Annual diminution	0·0000058		
Log. of the greater semiaxis..	0·4420728		

To the observations of Vesta, given in our Journal, vol. XVIII, p. 75, we can now add the following.

Observations of Vesta.

1807.	Mean time. hrs.	Apparent right ascension.	Apparent declin.
April 1.	9 50′	183° 28′	12° 5′ N.
5.	11 17 2·784″	182 33 10·92	12 24 19·1″
6.	11 12 16·022	182 20 47·91	12 27 54·4

The

The first of these is by Dr. Olbers, the other two from the observatory of Gottingen.

Dr. Gauss has determined its elements in the following manner.

Elements of Vesta.

Epoch of the mean longitude at Bremen, March 29, 1807, at 12 o'clock, mean time	193°	8′	4·6″*
Longitude of its perihelion	249	7	41
——— aphelion	69	57	52
——— ascending node on the ecliptic	103	8	36
Inclination of its orbit	7	5	49·5 †
Diurnal tropical motion	0	16	18·91
Logarithm of the mean distance	0·3728428		
Eccentricity	0·097505		
Greatest distance from the sun	25·625		
Least	21·614		
Period of its revolution	1321 days, 12 hours.		

Mathematical part of Humboldt & Bonpland's travels.

The fourth part of von Humboldt and Bonpland's Travels will contain in two 4to. vols. the astronómical observations, trigonometrical operations, and barometrical measures. Mr. von H. has thought it would be most satisfactory to give the whole of the original observations themselves, that it may be seen what degree of confidence the results deduced from them deserve. The calculations have been made by Mr. Jabbo Oltmanns from the best tables. The magnetical observations, with an examination of them and of those of Cook, Vancouver, and other able astronomers, by Biot, will occupy the 2d. vol. As such a number of figures must be a long while printing, the latitudes and longitudes of various places, deduced from astronomical observations, have been published in a separate tract in Latin.

Statistical account of Mexico.

In the third part of their travels, consisting of a statistical Essay on the Kingdom of New Spain, they estimate the present population of Mexico at more than six millions.

* In the Magazin Encyclopédique it is 192° 9′ 54″.

† Ibid. 7° 8′ 34′.

They

They likewise give the following comparative table of births and deaths.

	BIRTHS.	DEATHS.
In France	110	100
In England	120	100
In Sweden	130	100
In Finland	160	100
In the Russian Empire	166	100
In Western Prussia	180	100
In the government of Tobolsk	210	100
In several parts of the high plains of Mexico	230	100
In the state of New Jersey, North America	300	100

Table of mortality in various places.

Famine however not unfrequently interferes, to check the population of Mexico. In 1784 no less than 300000 died for want. The mortality among the miners does not appear to be greater than in other classes. The heat of most of these mines is very considerable. At the bottom of that of Valenciana, at the depth of 513 met. [560 yards] the centigrade thermometer was at 34° [93·2° Fahr.], while in the open air in winter it is only 4° or 5° above 0 [from 39·2° to 41° F.].

Famine.

Miners not unhealthy.

Heat of the mines.

On the 22d of August last Mr. Andreoli and Mr. Brioschi went up with a balloon at Padua. When the mercury had fallen to 15 inches [about the height of 3½ miles] Mr. B. began to feel an extraordinary palpitation of the heart, without any painful sensation in breathing. When the mercury was down to 12 [4½ miles] he was overpowered with a pleasing sleep, that soon became a real lethargy. The balloon continued ascending, and when the mercury was about 9 inches [near 6 miles] Mr. A. perceived himself swollen all over, and could not move his left hand. When the mercury had fallen to 8·5 [about 6 miles and a quarter high] the balloon burst with a loud explosion, began to descend rapidly with much noise, and Mr. B. awoke. It fell about 12 miles from Padua, without any injury being received by the aërial travellers.

Ascent with a balloon to a great height.

The scheme of bishop Wilkins I understand has been pursued with some success at Vienna. A watchmaker of the name of Degen is reported to have ascended above the trees in the Prater with artificial wings, taken his flight in various directions, and alighted on the ground with as much ease as a bird.

Artificial wings.

Meteorolo-

METEOROLOGICAL JOURNAL,

For JULY, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JUNE Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
26	57	58	65	51	30·42	Fair	Cloudy
27	58	59	67	52	30.22	Ditto	Fair
28	57	56	62	50	30·08	Rain	Cloudy
29	56	57	61	51	30·01	Ditto	Rain
30	57	56	62	54	29·95	Ditto	Fair
JULY							
1	56	63	67	58	29·83	Fair	Ditto
2	63	58	62	51	29·79	Rain *	Rain
3	53	51	63	49	29·53	Ditto	Ditto
4	49	52	55	49	29·48	Ditto †	Cloudy
5	52	53	55	53	29·52	Ditto	Ditto
6	53	58	60	57	29·70	Ditto ‡	Ditto
7	62	61	65	60	29·81	Ditto	Ditto ‖
8	61	59	66	52	29·82	Ditto	Rain
9	53	52	56	52	29·88	Ditto §	Cloudy
10	52	53	57	50	29·88	Ditto	Fair
11	53	54	64	58	30·03	Fair	Ditto
12	61	65	68	62	30·09	Ditto	Ditto
13	62	62	68	55	30·09	Ditto	Ditto
14	62	62	69	60	30·18	Ditto	Ditto
15	63	63	68	60	30·09	Ditto	Ditto
16	63	64	72	59	30·00	Ditto	Ditto
17	62	59	67	53	29·76	Ditto	Ditto
18	58	55	60	50	29·90	Ditto	Ditto
19	56	62	65	57	30·03	Ditto	Ditto
20	60	62	66	55	30·12	Ditto	Ditto
21	60	61	65	55	30·20	Ditto	Ditto
22	61	62	63	56	30·05	Ditto	Ditto
23	59	61	69	60	29·89	Ditto	Ditto
24	60	61	69	60	29·86	Ditto	Cloudy
25	62	65	74	61	29·78	Ditto	Ditto ¶

* A.M. at 1 P.M. thunder and lightning the thermometer retiring 9°.
† Hail, thunder and lightning at 2 P.M. the thermometer retiring 4°.
‡ Rain the whole day.
‖ At 11, lightning, thunder, and heavy rain.
§ Rain the whole day.
¶ Heavy rain, thunder, and lightning in the night.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXIII.

ARTICLE I.

The Bakerian Lecture. An Account of some New analytical Researches on the Nature of certain Bodies, &c. By HUMPHRY DAVY, ESQ. *Sec. R. S. F. R. S. Ed. and M. R. I. A.*

(Continued from Page 257.)

3. *Analytical Experiments on Sulphur.*

I HAVE referred, on a former occasion*, to the experiments of Mr. Clayfield and of Mr. Berthollet jun., which seemed to show that sulphur, in its common form, contained hidrogen. In considering the analytical powers of the voltaic apparatus, it occurred to me, that though sulphur, from its being a nonconductor, could not be expected to yield its elements to the electrical attractions and repulsions of the opposite surfaces, yet that the intense heat connected with the contact of these surfaces might possibly effect some alteration in it, and tend to separate any elastic matter it might contain. Sulphur seemed to contain hidrogen.

On this idea some experiments were instituted in 1807. A curved glass tube, having a platina wire hermetically sealed in its upper extremity, was filled with sulphur. [See our last Number, Pl. VII, Fig. 4.] The sulphur was melted Experiments to ascertain this.

* Bakerian Lecture, 1808, p. 16; or Journal, Vol. xx, p. 302.

over a spirit lamp; and a proper connection being made with the voltaic apparatus of one hundred plates of six inches, in great activity, a contact was made in the sulphur by means of another platina wire. A most brilliant spark, which appeared orange coloured through the sulphur, was produced, and a minute portion of elastic fluid rose to the upper extremity of the tube. By a continuation of the process for nearly an hour, a globule equal to about the tenth of an inch in diameter was obtained, which, when examined, was found to be sulphuretted hidrogen.

Sulphuretted hidrogen produced.

But the sulphur might have contained water.

This result perfectly coincided with those which have been just mentioned; but as the sulphur that I had used was merely in its common state, and as the ingenious experiments of Dr. Thompson have shown, that sulphur in certain forms may contain water, I did not venture, at that time, to form any conclusion upon the subject.

The experiment repeated with pure sulphur.

In the summer of the present year, I repeated the experiment with every precaution. The sulphur that I employed was Sicilian sulphur, that had been recently sublimed in a retort filled with nitrogen gas, and that had been kept hot till the moment that it was used. The power applied was that of the battery of five hundred double plates of six inches, highly charged. In this case the action was most intense, the heat strong, and the light extremely brilliant; the sulphur soon entered into ebullition, elastic matter was formed in great quantities, much of which was permanent; and the sulphur, from being of a pure yellow, became of a deep red brown tint.

Sulphuretted hidrogen produced, and part of the sulphur acidified? Large quantity evolved.

The gas, as in the former instance, proved to be sulphuretted hidrogen. The platina wires were considerably acted upon; the sulphur, at its point of contact with them, had obtained the power of reddening moistened limtus paper.

I endeavoured to ascertain the quantity of sulphuretted hidrogen evolved in this way from a given quantity of sulphur, and for this purpose, I electrized a quantity equal to about two hundred grains in an apparatus of the kind I have just described, and when the upper part of the tube was full of gas, I suffered it to pass into the atmosphere; so as to enable me to repeat the process.

When

When I operated in this way, there seemed to be no limit to the generation of elastic fluid, and in about two hours a quantity had been evolved, which amounted to more than five times the volume of the sulphur employed. From the circumstances of the experiment, the last portion only could be examined, and this proved to be sulphuretted hidrogen. Towards the end of the process, the sulphur became extremely difficult of fusion, and almost opaque, and when cooled and broken, was found of a dirty brown colour.

Sulphur and potassium evolve sulphuretted hidrogen.

The experiments upon the union of sulphur and potassium, which I laid before the Society last year, prove that these bodies act upon each other with great energy, and that sulphuretted hidrogen is evolved in the process, with intense heat and light.

Potassium heated with compound inflammables.

In heating potassium in contact with compound inflammable substances, such as resin, wax, camphor, and fixed oils, in close vessels out of the contact of the air, I found, that a violent inflammation was occasioned; that hidrocarbonate was evolved; and that when the compound was not in great excess, a substance was formed, spontaneously inflammable at common temperatures, the combustible materials of which were charcoal and potassium.

Pyrophorus.

Analogies.

Here was a strong analogy between the action of these bodies and sulphur on potassium. Their physical properties likewise resemble those of sulphur; for they agree in being nonconductors, whether fluid or solid, in being transparent when fluid, and semitransparent when solid, and highly refractive; their affections by electricity are likewise similar to those of sulphur; for the oily bodies give out hidrocarbonate by the agency of the voltaic spark, and become brown, as if from the deposition of carbonaceous matter.

Hidrogen certainly exists in sulphur.

But the resinous and oily substances are compounds of a small quantity of hidrogen and oxigen, with a large quantity of a carbonaceous basis. The existence of hidrogen in sulphur is fully proved, and we have no right to consider a substance, which can be produced from it in such large quantities, merely as an accidental ingredient.

Attempt to ascertain whether sulphur form water by burning in dry oxigen.

The oily substances in combustion produce two or three times their weight of carbonic acid and some water. I endeavoured to ascertain, whether water was formed in the combustion of sulphur in oxigen gas, dried by exposure to potash; but in this case sulphureous acid is produced in much larger quantities than sulphuric acid, and this last product is condensed with great difficulty. In cases, however, in which I have obtained, by applying artificial cold, a deposition of acid in the form of a film of dew in glass retorts out of the contact of the atmosphere, in which sulphur had been burned in oxigen gas hygrometrically dry, it has appeared to me less tenacious and lighter than the common sulphuric acid of commerce, which in the most concentrated form in which I have seen it, namely, at 1·855, gave abundance of hidrogen as well as sulphur at the negative surface in the voltaic circuit, and hence evidently contained water.

Reddening of the litmus might be by sulphuretted hidrogen.

The reddening of the litmus paper, by sulphur that had been acted on by voltaic electricity, might be ascribed to its containing some of the sulphuretted hidrogen formed in the process; but even the production of this gas, as will be immediately seen, is an evidence of the existence of oxigen in sulphur.

Potassium heated in sulphuretted hidrogen.

In my early experiments on potassium, procured by electricity, I heated small globules of potassium in large quantities of sulphuretted hidrogen, and I found that sulphuret of potash was formed; but this might be owing to the water dissolved in the gas, and I ventured to draw no conclusion till I had tried the experiment in an unobjectionable manner.

Perfectly dried,

I heated four grains of potassium in a retort of the capacity of twenty cubical inches; it had been filled after the usual processes of exhaustion with sulphuretted hidrogen, dried by means of muriate of lime that had been heated to whiteness; as soon as the potassium fused, white fumes were copiously emitted, and the potassium soon took fire, and burnt with a most brilliant flame, yellow in the centre and red towards the circumference *.

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The diminution of the volume of the elastic matter, in this operation, did not equal more than two cubical inches and a half. A very small quantity of the residual gas only was absorbable by water. The nonabsorbable gas was hidrogen, holding a minute quantity of sulphur in solution.

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A yellow sublimate lined the upper part of the retort, which proved to be sulphur. The solid matter formed was red at the surface like sulphuret of potash, but in the interior it was dark gray, like sulphuret of potassium. The piece of the retort containing it was introduced into a jar inverted over mercury, and acted upon by a small quantity of dense muriatic acid, diluted with an equal weight of water, when there were disengaged two cubical inches and a quarter of gas, which proved to be sulphuretted hidrogen.

and sulphur sublimed. Solid matter.

In another experiment, in which eight grains of potassium were heated in a retort of the capacity of twenty cubical inches, containing about nineteen cubical inches of sulphuretted hidrogen, and a cubical inch of phosphuretted hidrogen, which was introduced for the purpose of absorbing the oxigen of the small quantity of common air admitted by the stop-cock, the inflammation took place as before; there was a similar precipitation of sulphur on the sides of the retort; the mass formed in the place of the potassium was orange externally, and of a dark gray colour internally, as in the last instance; and when acted on by a little water holding muriatic acid in solution, there were evolved from it five cubical inches only of sulphuretted hidrogen.

Experiment repeated.

Both these experiments concur in proving the existence of a principle in sulphuretted hidrogen, capable of destroying partially the inflammability of potassium, and of producing upon it all the effects of oxigen; for had the potassium combined merely with pure combustible matter, it ought, as will be seen distinctly from what follows, to have evolved by the action of the acid a volume of sulphuretted

Principle in sulphuretted hidrogen producing the effects of oxigen.

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Sulphur heated n hidrogen.

Sulphuretted hidrogen, as has been long known to chemists, may be formed by heating sulphur strongly in hidrogen gas. I heated four grains of sulphur in a glass retort, containing about twenty cubical inches of hidrogen, by means of a spirit lamp, and pushed the heat nearly to redness. There was no perceptible change of volume in the gas after the process; the sulphur that had sublimed was unaltered in its properties, and about three cubical inches of an elastic fluid absorbable by water were formed: the solution reddened litmus, and had all the properties of a solution of pure sulphuretted hidrogen. Now if we suppose sulphuretted hidrogen to be constituted by sulphur dissolved in its unaltered state in hidrogen, and allow the existence of oxigen in this gas; its existence must likewise be allowed in sulphur, for we have no right to assume, that sulphur in sulphuretted hidrogen is combined with more oxigen than in its common form: it is well known, that, when electrical sparks are passed through sulphuretted hidrogen, a considerable portion of sulphur is separated, without any alteration in the volume of the gas. This experiment I have made more than once, and I found that the sulphur obtained, in fusibility, combustibility, and other sensible properties, did not perceptibly differ from common sublimed sulphur.

Oxigen in sulphur

accounts for its intense ignition with potassium.

According to these ideas, the intense ignition produced by the action of sulphur, on potassium and sodium, must not be ascribed merely to the affinity of the metal of the alkalis for its basis, but may be attributed likewise to the agency of the oxigen that it contains.

The minute examination of the circumstances of the action of potassium and sulphur likewise confirms these opinions.

Farther confirmed.

When two grains of potassium and one of sulphur were heated gently in a green glass tube filled with hidrogen, and connected with a pneumatic apparatus, there was a most intense ignition produced by the action of the two bodies, and one eighth of a cubical inch of gas was disengaged

engaged, which was sulphuretted hidrogen. The compound was exposed in a mercurial apparatus to the action of liquid muriatic acid; when a cubical inch and a quarter of aeriform matter was produced, which proved to be pure sulphuretted hidrogen.

The same experiment was repeated, except that four grains of sulphur were employed instead of one. In this case, a quarter of a cubical inch of gas was disengaged during the process of combination; and when the compound was acted upon by muriatic acid, only three quarters of a cubical inch of sulphuretted hidrogen were obtained.

Now, *sulphuret* of potash produces sulphuretted hidrogen by the action of an acid; and if the sulphur had not contained oxigen, the hidrogen evolved by the action of the potassium in both these experiments ought to have equalled at least two cubical inches, and the whole quantity of sulphuretted hidrogen ought to have been more: and that so much less sulphuretted hidrogen was evolved in the second experiment, can only be ascribed to the larger quantity of oxigen furnished to the potassium by the larger quantity of the sulphur.

Several experiments made with similar results.

I have made several experiments of this kind with similar results. Whenever equal quantities of potassium were combined with unequal quantities of sulphur, and exposed afterward to the action of muriatic acid, the largest quantity of sulphuretted hidrogen was furnished by the product containing the smallest proportion of sulphur; and in no case was the quantity of gas equal in volume to the quantity of hidrogen, which would have been produced by the mere action of potassium upon water.

Composition of sulphur.

From the general tenour of these various facts, it will not be, I trust, unreasonable to assume, that sulphur, in its common state, is a compound of small quantities of oxigen and hidrogen with a large quantity of a basis, that produces the acids of sulphur in combustion, and which, on account of its strong attractions for other bodies, it will probably be very difficult to obtain in its pure form.

In metallic combinations even, it still probably retains its oxigen and part of its hidrogen. Metallic sulphurets can only be partially decomposed by heat, and the small quantity

quantity of sulphur evolved from them in this case when perfectly dry and out of the contact of air, as I found in an experiment on the sulphuret of copper and iron, exists in its common state, and acts upon potassium, and is affected by electricity, in the same manner as native sulphur.

4. *Analytical Experiments on Phosphorus.*

Phosphorus analogous to sulphur.

The same analogies apply to phosphorus as to sulphur, and I have made a similar series of experiments on this inflammable substance.

Acted on by the pile,

Common electrical sparks, passed through phosphorus, did not evolve from it any permanent gas; but when it was acted upon by the voltaic electricity of the battery of five hundred plates in the same manner as sulphur, gas was produced in considerable quantities, and the phosphorus became of a deep red brown colour, like phosphorus that has been inflamed and extinguished under water. The gas examined proved to be phosphuretted hidrogen, and in one experiment, continued for some hours, a quantity estimated to be nearly equal to four times the volume of the phosphorus employed was given off. The light of the voltaic spark in the phosphorus was at first a brilliant yellow, but as the colour of the phosphorus changed, it appeared orange.

evolved phosphuretted hidrogen.

Potassium heated in phosphuretted hidrogen.

I heated three grains of potassium in sixteen cubical inches of phosphuretted hidrogen; as soon as it was fused, the retort became filled with white fumes, and a reddish substance precipitated upon the sides and upper part of it. The heat was applied for some minutes. No inflammation took place*. When the retort was cool, the absorption was found to be less than a cubical inch. The potassium externally was of a deep brown colour, internally it was of a dull lead colour. The residual gas had lost its property of spontaneous inflammation, but seemed still to contain a small quantity of phosphorus in solution.

* It is stated, in the account before referred to of M. M. Gay-Lussac and Thenard's experiments, that potassium inflames in phosphuretted hidrogen. My experiments upon this gas have been often repeated. I have never perceived any luminous appearance; but I have always operated in daylight.

The phosphuret acted upon over mercury by solution of muriatic acid evolved only one cubical inch and three quarters of phosphuretted hidrogen.

From this experiment there is great reason to suppose, that phosphuretted hidrogen contains a minute proportion of oxigen, and consequently that phosphorus likewise may contain it; but the action of potassium on phosphorus itself furnishes perhaps more direct evidences of the circumstance. Phosphuretted hidrogen contains oxigen.

One grain of potassium and one grain of phosphorus were fused together in a proper apparatus. They combined with the production of the most vivid light and intense ignition. During the process one tenth of a cubical inch of phosphuretted hidrogen was evolved. The phosphuret formed, exposed to the action of diluted muriatic acid over mercury, produced exactly three tenths of a cubical inch of phosphuretted hidrogen. Phosphorus fused with potassium.

In a second experiment, one grain of potassium was fused with three grains of phosphorus; in this case nearly a quarter of a cubical inch of phosphuretted hidrogen was generated during the ignition. But from the compound exposed to muriatic acid, only one tenth of a cubical inch could be procured. Experiment repeated.

Now it is not easy to refer the deficiency of phosphuretted hidrogen in the second case to any other cause, than to the supply of oxigen to the potassium from the phosphorus: and the quantity of phosphuretted hidrogen evolved in the first case is much less than could be expected, if both potassium and phosphorus consisted merely of pure combustible matter. Phosphorus contains oxigen.

The phosphoric acid, formed by the combustion of phosphorus, though a crystalline solid, may still contain water. The hidrogen evolved from phosphorus by electricity proves indeed, that this must be the case; and though the quantity of hidrogen and oxigen in phosphorus may be exceedingly small, yet they may be sufficient to give it peculiar characters; and till the basis is obtained free, we shall have no knowledge of the properties of the pure phosphoric element. Phosphoric acid may contain water.

5. On

5. *On the States of the carbonaceous Principle in Plumbago, Charcoal, and the Diamond.*

Plumbago, charcoal, and diamond,

The accurate researches of Messrs. Allen and Pepys have distinctly proved, that plumbago, charcoal, and the diamond produce very nearly the same quantities of carbonic acid, and absorb very nearly the same quantities of oxigen in combustion.

consist principally of the same element,

Hence it is evident, that they must consist principally of the same kind of elementary matter; but minute researches upon their chemical relations, when examined by new analytical methods, will, I am inclined to believe, show, that the great difference in their physical properties does not merely depend upon the differences of the mechanical arrangement of their parts, but likewise upon differences in their intimate chemical nature.

but with chemical differencies.

Plumbago acted upon by the pile in vacuo.

I endeavoured to discover, whether any elastic matter could be obtained from plumbago very intensely ignited by the Voltaic battery in a Torricellian vacuum: but though the highest power of the battery of five hundred was employed, and though the heat was such, as in another experiment instantly melted platina wire of $\frac{1}{70}$th of an inch in diameter, yet no appearance of change took place upon the plumbago. Its characters remained wholly unaltered, and no permanent elastic fluid was formed.

Heated with potassium in hidrogen gas.

I heated one grain of plumbago, with twice its weight of potassium, in a plate glass tube connected with a proper apparatus, and I heated an equal quantity of potassium alone in a tube of the same kind, for an equal length of time, namely, eight minutes. Both tubes were filled with hidrogen: no gas was evolved in either case. There was no ignition in the tube containing the plumbago, but it seemed gradually to combine with the potassium. The two results were exposed to the action of water; the result from the plumbago acted upon that fluid with as much energy as the other result, and the two volumes of elastic fluids were 1·8 cubical inch and 1·9 cubical inch; and both gave the same diminution by detonation with oxigen, as pure hidrogen. Two grains of potassium, by acting upon water, would have produced two cubical inches and one eighth of

of hidrogen gas; the deficiency in the result, in which potassium alone was used, must be ascribed to the loss of a small quantity of metal, which must have been carried off in solution in the hidrogen, and perhaps, likewise, to the action of the minute quantity of metallic oxides in the plate glass. The difference in the quantity of hidrogen given off in the two results is however too slight, to ascribe it to the existence of oxigen in the plumbago.

I repeated this experiment several times with like results, and in two or three instances examined the compound formed. It was infusible at a red heat, had the lustre of plumbago. It inflamed spontaneously, when exposed to air, generated potash, and left a black powdery residuum. It effervesced most violently in water, and produced a gas, which burnt like pure hidrogen.

The experiment repeated.

When small pieces of charcoal from the willow, that had been intensely ignited, were acted upon by Voltaic electricity in a Torricellian vacuum, every precaution being taken to exclude moisture from the mercury and the charcoal, the results were very different from those occurring in the case of plumbago.

Charcoal acted upon by the pile in vacuo.

When plumbago was used, after the first spark, which generally passed through a distance of about one eighth of an inch, there was no continuation of light, without a contact or an approach to the same distance; but from the charcoal a flame seemed to issue of a most brilliant purple, and formed, as it were, a conducting chain of light of nearly an inch in length, at the same time that elastic matter was rapidly formed, some of which was permanent. After many unsuccessful trials, I at length succeeded in collecting the quantity of elastic fluid given out by half a grain of charcoal; the process had been continued nearly half an hour. The quantity of gas amounted to nearly an eighth of a cubical inch; it was inflammable by the electric spark with oxigen gas, and four measures of it absorbed three measures of oxigen, and produced one measure and a half of carbonic acid. The charcoal in this experiment had become harder at the point, and its lustre, where it had been heated to whiteness, approached to that of plumbago.

A purple flame formed, and elastic matter evolved.

I heated two grains of potassium together with two grains of

Charcoal heat-

ed with potassium.

of charcoal, for five minutes; and to estimate the effects of the metallic oxides and potash in the green glass tube, I made a comparative experiment, as in the case of plumbago; but there was no proof of any oxigen being furnished to the potassium from the charcoal in the process, for the compound acted upon water with great energy, and produced a quantity of inflammable gas, only inferior by one twelfth to that produced by the potassium, which had not been combined with charcoal, and which gave the same diminution by detonation with oxigen; and the slight difference may be well ascribed to the influence of foreign matters in the charcoal. There was no ignition in the process, and no gas was evolved.

Compound produced.

The compound produced in other experiments of this kind was examined. It is a conductor of electricity, is of a dense black, inflames spontaneously, and burns with a deep red light in the atmosphere*.

Diamond could not be acted on by the pile.

The nonconducting nature of the diamond, and its infusibility, rendered it impossible to act upon it by voltaic electricity; and the only new agents which seemed to offer any means of decomposing it, were the metals of the alkalis.

Heated with potassium,

When a diamond is heated in a green glass tube with potassium, there is no elastic fluid given out, and no intensity of action; but the diamond soon blackens, and scales seem to detach themselves from it, and these scales, when examined in the magnifier, are gray externally, and of the colour of plumbago internally, as if they consisted of plumbago covered by the gray oxide of potassium.

in hidrogen gas.

In heating together three grains of diamonds in powder, and two grains of potassium, for an hour, in a small retort of plate glass filled with hidrogen, and making the comparative trial with two grains of potassium heated in a similar apparatus, without any diamonds, I found, that the potassium which had been heated with the diamonds produced, by its action upon water, one cubical inch and $\frac{1}{10}$ of in-

* In the Bakerian Lecture for 1807, I have mentioned the decomposition of carbonic acid by potassium, which takes place with inflammation. If the potassium is in excess in this experiment, the same pyrophorus as that described above is formed.

flammable

flammable air, and that which had been exposed to heat alone, all other circumstances being similar, evolved nearly one cubical inch and $\frac{7}{10}$, both of which were pure hidrogen.

In another experiment of a similar kind, in which fragments of diamonds were used in the quantity of four grains, the potassium became extremely black from its action upon them during an exposure to heat for three hours, and the diamonds were covered with a grayish crust, and when acted upon by water and dried, were found to have lost about $\frac{28}{100}$ of a grain in weight. The matter separated by washing, and examined, appeared as a fine powder of a dense black colour. When a surface of platina wire was covered with it, and made to touch another wire in the Voltaic circuit, a brilliant spark with combustion occurred. It burnt, when heated to redness in a green glass tube filled with oxigen gas, and produced carbonic acid by its combustion. A similar experiment.

These general results seem to show, that in plumbago the carbonaceous element exists merely in combination with iron, and in a form which may be regarded as approaching to that of a metal in its nature, being conducting in a high degree, opaque, and possessing considerable lustre. Plumbago.

Charcoal appears to contain a minute quantity of hidrogen in combination. Possibly likewise, the alkalis and earths produced during its combustion exist in it not fully combined with oxigen; and according to these ideas, it is a very compounded substance, though in the main it consists of the pure carbonaceous element. Charcoal.

The experiments on the diamond render it extremely likely, that it contains oxigen; but the quantity must be exceedingly minute, though probably sufficient to render the compound nonconducting: and if the carbonaceous element in charcoal and the diamond be considered as united to still less foreign matter in quantity, than in plumbago, which contains about $\frac{1}{12}$ of iron, the results of their combustion, as examined independently of hygrometrical tests, will not differ perceptibly. Diamond.

Whoever considers the difference between iron and steel, in which there does not exist more than $\frac{1}{100}$ of plumbago, or the difference between the amalgam of ammonium and mercury, Minute differences in composition may greatly alter

over a spirit lamp; and a proper connection being made with the voltaic apparatus of one hundred plates of six inches, in great activity, a contact was made in the sulphur by means of another platina wire. A most brilliant spark, which appeared orange coloured through the sulphur, was produced, and a minute portion of elastic fluid rose to the upper extremity of the tube. By a continuation of the process for nearly an hour, a globule equal to about the tenth of an inch in diameter was obtained, which, when examined, was found to be sulphuretted hidrogen.

Sulphuretted hidrogen produced.

But the sulphur might have contained water.

This result perfectly coincided with those which have been just mentioned; but as the sulphur that I had used was merely in its common state, and as the ingenious experiments of Dr. Thompson have shown, that sulphur in certain forms may contain water, I did not venture, at that time, to form any conclusion upon the subject.

The experiment repeated with pure sulphur.

In the summer of the present year, I repeated the experiment with every precaution. The sulphur that I employed was Sicilian sulphur, that had been recently sublimed in a retort filled with nitrogen gas, and that had been kept hot till the moment that it was used. The power applied was that of the battery of five hundred double plates of six inches, highly charged. In this case the action was most intense, the heat strong, and the light extremely brilliant; the sulphur soon entered into ebullition, elastic matter was formed in great quantities, much of which was permanent; and the sulphur, from being of a pure yellow, became of a deep red brown tint.

Sulphuretted hidrogen produced, and part of the sulphur acidified? Large quantity evolved.

The gas, as in the former instance, proved to be sulphuretted hidrogen. The platina wires were considerably acted upon; the sulphur, at its point of contact with them, had obtained the power of reddening moistened limtus paper.

I endeavoured to ascertain the quantity of sulphuretted hidrogen evolved in this way from a given quantity of sulphur, and for this purpose, I electrized a quantity equal to about two hundred grains in an apparatus of the kind I have just described, and when the upper part of the tube was full of gas, I suffered it to pass into the atmosphere; so as to enable me to repeat the process.

When

When I operated in this way, there seemed to be no limit to the generation of elastic fluid, and in about two hours a quantity had been evolved, which amounted to more than five times the volume of the sulphur employed. From the circumstances of the experiment, the last portion only could be examined, and this proved to be sulphuretted hidrogen. Towards the end of the process, the sulphur became extremely difficult of fusion, and almost opaque, and when cooled and broken, was found of a dirty brown colour.

Sulphur and potassium evolve sulphuretted hidrogen.

The experiments upon the union of sulphur and potassium, which I laid before the Society last year, prove that these bodies act upon each other with great energy, and that sulphuretted hidrogen is evolved in the process, with intense heat and light.

Potassium heated with compound inflammables.

In heating potassium in contact with compound inflammable substances, such as resin, wax, camphor, and fixed oils, in close vessels out of the contact of the air, I found, that a violent inflammation was occasioned; that hidrocarbonate was evolved; and that when the compound was not in great excess, a substance was formed, spontaneously inflammable at common temperatures, the combustible materials of which were charcoal and potassium.

Pyrophorus.

Analogies.

Here was a strong analogy between the action of these bodies and sulphur on potassium. Their physical properties likewise resemble those of sulphur; for they agree in being nonconductors, whether fluid or solid, in being transparent when fluid, and semitransparent when solid, and highly refractive; their affections by electricity are likewise similar to those of sulphur; for the oily bodies give out hidrocarbonate by the agency of the voltaic spark, and become brown, as if from the deposition of carbonaceous matter.

Hidrogen certainly exists in sulphur.

But the resinous and oily substances are compounds of a small quantity of hidrogen and oxigen, with a large quantity of a carbonaceous basis. The existence of hidrogen in sulphur is fully proved, and we have no right to consider a substance, which can be produced from it in such large quantities, merely as an accidental ingredient.

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Reddening of the litmus might be by sulphuretted hidrogen.

The reddening of the litmus paper, by sulphur that had been acted on by voltaic electricity, might be ascribed to its containing some of the sulphuretted hidrogen formed in the process; but even the production of this gas, as will be immediately seen, is an evidence of the existence of oxigen in sulphur.

Potassium heated in sulphuretted hidrogen.

In my early experiments on potassium, procured by electricity, I heated small globules of potassium in large quantities of sulphuretted hidrogen, and I found that sulphuret of potash was formed; but this might be owing to the water dissolved in the gas, and I ventured to draw no conclusion till I had tried the experiment in an unobjectionable manner.

Perfectly dried,

I heated four grains of potassium in a retort of the capacity of twenty cubical inches; it had been filled after the usual processes of exhaustion with sulphuretted hidrogen, dried by means of muriate of lime that had been heated to whiteness; as soon as the potassium fused, white fumes were copiously emitted, and the potassium soon took fire, and burnt with a most brilliant flame, yellow in the centre and red towards the circumference *.

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In another experiment, in which eight grains of potassium were heated in a retort of the capacity of twenty cubical inches, containing about nineteen cubical inches of sulphuretted hidrogen, and a cubical inch of phosphuretted hidrogen, which was introduced for the purpose of absorbing the oxigen of the small quantity of common air admitted by the stop-cock, the inflammation took place as before; there was a similar precipitation of sulphur on the sides of the retort; the mass formed in the place of the potassium was orange externally, and of a dark gray colour internally, as in the last instance; and when acted on by a little water holding muriatic acid in solution, there were evolved from it five cubical inches only of sulphuretted hidrogen.

Experiment repeated.

Both these experiments concur in proving the existence of a principle in sulphuretted hidrogen, capable of destroying partially the inflammability of potassium, and of producing upon it all the effects of oxigen; for had the potassium combined merely with pure combustible matter, it ought, as will be seen distinctly from what follows, to have evolved by the action of the acid a volume of sulphuretted

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Sulphur heated in hidrogen.

Sulphuretted hidrogen, as has been long known to chemists, may be formed by heating sulphur strongly in hidrogen gas. I heated four grains of sulphur in a glass retort, containing about twenty cubical inches of hidrogen, by means of a spirit lamp, and pushed the heat nearly to redness. There was no perceptible change of volume in the gas after the process; the sulphur that had sublimed was unaltered in its properties, and about three cubical inches of an elastic fluid absorbable by water were formed: the solution reddened litmus, and had all the properties of a solution of pure sulphuretted hidrogen. Now if we suppose sulphuretted hidrogen to be constituted by sulphur dissolved in its unaltered state in hidrogen, and allow the existence of oxigen in this gas; its existence must likewise be allowed in sulphur, for we have no right to assume, that sulphur in sulphuretted hidrogen is combined with more oxigen than in its common form: it is well known, that, when electrical sparks are passed through sulphuretted hidrogen, a considerable portion of sulphur is separated, without any alteration in the volume of the gas. This experiment I have made more than once, and I found that the sulphur obtained, in fusibility, combustibility, and other sensible properties, did not perceptibly differ from common sublimed sulphur.

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According to these ideas, the intense ignition produced by the action of sulphur, on potassium and sodium, must not be ascribed merely to the affinity of the metal of the alkalis for its basis, but may be attributed likewise to the agency of the oxigen that it contains.

The minute examination of the circumstances of the action of potassium and sulphur likewise confirms these opinions.

Farther confirmed.

When two grains of potassium and one of sulphur were heated gently in a green glass tube filled with hidrogen, and connected with a pneumatic apparatus, there was a most intense ignition produced by the action of the two bodies, and one eighth of a cubical inch of gas was disengaged

engaged, which was sulphuretted hidrogen. The compound was exposed in a mercurial apparatus to the action of liquid muriatic acid; when a cubical inch and a quarter of aeriform matter was produced, which proved to be pure sulphuretted hidrogen.

The same experiment was repeated, except that four grains of sulphur were employed instead of one. In this case, a quarter of a cubical inch of gas was disengaged during the process of combination; and when the compound was acted upon by muriatic acid, only three quarters of a cubical inch of sulphuretted hidrogen were obtained.

Now, *sulphuret* of potash produces sulphuretted hidrogen by the action of an acid; and if the sulphur had not contained oxigen, the hidrogen evolved by the action of the potassium in both these experiments ought to have equalled at least two cubical inches, and the whole quantity of sulphuretted hidrogen ought to have been more: and that so much less sulphuretted hidrogen was evolved in the second experiment, can only be ascribed to the larger quantity of oxigen furnished to the potassium by the larger quantity of the sulphur.

Several experiments made with similar results.

I have made several experiments of this kind with similar results. Whenever equal quantities of potassium were combined with unequal quantities of sulphur, and exposed afterward to the action of muriatic acid, the largest quantity of sulphuretted hidrogen was furnished by the product containing the smallest proportion of sulphur; and in no case was the quantity of gas equal in volume to the quantity of hidrogen, which would have been produced by the mere action of potassium upon water.

Composition of sulphur.

From the general tenour of these various facts, it will not be, I trust, unreasonable to assume, that sulphur, in its common state, is a compound of small quantities of oxigen and hidrogen with a large quantity of a basis, that produces the acids of sulphur in combustion, and which, on account of its strong attractions for other bodies, it will probably be very difficult to obtain in its pure form.

In metallic combinations even, it still probably retains its oxigen and part of its hidrogen. Metallic sulphurets can only be partially decomposed by heat, and the small quantity

quantity of sulphur evolved from them in this case when perfectly dry and out of the contact of air, as I found in an experiment on the sulphuret of copper and iron, exists in its common state, and acts upon potassium, and is affected by electricity, in the same manner as native sulphur.

4. *Analytical Experiments on Phosphorus.*

Phospohrus analogous to sulphur.

The same analogies apply to phosphorus as to sulphur, and I have made a similar series of experiments on this inflammable substance.

Acted on by the pile,

Common electrical sparks, passed through phosphorus, did not evolve from it any permanent gas; but when it was acted upon by the voltaic electricity of the battery of five hundred plates in the same manner as sulphur, gas was produced in considerable quantities, and the phosphorus became of a deep red brown colour, like phosphorus that has been inflamed and extinguished under water. The gas examined proved to be phosphuretted hidrogen, and in one experiment, continued for some hours, a quantity estimated to be nearly equal to four times the volume of the phosphorus employed was given off. The light of the voltaic spark in the phosphorus was at first a brilliant yellow, but as the colour of the phosphorus changed, it appeared orange.

evolved phosphuretted hidrogen.

Potassium heated in phosphuretted hidrogen.

I heated three grains of potassium in sixteen cubical inches of phosphuretted hidrogen; as soon as it was fused, the retort became filled with white fumes, and a reddish substance precipitated upon the sides and upper part of it. The heat was applied for some minutes. No inflammation took place*. When the retort was cool, the absorption was found to be less than a cubical inch. The potassium externally was of a deep brown colour, internally it was of a dull lead colour. The residual gas had lost its property of spontaneous inflammation, but seemed still to contain a small quantity of phosphorus in solution.

* It is stated, in the account before referred to of M. M. Gay-Lussac and Thenard's experiments, that potassium inflames in phosphuretted hidrogen. My experiments upon this gas have been often repeated. I have never perceived any luminous appearance; but I have always operated in daylight.

The phosphuret acted upon over mercury by solution of muriatic acid evolved only one cubical inch and three quarters of phosphuretted hidrogen.

From this experiment there is great reason to suppose, that phosphuretted hidrogen contains a minute proportion of oxigen, and consequently that phosphorus likewise may contain it; but the action of potassium on phosphorus itself furnishes perhaps more direct evidences of the circumstance. Phosphuretted hidrogen contains oxigen.

One grain of potassium and one grain of phosphorus were fused together in a proper apparatus. They combined with the production of the most vivid light and intense ignition. During the process one tenth of a cubical inch of phosphuretted hidrogen was evolved. The phosphuret formed, exposed to the action of diluted muriatic acid over mercury, produced exactly three tenths of a cubical inch of phosphuretted hidrogen. Phosphorus fused with potassium.

In a second experiment, one grain of potassium was fused with three grains of phosphorus; in this case nearly a quarter of a cubical inch of phosphuretted hidrogen was generated during the ignition. But from the compound exposed to muriatic acid, only one tenth of a cubical inch could be procured. Experiment repeated.

Now it is not easy to refer the deficiency of phosphuretted hidrogen in the second case to any other cause, than to the supply of oxigen to the potassium from the phosphorus: and the quantity of phosphuretted hidrogen evolved in the first case is much less than could be expected, if both potassium and phosphorus consisted merely of pure combustible matter. Phosphorus contains oxigen.

The phosphoric acid, formed by the combustion of phosphorus, though a crystalline solid, may still contain water. The hidrogen evolved from phosphorus by electricity proves indeed, that this must be the case; and though the quantity of hidrogen and oxigen in phosphorus may be exceedingly small, yet they may be sufficient to give it peculiar characters; and till the basis is obtained free, we shall have no knowledge of the properties of the pure phosphoric element. Phosphoric acid may contain water.

5. On

5. *On the States of the carbonaceous Principle in Plumbago, Charcoal, and the Diamond.*

Plumbago, charcoal, and diamond,

The accurate researches of Messrs. Allen and Pepys have distinctly proved, that plumbago, charcoal, and the diamond produce very nearly the same quantities of carbonic acid, and absorb very nearly the same quantities of oxigen in combustion.

consist principally of the same element,

Hence it is evident, that they must consist principally of the same kind of elementary matter; but minute researches upon their chemical relations, when examined by new analytical methods, will, I am inclined to believe, show, that the great difference in their physical properties does not merely depend upon the differences of the mechanical arrangement of their parts, but likewise upon differences in their intimate chemical nature.

but with chemical differencies.

Plumbago acted upon by the pile in vacuo.

I endeavoured to discover, whether any elastic matter could be obtained from plumbago very intensely ignited by the Voltaic battery in a Torricellian vacuum: but though the highest power of the battery of five hundred was employed, and though the heat was such, as in another experiment instantly melted platina wire of $\frac{1}{70}$th of an inch in diameter, yet no appearance of change took place upon the plumbago. Its characters remained wholly unaltered, and no permanent elastic fluid was formed.

Heated with potassium in hidrogen gas.

I heated one grain of plumbago, with twice its weight of potassium, in a plate glass tube connected with a proper apparatus, and I heated an equal quantity of potassium alone in a tube of the same kind, for an equal length of time, namely, eight minutes. Both tubes were filled with hidrogen: no gas was evolved in either case. There was no ignition in the tube containing the plumbago, but it seemed gradually to combine with the potassium. The two results were exposed to the action of water; the result from the plumbago acted upon that fluid with as much energy as the other result, and the two volumes of elastic fluids were 1·8 cubical inch and 1·9 cubical inch; and both gave the same diminution by detonation with oxigen, as pure hidrogen. Two grains of potassium, by acting upon water, would have produced two cubical inches and one eighth

of

of hidrogen gas; the deficiency in the result, in which potassium alone was used, must be ascribed to the loss of a small quantity of metal, which must have been carried off in solution in the hidrogen, and perhaps, likewise, to the action of the minute quantity of metallic oxides in the plate glass. The difference in the quantity of hidrogen given off in the two results is however too slight, to ascribe it to the existence of oxigen in the plumbago.

The experiment repeated.

I repeated this experiment several times with like results, and in two or three instances examined the compound formed. It was infusible at a red heat, had the lustre of plumbago. It inflamed spontaneously, when exposed to air, generated potash, and left a black powdery residuum. It effervesced most violently in water, and produced a gas, which burnt like pure hidrogen.

Charcoal acted upon by the pile in vacuo.

When small pieces of charcoal from the willow, that had been intensely ignited, were acted upon by Voltaic electricity in a Torricellian vacuum, every precaution being taken to exclude moisture from the mercury and the charcoal, the results were very different from those occurring in the case of plumbago.

A purple flame formed, and elastic matter evolved.

When plumbago was used, after the first spark, which generally passed through a distance of about one eighth of an inch, there was no continuation of light, without a contact or an approach to the same distance; but from the charcoal a flame seemed to issue of a most brilliant purple, and formed, as it were, a conducting chain of light of nearly an inch in length, at the same time that elastic matter was rapidly formed, some of which was permanent. After many unsuccessful trials, I at length succeeded in collecting the quantity of elastic fluid given out by half a grain of charcoal; the process had been continued nearly half an hour. The quantity of gas amounted to nearly an eighth of a cubical inch; it was inflammable by the electric spark with oxigen gas, and four measures of it absorbed three measures of oxigen, and produced one measure and a half of carbonic acid. The charcoal in this experiment had become harder at the point, and its lustre, where it had been heated to whiteness, approached to that of plumbago.

Charcoal heat-

I heated two grains of potassium together with two grains of

ed with potassium. of charcoal, for five minutes; and to estimate the effe the metallic oxides and potash in the green glass tu made a comparative experiment, as in the case of plum but there was no proof of any oxigen being furnisl the potassium from the charcoal in the process, fo compound acted upon water with great energy, and pro a quantity of inflammable gas, only inferior by one t to that produced by the potassium, which had not combined with charcoal, and which gave the same di tion by detonation with oxigen; and the slight diff may be well ascribed to the influence of foreign matt the charcoal. There was no ignition in the process no gas was evolved.

Compound produced. The compound produced in other experiments o kind was examined. It is a conductor of electricity, a dense black, inflames spontaneously, and burns v deep red light in the atmosphere*.

Diamond could not be acted on by the pile. The nonconducting nature of the diamond, and its i bility, rendered it impossible to act upon it by v electricity; and the only new agents which seemed to any means of decomposing it, were the metals o alkalis.

Heated with potassium, When a diamond is heated in a green glass tube wi tassium, there is no elastic fluid given out, and no int of action; but the diamond soon blackens, and seem to detach themselves from it, and these scales, examined in the magnifier, are gray externally, and c colour of plumbago internally, as if they consist plumbago covered by the gray oxide of potassium.

In hidrogen gas. In heating together three grains of diamonds in po and two grains of potassium, for an hour, in a small of plate glass filled with hidrogen, and making the co rative trial with two grains of potassium heated in a s apparatus, without any diamonds, I found, that the assium which had been heated with the diamonds proc by its action upon water, one cubical inch and $\frac{3}{10}$

* In the Bakerian Lecture for 1807, I have mentioned t composition of carbonic acid by potassium, which takes plac inflammation. If the potassium is in excess in this experime same pyrophorus as that described above is formed.

flam

flammable air, and that which had been exposed to heat alone, all other circumstances being similar, evolved nearly one cubical inch and $\frac{7}{10}$, both of which were pure hidrogen.

In another experiment of a similar kind, in which fragments of diamonds were used in the quantity of four grains, the potassium became extremely black from its action upon them during an exposure to heat for three hours, and the diamonds were covered with a grayish crust, and when acted upon by water and dried, were found to have lost about $\frac{28}{100}$ of a grain in weight. The matter separated by washing, and examined, appeared as a fine powder of a dense black colour. When a surface of platina wire was covered with it, and made to touch another wire in the Voltaic circuit, a brilliant spark with combustion occurred. It burnt, when heated to redness in a green glass tube filled with oxigen gas, and produced carbonic acid by its combustion. A similar experiment.

These general results seem to show, that in plumbago the carbonaceous element exists merely in combination with iron, and in a form which may be regarded as approaching to that of a metal in its nature, being conducting in a high degree, opaque, and possessing considerable lustre. Plumbago.

Charcoal appears to contain a minute quantity of hidrogen in combination. Possibly likewise, the alkalis and earths produced during its combustion exist in it not fully combined with oxigen; and according to these ideas, it is a very compounded substance, though in the main it consists of the pure carbonaceous element. Charcoal.

The experiments on the diamond render it extremely likely, that it contains oxigen; but the quantity must be exceedingly minute, though probably sufficient to render the compound nonconducting: and if the carbonaceous element in charcoal and the diamond be considered as united to still less foreign matter in quantity, than in plumbago, which contains about $\frac{1}{10}$ of iron, the results of their combustion, as examined independently of hygrometrical tests, will not differ perceptibly. Diamond.

Whoever considers the difference between iron and steel, in which there does not exist more than $\frac{1}{100}$ of plumbago, or the difference between the amalgam of ammonium and mercury, Minute differences in composition may greatly alter

external appearance.

mercury, in which the quantity of new matter is not moi than $\frac{1}{1000}$, or that between the metals and their suboxide some of which contain less than $\frac{1}{10}$ of oxigen, will not l disposed to question the principle, that minute differenc in chemical composition may produce great differences i external and physical characters.

(To be continued in our next.)

II.

On the Stem of Trees; with an Attempt to discover th Cause of Motion in Plants. By MRS. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Method of dividing the stem of trees.

THE manner in which Linnæus divided the stem of tre was naturally suggested by its appearance to the eye, *litt aided* by glasses: cortex, the rind; liber, the bark; lignun the wood; and medulla, the pith. But at this time, th our magnifiers are so perfected; nature points out a moi regular division, and one marked not only by the forn but by the difference of the *juices*, with which the parts ar swelled. Indeed so different are the purposes to be effecte and so clear are the divisions nature has made; that, whe seen much magnified, *they appear to me* directly to strik the mind, and convince the reason; provided the study i pursued in a manner, that will enable the person, by view of the different parts properly prepared, to judg sanely on the subject. The vegetable cuttings sold with th solar microscope will do very well for superficial learner but no person can understand the nature of plants, or e pect to profit from knowledge so obtained, who does n cut his own specimens, and generally from fresh plant It is laborious and troublesome, and requires great car but I have never a moment repented the time so expende as from dried cuttings much of the real nature *and all th motion* escape. Still both are to be consulted; and th proper method is perhaps to compare them together. copy from no book, every experiment has been made b myself, and carefully repeated a number of times: I ma

To judge from both dried and fresh cuttings.

perhap

perhaps be accused of presumption, in *venturing to introduce* so *many new ideas;* and depending thus *on myself* only; but I recount merely what I have seen in a very *good solar microscope*; if my deductions are false, I detail my reasons; and every reader may judge for himself. It is to the *great magnifying powers* I am indebted; and every one (with the same instrument) may prove the truth of what I advance.

I shall divide the stem of trees into 6 parts; 1st the rind; 2nd the bark and inner bark; 3d the wood; 4th the spiral nerves; 5th the *nerves* or *circle of life;* 6th the pith. Division of the stem.

The rind is I conceive merely an outward covering to the tree, to preserve its moisture, that the sun may not evaporate its juices. It is true, that the same is continued under ground; but it may be as useful there to prevent the entrance of the dust and earth, and pressure of stones, or the injury of insects. It is composed of rows of cylinders with a single line to divide them, and they are filled with a clear and pellucid liquor. There are seldom more than four or five layers of vessels; but it is in general so covered with parasite plants, and powdered lichens, that its thickness is often more than doubled; and it is not fit for examination, till divested of all extraneous matter. It is the rind thickened that forms much of the armature of plants. It appears by no means necessary to plants, as there are a number in which the bark serves as a covering instead of a rind; it is not therefore *essential* to them. Though to trees it must be so reckoned. Division of the rind.

2d The bark and inner bark, though certainly very different as to *form*, are the same in juice; and being so nearly allied, I shall treat them as one. They are truly of the first consequence in the tree. They are the origin of the leaves; the lengthened vessels of the bark and inner bark, forming the *interlacing* vessels of the leaf, while the juice concentrated and thickened produces the pabulum of the leaf, as I endeavoured to show in my last paper. The juice of the bark is I conceive the blood of the tree. It is here alone are produced the gums, the resins, the oil, the milk, in short all that truly belongs to the tree; gives taste to it; all I conceive that makes one plant different from another; Divisions of the bark.

another; and its virtues, if I may so express myself. The bark is *generally* green, the inner bark white, yellow, or green. The former consists of vessels crossing each other; the latter of bundles of vessels of two sizes, the large ones being formed in a very peculiar manner. They consist of broad cylinders, having a bottom with a hole in it, through which the liquid passes, though not with perfect ease. On exposing several pieces of the inner bark to the solar microscope, the moment I turned the light on the specimen, the juice of which had before proceeded up the pipes rather slowly, it was suddenly propelled forward with a force truly astonishing. When I increased the heat and light by pointing the full focus of the rays on the vessels, the power of the heat was too strong, and broke through the side divisions, inundating the specimen: but when I merely kept up a proper degree of light and heat, it was curious to observe the liquid pass from pipe to pipe, in one regular and easy flow; making a little stop as it issued through the straitened apertures at the bottom of the vessels. I have often stood more than an hour watching the current, (which passes however much slower than the sap does) nor could I perceive, that it required (while the heat and light were on it) any additional expedient to hasten it; but in the night, when both are wanting, the pressure Mr. Knight mentions from the bastard grain is (I should suppose) very likely to assist or quicken its flow; and as at night it is pressed against the cylinders, it is at this time (I should conceive) it would have its effect. This part is however formed in the wood only; but the contraction at the bottom of the large vessels of the inner bark, it is probable may serve the same purpose, that of forcing the liquid forward, by lessening the apertures, and giving therefore more impetus to the current. The vessels are also of great thickness in proportion to their size; and have on them a peculiar circular thing resembling a cullender full of very diminutive holes, so small that no liquid could pass them; but in viewing the thick juice, that runs through these pipes, I observed many bubbles of air, which, as the heat increased or diminished *their size*, *accelerated* or *retarded* the flow of the liquid. Might not these apertures be designed for the entrance of air

Extraordinary flow of the liquid.

Curious formation of the vessels in the bark.

Bubbles of air in the liquid.

air to promote this purpose? The thickness of these vessels is such, as almost to conceal the darkness of the liquid that runs through them. To see their forms well, it is sometimes necessary to clear out their contents, which is best done by placing the specimens in a basket fastened down in a running stream, or boiling them thoroughly, and then throwing them into green wax perfectly melted. When this succeeds, it makes excellent specimens for the cabinet.

Though half fearful to give an opinion absolutely contradictory to one whose abilities I so *much respect as Mirbel's*, yet I must think he is mistaken, when he says: Il y a des plantes qui ont les mêmes sucs dans toutes leur parties." I never could find this; and though the potent smell of the liquid belonging to the bark will often extend to other parts of the plant, it generally vanishes if kept separate for a day, or grows so faint in comparison with the real liquid, as to prove it is not an ingredient. Nor can I understand why he should suppose, that the tubes or cylinders of the inner bark are merely vacancies of the ordinary vessels; for they are exactly the same, and occupy the same place; their peculiar shape and office attend them every where; and there are no vessels like them in any other part of the tree or shrub. I have mentioned only the vessels of the inner bark, because their form is unusual; but the vessels of the bark are more simple and smaller, and divided by a line or two, running longitudinally between them. How the gums, resins, oils, milk, &c. are formed, I am not chymist sufficient to give any clear idea concerning; but the labours of Dr. Thomson seem more to elucidate this subject, than those of any other author I am acquainted with. Nothing can be more admirable than the manner in which he accounts for sugar in plants; it is exemplified each day in those that are out of health. Mirbel has also a very valuable paper on the subject.

Liquids peculiar to each part.

3d I now turn to the wood of the stem. This is marked by nature with such strong lines, it is hardly possible to mistake its parts. Place the stem of any plant in a coloured liquor, and every vessel which conveys the sap from the earth to the top of the tree will be marked and tinged.

Formation and use of the wood vessels.

 The

The sap is the nourishment those vessels convey; it is a thin waterish liquor, which is probably the juices of the earth, *medicated* into this form, as most suitable to the life it is to support. I suppose it is different in each different soil; but though I have often tried "by separating the wood from the rest of the stem, and then macerating it, to draw forth the liquor from the same tree in different soils," I never could perceive there was the change one should naturally expect.

Two different stripes in the wood.

On dissecting the wood; two different kinds of stripes present themselves, some circular, an additional one being each year added, which timber merchants call the silver grain; and another from the circumference to the centre, at least from the first line of the wood to the pith, which they call the bastard grain. The first is the yearly stripe, and I had an opportunity in a large wood that was felled of observing the truth, not only of one stripe being added each year, but that the stripe was large or small, according to the exposure of the tree, and the favourableness of the season. The wood had been planted at two different times, one part 88 years, and the other 56; and each tree was exactly marked according to its age, except *three or four which* gave not the number of stripes specified, and were afterward proved to have been planted instead of others, that had been broken and cut down. In exposed situations the *west side* was much *narrower* in several of the trees; and in the forwarder trees the N. and N. E. was the most *crowded*, I mean, that in measuring the diameter of the wood, it was less on one side of the circumference, than on the other. In several trees there was sometimes only a half circle; and in three different oaks, a rotten part having caused the line of life to leave its situation, part of the pith had followed it, and it had formed two piths, with many rows of wood between. The bastard stripe consists I think of two lines, or strings, with a little scale between them; and they appear from their extreme susceptibility to be formed of the same leatherlike substance as the spiral vessels.

Leatherlike strings of the bastard grain.

Mr. Knight is of opinion, that they are scales only, and he is too exact an observer to be contradicted lightly; but as he mentions their pressing close (which they certainly do) to the

the cylinders at night and in cold weather, they would equally have the effect required; *that* of *supplying by their pressure the want of the sun's rays.* The wood vessels are far more simply made than those of the bark; they are very narrow cylinders; and the last two rows next to the circle of life are sap vessels covered by the spiral ones. The horse chesnut has three or four rows, and they appear to be in quantity according to the size of the leaves. It is indeed difficult to ascertain them exactly even in the solar microscope, as it is in *unwinding* them alone they *can be known*; and their extreme fineness confuses. This has however caused the spiral vessels to be taken for sap vessels. It was a great pleasure to me to find, that neither Mr. Knight nor Mirbel was of this opinion. I believe there can be no doubt, that they are *solid strings*, and hold *no liquid*. When wood is very aged, it grows so compact, that it is difficult without preparation to see the open mouths of the vessels. The wood should then be cut in thin slices, and laid in a very dry place; and it is wonderful how this will stretch the upper end of the cylinders; but fresh cut specimens, *if examined directly*, will almost always be sufficiently visible. *If much magnified*, and cut longitudinally, it is truly wonderful to see the effect of light and heat on the wood vessels; how immediately on turning the light on the glass, the flow of sap is accelerated, and with what perfect ease it runs up vessels so diminutive, that to measure them is almost impossible. Is it not most wonderful to consider the force necessary to carry up this sap, when the vessels are formed of a substance *so thin, so transparent*, that it would appear impossible to confine a liquid within it; and yet that, without being worn out by *friction*, it will bear this force exerted against it, for eighty years together, without showing any signs of decay, a term which many trees will sustain? This indeed proclaims its author, and should make the atheist fall down and worship. A few of the wood vessels are separated, and run with the spiral vessels as nourishing vessels to each leaf, as I have shown in my last; but this diverts but little of the sap from its chief current, which flows on; its last purpose being to form the stamen, and the curious powder that apertains to

Spiral vessels not sap vessels.

All cut with an instrument not a knife.

to it; and afterward to lend its principal aid to the formation of the fruit and seed. For it is this last, that is the grand and finishing work of nature, to which all the rest tends but as a means to the great accomplishment of producing new vegetable lives.

The spiral vessels.

The spiral vessels are a quantity of solid strings coiled up into a spiral form. I cannot but suppose them of a leatherlike substance, and to be found rolled round the last few rows of sap vessels. In this manner they run up the stems of trees and plants of every kind (with a few exceptions) and thence into every leaf and flower. They are singly too small for the naked eye; they run into every fibre of the leaf, and are fastened at the edges, by which means, crossing like a spider's web in every direction through the vessels, they can draw the leaves in any way that is necessary to them. In the larger vessels they are in sets of ten or twelve, but in the smaller only three or four to each vessel. In the cabbage leaf and in the burdock they are in bundles almost as thick as a packthread; but in smaller leaves they are properly propertioned. The more sensitive the leaf, the more they are *coiled up*. These are (I truly believe) the cause.

The cause of motion in leaves and flowers.

The spiral vessels are *(I truly believe) the cause of motion in plants.* I do not mean to say, that there is no motion in plants but what arises from them; but I am fully persuaded, that the *greatest part of the motion in leaves and flowers* proceeds from the management of this spiral wire. I shall now detail my reasons for this persuasion.

Spiral vessels found in no leaves that do not turn.

1*st*. The spiral vessels are not to be found in any plants, to which motion is unnecessary. They are never found in any of the firs, in any of the water plants that spread their leaves on the top of the water, in any of the sea weeds, or in any of the lichens; I think too they are not found in the scolopendrums, or in the lemnas; though at first I took the line of life, that runs into the leaf to form the flower, for one. The grasses also, having no cause for turning their leaves, are wholly without them.

2*d*. If a plant in a window, having all its leaves with their backs turned from the light, is moved, and placed so as to turn them to the sun, they will in a few hours regain their

their former position; reverse it, and it will now want double the time to bring them right; change the order a third time, and though the plant will not in any manner have suffered, yet the leaves will be long regaining their pristine force. Few can move after the third or fourth regression; and why? because the *spiral, like elastic vessels,* were so relaxed by the operation, as to have lost all power of coiling into their usual form.

3d. I have observed that those leaves, that have the most motion, have also the most of these spiral vessels, and have them most twisted. This is particularly seen in the *populus tremula*; the leaf stalk, though small, is full of them, and so hard twisted, that I have known the stalk to measure a *quarter of an inch* difference in length between the middle of the *day* and a cold *evening*. This could arise only from the untwisting of the spiral wire; and few plants have more motion, indeed it has far more than can fairly be attributed to its long leaf stalk.

Most motion in spiral vessels most twisted.

4th. I took a vine leaf, and without separating it from its parent plant, I merely divided the spiral vessels, *without touching the nourishing ones*; it never from that moment either *turned*, or *contracted*; and when placed with its back to the light, it *remained* in this position, though it was long before it decayed. Both electricity and galvanism draw up these leaves, as if they were leather: but it is the spiral fibres, not the *cuticles*; for after I took from a leaf all the spiral wire, the leaf did *not* contract at all. Bonnet was convinced, that all the motion of plants might be given by the means of threads, but microscopes were not so perfect then as to give him the delight of knowing, that he had guessed the operations of nature. He made an artificial leaf and flower, that would move by the contrivance of threads that passed through all the larger vessels, and by this means they effected every movement common to either. But his were *plain threads*, not a spiral wire, the elastic power of which is well known to every person: nor had he an idea, that such vessels existed, but thought it was the contraction and elongation of the upper and under cuticle of the leaf; but this is certainly not the case, as I have proved above, that it has *no* such powers. There are insects

Leaves neither turn nor move if the spiral wires be cut.

Bonnet's contrivance.

Insects contract the spiral wire.

sects in the currant, and many other leaves, that show the power of the elastic wire, *as much as any thing yet mentioned.* Nature has taught them, to draw up these spiral vessels, to make themselves nests, in which to deposit their eggs and young; and any one may see in what manner it is done, and how the leaf is shortened.

Heat contracts the spiral wire.

5th. I took a quantity of these spiral vessels from a cabbage leaf, and placed them on a long netting needle in my solar microscope, that the motion might be extremely visible, and made my assistant hold a candle to the other end of the needle. As the heat approached it, the vessels were agitated inexpressibly, and appeared wreathing like a worm, till with one effort they flung themselves off the needle. The fresh water conferva, and the dodder tribe, are the only plants without leaves, that have the *spiral vessels*, that I am acquainted with. The former is almost formed of it; and the sensitive plants have scarce more motion than the common green conferva. I have seen it draw itself up, then turn with a sudden motion, and surround a pin, coiling up it like a worm; and it will continue to move thus for more than an hour after it is taken from the water. In the leaf stem of the geranium cordifolium the spiral vessels are so very tough, and so very tightly coiled, that I have by great care drawn up the leaf by their means; but this is difficult to be done. Some may imagine, that these spiral wires are too delicate to turn the leaf or flower; but can any one say this, who is in the constant habit of dissecting plants? or who has seen the extreme delicacy of flowers, and yet the force they will exert, or the tenderness of the young shooting plant, and yet the strength with which it will force its way through brick and mortar, and even through solid stones? The works of man are effected by using strong materials, when *powerful ends* are in view; but the works of God are performed in a more wonderful manner, the most delicate means produce the greatest ends. Look on the vegetable cuttings; it is the aggregate of such pieces which forms our ships, and which stands the united attacks of winds and waves. View the metals, as they first grow or shoot into crystallization in the Arbor Dianæ or the leaden tree; who would recognize the destructive bomb, or the hardened

Strength of many apparently weak parts of vegetables.

hardened coin? But the mind that is accustomed to see them in their first delicate forms produce great effects, will not doubt what the Almighty power may fit them for.

In detailing the arguments that tend to prove, that the spiral wire is the cause of motion in plants, I must suggest one, which will at least clear it from all *improbability*. To those to whom the energy, strength, delicacy, and susceptibility of Captain Kater's hygrometer is known, it will offer a certain proof of the possibility of such an *existing power*; since that little instrument is acted upon by the power moisture has of untwisting the awn of a grass brought from India. Now if the most trifling change of moisture can untwist one sort of vegetable fibre, and by this means manage an instrument, why should not a quantity of similarly formed fibres or spiral wires produce the same effect on leaves and flowers? Captain Kater's hygrometer moves very sensibly if a finger is placed within *half an inch of the fibre:* now the most sensitive plant we have will not move but with the *touch:* though I doubt not in its natural soil and climate it is more sensible: but in the sensitive plants there is a peculiarity in the joint, which helps to produce that regularity of movement which is the most curious circumstance in its formation; this I hope to explain in my next. My only doubt is, I confess, whether the power that governs the spiral wire is light, heat, or moisture? I am rather inclined to think it is moisture; though of course light and heat must have very great influence, as no change of either can happen, without its increasing or diminishing the moisture of the atmosphere.

Argument from Kater's hygrometer, that the spiral wire is the motive organ.

I fear I have tired the reader; but I have not produced half the proofs I might bring forward to show, "that if the spiral vessels are *the origin of motion in both leaves and flowers*," flowers may be made to change their position with every variation of light and heat, even *more* than *leaves*; and in the acacia I have made the leaves and flowers droop in the middle of the day, by holding a wet napkin suspended over them after I had completely shaded them; and by carrying flowers into an ice house, they will distinctly prove what part is affected.

Flowers.

The

Circle of life.

The next part is the small circle of vessels situate between the wood and the pith, or rather between the spiral vessels and the pith; which plays so very conspicuous a part in the history of the beginning seed, as I hope to have proved in my first letter; and which I have ventured to call the circle of life. I gave before the strongest proof I could, that a plant cannot exist a day without it; and that, if taken away at a very early age, it will *not* (like every other part) grow again: but when older it will certainly renew itself. It is very curious, that every botanical anatomist has drawn these lines without giving them a name, or otherwise noticing them; they attributed all their powers to the pith, which, from the scanty term of its existence, and its being perpetually impeded in its progress, to make way for the flower bud, can evidently have little power. But it was probably their extreme delicacy that caused them to be overlooked *by all but Hill*, whose admirable treatise on the woods it is quite wonderful should be disregarded. The circle of life consists of rows of little cylinders, that have their own peculiar juice, generally of an austere quality. From this part all branches take their rise, and all wood threads grow. They run up (see Pl. IX, Fig. 10 and 11) into all flower buds, but never approach the leaf bud. When they enter the former, they make their way distinctly to each separate flower, forming the pistil, and after depositing in each seed the *line* which is the first origin of life, they are afterward impregnated, or gain the power of giving life, by the juice of the stamen, which runs through the same string into the seed.

Circle of life overlooked.

Is the life or principal part of the stem.

Is the first part that dies.

That in this part resides the principal *vitality* of the plant, I think I proved in my *former letter;* but I must add, that it is the *first part that dies*, when a branch is cut from a tree, or a tree torn up. In watching the fruit after a sudden frost, if taken *soon enough*, it is *this line* alone, that will appear to be *burnt*. In a few hours after, the rest of the pistil (at least the pointal and style) will be turned a reddish black; but after the first sign it never recovers. But in wood, if this line gets injured (either by the decay of the bud or other means) the circle will undulate into a thousand forms, to regain a wholesome situation in which to pursue its course. I have many curious specimens of decayed wood

wood rotted in this manner, that would explain this subject most evidently, and I have many drawings taken from other specimens, but too large to trouble Mr. Nicholson with; but which I may at a future time make public.

The circle of life found best in decayed wood.

I was once fortunate enough to see a tree cut down, that had been managed according to Mr. Forsyth's excellent method; and procuring some specimens of it, the new wood had begun to form in the middle, where the *pith* should have come, but wood grew instead; and the circle of life, making a large circuit, left a place in the new part *for the pith*. I shall give a sketch from some of my drawings, as it may better explain the nature of the circle of life, which after a certain course returned to the place in the new wood, it would have occupied in the old; as if it did not *venture* on the fresh formed wood, till it was solid and secure. In the rotten wood these vessels may be always traced by their turning *black*, or *darkened*; and in an infant plant (if the seed is boiled for dissection) by their dark colour; though often quite white when alive. I have now before me an Anson's apricot tree, which has the extraordinary property of losing one of its branches every year (I believe it is common to the species). In dissecting it I find near eight inches dead, all but a small piece of the bark and inner bark, which has given *liquid enough* to form a new *flush of leaves*, apparently since the wood has been entirely *dead* (for the wood is totally void of moisture, and must have been without life some time). This shows whence *the leaves proceed*, and that the only nourishment they got was from the *carbonic acid gas they absorbed*. It is true they appeared languishing and ill; still they showed fresh leaves. But it is most curious to see the struggle the circle of life has made to maintain its existence in the injured part, and when I cut it, it was wholly in the bark: but I never found any but delicate fruit trees able to support such stagnations in the wood, it kills our forest trees; or at least the limb that has it; though they have many other complaints, quite as bad as this palsy. I never see a defective limb or branch, without endeavouring to find its cause of decay by dissecting it. The cherry tree is very subject to this complaint, but I know no tree that better shows the line of life, though of the same

Mr. Forsyth's new wood.

Plants can give leaves though near dying.

Circle of life struggles to maintain itself.

same colour as the pith, it is so very clear in its undulations.

Curious growth of the poa reptans.

But of all the plants which prove the powers of the circle of life, none perhaps equal the *grass* called *poa reptans*. It grew in a piece of swampy rubbish ground at the bottom of my garden. I had often measured seven or eight yards in length in the winter, perfectly dead; and yet in June, or the end of May, perceived life beginning to show itself at the farthest end from the stalk. Surprised at this, I the next spring chose two, much alike, dissected one of them *the whole way*, and found a collection of little vessels, which in thickness was not larger than a very *fine thread*. It had got half way the length of the grass, which was about three yards. Having merely opened the cover, I laid it down again, and the little vessels continued growing, till they got to the end of the length of grass. They then made a stop, and I perceived the grass began to thicken; and at the end nearest the roots, the dead part became inflated with juice, lost by degrees its dead appearance, got thickened *about the joints* within; and at last shot forth fresh leaves and *fresh roots*, from every joint. I have since watched it with the greatest care, and find it is the circle of life, that runs *thus*, *protected by the dead scale*. When it is stopped by the cover ceasing, it waits till the season permits the rest to grow.

Dead vegetable matter may be revived.

But it should teach more than this; it will show, that the dead matter may be *inflated* with a *living juice*, and live again, provided the *life* at bottom *is not extinguished*; and I have since seen this in many things, as in the hydrangia, where the stalks apparently die down, and are inflated again, or at least a *part* of them; and I doubt not a gardener must know many instances.

Cause of the double pith.

The extreme delicacy of the circle of life is the cause of the double pith: the parts around it get injured, it starts on this account from its place, and gets farther into the wood; and if it has gone very far, instead of returning the pith begins to form near it, till two complete piths appear with the circle of life surrounding each on one side; or if any wood is formed between they will each complete its circle of life. I could give an innumerable number of additional proofs of the right these vessels have to be called the *circle of life*, or propagation, did I not fear to disgust

disgust and tire my reader; but I may at a future time give the rest.

The pith, which I shall now turn to, I esteem merely as a source of *moisture* to the rest of the plant when wanted: it stops with every flower bud, and begins again to grow as *soon* as the bud is past: it decreases as the strength and size of the tree increases; it is the only part of the tree, that has no *vessels* to contain liquor, for it is a net only, not a bundle of *cylinders*. It has been said, that it is composed of a great variety of figures, but this is a mistake: take it out *extremely thin*, and most piths will be found of one figure only. There are, however, a few different sorts; the net of the dogwood is very curious, and the pith of the juglans, and a few others differ in *form*. The size of the pith will form a tolerable division between the tree and shrub. Pith.

I have but little to say of the root, except that I look upon it to be wholly formed of the rind, much thickened, and perhaps a very little of the bark, but to be *without inner bark*, to have a quantity of wood, *no spiral vessels*, and hardly any pith. I searched in vain for the larger vessels of the inner bark, till it struck me, that the want of it was the reason of there never being a leaf on a root. In Devon this is a trial more easily made than in any other place; and I have repeatedly been assured, that roots were found with leaves, but it always turned out *to be a branch which crossed the root;* and I always found it so, on dissecting it, to try the truth of the assertion. Growth of the root.

I shall now close my letter with endeavouring to prove the truth of an observation made long ago by that *excellent observer* Linnæus, and since so absolutely denied by many: I mean, "that each part of the stem has, when it arrives near the flower stalk, its peculiar juice" for the formation of each part *of the flower;* that the bark produces the calyx of the flower; the inner bark the corolla; the wood the stamen; the circle of life the pistil: and that they all join in forming the fruit and seed. Willdenouw says, that, without having recourse to the *plant*, or to *dissection*, it is at *once possible* to show the folly of supposing, that each *particular* part of the plant should produce only one part of the Each part of the stem has each its particular part of the flower.

Each part of the stem has each its particular part of the flower.

the flower, and he directly adduces the *syngenesian* class, which contains the *very plants*, that (if he had dissected them) would have *proved the mistake* of his *argument*. But as all my opinions are formed on *dissection alone*, I have no theory to carry on, if I deduce from what I see in the microscope a false conclusion. I am very ready on conviction to give up the point; but as I reason from no other data than dissection, I would ask him these simple questions: why, if the nourishment of each part of the stem is not confined to each *different part of the flower*, does the whole arrangement of the stalk *alter*, the moment it gets to the flower stalk? why are there particular vessels, to confine and carry the juice to each peculiar part, if it was not of consequence, that this juice should touch no other places? for what purpose is the curious and artificial management in the bottom and top of a seed vessel, which enables the dissecter to say, "there are five divisions of little vessels proceeding from the wood, I know therefore (though I do not see it) that this must be a *pentandrian flower;* here is but one middle vessel proceeding from the *circle of life* (for the pith stops,) it is therefore of the order *pentandria monogynia:* here are five divisions of little vessels proceeding from the inner bark, it must therefore have *five petals?* This is a simple way of showing the truth, and may disgust, but it is *truth*, and *should not do so;* I ardently wish to convince; because I am convinced myself. Cut above or below the seed vessel of a lily, a violet, a tulip, and *conviction will* I THINK *certainly follow*. Why in cutting below or above the seed vessel of a syngenesian flower, can you directly tell whether it is superflua, æqualis, or segregata? Look at the bottom of the seed vessel of the sonchus; every *pin hole* of the vessel of the male is carried up by corresponding vessels in the outward cuticle of the seed: this I have proved in the solar microscope, (diminutive as it is) it is *thus carried up till* it meets and joins the *ligature* of *the males;* and the female *liquor* is protruded through the *inside* of the seed, and is perhaps one of the *strongest proofs* of the impregnation of the female. In the syngenesian class *(see Plate* IX*)* the delicacy of the vessels, which may be supposed too small for a liquid to flow through, must

must not impede the belief that it does so, when we consider the circulation of blood in the diminutive animal that torments the body of the flea and louse. I have seen the liquor run up with the utmost celerity through the upper cuticle of a very small seed of the syngenesian order, till it met the male and continued its course. It must be understood, that the juice from the corolla flows in the rest of the cuticle, and the largest vessels are those for the male liquor. The production of these little floscules, and the curious arrangement for the vessels, and for the nourishment of each separate part, is so wonderful, that I hardly know an object that has given me more delight than the contrivance manifested in them, or a sight more formed to strike with wonder, when seen in the microscope: and how wholly is this beautiful order of arrangement counteracted by a double flower; that is by finding *none* of these *peculiar* vessels, but a general confusion, that seems to make a mixture of the whole! I never permit myself to form any opinion what the thing is to be, before I have dissected it: my opinions are wholly taken from what I do see, which on this subject I have here given. The person who doubts need only dissect a lily, a violet, or any flower, below the seed vessel or above it, and, I fancy, he will be satisfied.

The line of life enters into leaves that bear the flowers.

In detailing the reasons I had to believe that the circle of life formed the pistil, and that it is the life of the plant, or rather may be better compared to the *spinal marrow and brain* of the animal frame, I forgot one of the strongest proofs, which is, "that, though the circle of life never runs into any other leaf, it is to be found in all those *leaves that have the flower either on the middle* or on the side of the leaf, &c. I first to my great astonishment perceived it in the butcher's broom, where it leads directly up to the flower: then in scolopendrums, and afterward in the xylophyllas, &c. Besides that there is vastly more wood than in any other kind of leaves, as every one will feel on breaking them, the circle of life may easily be traced, as leading from one flower to the other. But I shall detain the reader no longer than to say, that these ideas and discoveries are not the hasty productions of momentary examination, but the result of many years of study; I may

The line of life enters into leaves that bear the flowers.

my intense study: though till now I have not had the courage to lay the result before the public, till I found, that my discoveries were likely to be *superseded* and *published* from the study of others: as I discovered, six years ago, the second organ in plants, which was so well explained in a paper in your excellent Journal, though rather too obscure. I have not mentioned the sensitive plants, because I have not *yet* completed my study of them; but I must observe, they so intirely *confirm* my idea, that "the motion of plants is caused almost wholly by the spiral nerves," that when I lay them before the public, they will I hope eradicate every *remaining doubt* that may be left in the mind: and that they are only more or less sensitive, as the length to which they are fastened is more or less extended.

DEAR SIR,

Your obliged Servant,

A. IBBETSON.

Belleveu, June 12th.

Explanation of the Figures.

Plate VIII, Fig. 7. Divisions of the wood in the stem of trees; *a*, the rind; *b*, the bark: *c*, the inner bark; *d*, the wood; *e*, the spiral nerves; *f*, the line of life; *g*, the pith; *h*, *h*, the silver grain; *o*, *o*, *o*, the bastard grain.

Fig. 8. Cylinders of the inner bark.

Fig. 9. Cylinders of the wood.

Plate IX. Fig. 10. Part of a branch showing the manner in which the line of life, *cc*, enters into the flower bud, *a*, and passes by the leaf buds *bb*; also the manner in which it runs to avoid an injured part.

Fig. 11. A flower bud, showing the line of life, *c c*, running up to each flower, *a*, *a*, *a*, *a*, *a*, *a*, *a*, and the pith terminating at *b*.

Fig. 12. A seed vessel of the class syngenesia; *a*, the calyx, *b*, female florets, *c*, male and female florets.

Fig. 13. Section just above the seed vessel of the dianthus. *a*, the calyx proceeding from the bark; *b*, the corolla, from the inner bark; *c*, *c*, *c*, *c*, *c*, ten stamens from the wood; *d*, the seed vessel; *e*, the pistil from the circle of life.

III. On


Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13
Fig. 14.
Fig. 15.
Fig. 16.
Fig. 17.
Fig. 18.

III.

On the supposed Perspiration of Plants. By Mrs. Agnes Ibbetson.

To Mr. NICHOLSON,

SIR,

A FRIEND has suggested to me, that, to avoid all mistakes, I should have described the various kinds of moisture, that might be taken for the perspiration of plants; lest the subject, *from their appearance*, should be given up as a dream of the author's, without a fair and candid trial. It is certainly worth it, for great must be its influence on the atmosphere, and *immense* the calculation of the water necessary to *afford such a perspiration*, if we take into account also the quantity wanted for *their growth*. But, I may say, if leaves exude, in proportion to their surface, as much moisture as a healthy man, they must often drop water in the driest days; which I never could perceive they did. But if (as is insisted on) they yield 17 times as much as a robust subject, every tree must be a shower bath, and we could not sit under one without a complete wetting *.

Moisture mistaken for perspiration should be described.

Perspiration ascribed to plants enormous.

Of the various appearances of moisture, which the solar microscope so completely elucidates, I shall first mention the honey dew, though there are few not acquainted with its appearance. *Beside this*, there *are three others*, one the bladder in which a small insect infolds its larva; another sort in which an insect lays her eggs; and the third is the sickness of a plant; for there are few plants, that do not give out a sort of sugar when ill. After these I must mention the egg of some insect. It is found on the proteas, and one or two other plants. I have preserved the eggs till the animals showed themselves. The next is the cryptogamia found on the pea, the sun flower leaf, the mimulus, and a few others; of these I have given a sketch, just as I took them from the solar microscope, that every one may judge whether this looks like the perspiration of a plant.

Different kinds of apparent moisture on plants.

* This does not follow, unless some cause were present, to condense the aqueous vapour perspired. C.

plant*. I have also seen the beginning of the hairs of the leaf taken for it.

No perspiration visible with a very high magnifying power.

In the three or four years that I have been (as long as the leaves last) endeavouring to discover perspiration, it appears to me impossible I should not have found it, *if it did exist:* but I have sought it with microscopes that magnified *so extremely*, as to prevent my being deceived by *other objects.* I regret indeed the little use made of an instrument now carried to a degree of perfection, which must daily bring new wonders to our admiring senses. With respect to perspiration, it is so little shown, though the smallest hairs of the leaf are enlarged to the size of a ruler, and the water is seen running up as the rarefaction of the air forces it from the increased warmth of the glass. Nay, the pores of the leaf are so enlarged, that an object five times as small could be seen and examined: why then should I not see moisture, *if it existed?*

Heat increases the cryptogamian plants on leaves, and causes unnatural secretions.

The melon.

The cucumber.

In my former papers (which were written a long time since,) I did not mention (because I was not fully aware of it) the very defective manner made use of to try the quantity of perspiration given by plants, and to evince its existence, till the desire of studying the effect of various degrees of heat on plants, made me a constant attendant on the hothouse, green house, hot walls, and glasses, &c. I then found, that any increase of heat helped greatly to *increase* the *number of cryptogamian* plants on those leaves, on which they were not *at all inclined* to grow; and that, beside this, they produced secretions unknown to the plant in its natural situation. The melon gives a very curious one, found on the edge of the leaf of the plant every morning: but, instead of covering the plant from all air, leave *it a little* by *raising the glass*, and the moisture *intirely ceases.* It is the same, though not so much, with the cucumber. There is not the smallest appearance of moisture without the plant is first rendered ill, to study its secretions. It is objected to me, that I left the plant so long (being three hours) that

* See Plate IX, Figs. 15, and 16, the cryptogamian plant on the mimulus, or monkeyflower: fig. 17, those on the pea, which are recumbent: fig. 18, those on the sunflower. They seldom appear on young leaves, or on any leaf, till the plant is near flowering.

the

the moisture under the glass had evaporated. It might perhaps have given a little more in a shorter time, and the hygrometer would have marked a trifle more moisture; but it is forced from the plant, and, so far from giving it naturally, I have every reason to believe, that it acts *as heat does*, and tears its way through the cuticle, as animals in an air pump will sometimes have the blood forced through the pores of the skin.

It is certain, that a plant cannot exist without air, and that it languishes in a confined air. In this state how impossible to judge of its secretions. I cannot help being persuaded, that excellent botanist Mirbel had many doubts of its existence. The clear and simple account he gives of the production of the gasses and juices of plants *is such*, that *but for one line*, it would be the most perfect thing I ever saw; I hope I may be excused translating the few lines. "It is certain, that the carbonic acid gas, produced and renewed without ceasing by combustion, is dissolved in water, which the atmosphere holds suspended in vapour; and which passes through the thin cuticle of the leaves, and penetrates the albumen, and gains the nourishing vessels. This absorption takes place when the sap and other fluids (at first dilated by the heat of the day,) become condensed by the cold of the night, and fall towards the lower extremities of the tree; *for then* the liquids take *less room*, a sort of *vacuum takes place* in the higher parts; and the vapours flowing around enter the leaves by the pores, as we see water force itself into the pipe of a pump by the help of the piston, that produces a vacuum. But as soon as the sun appears above the horizon, these same fluids, joined to those the roots have pumped up from the earth, drawn by heat, *are carried into the leaves, and escape by the pores*, and it is then that the *water* and *carbonic acid* gas enforced by light are *decomposed*, and the torrent of oxigen flows from the leaves."

We cannot judge of the secretions of a plant in confinement. Mirbel.

Now if the water escapes through the pores, how can it be there to be decomposed by the light, and to give out its oxigen? Setting aside therefore this line, it is the clearest account of vegetation, and the most just, I had ever the pleasure of reading. But certain it is, that, if plants per-

Water cannot be decomposed if it escape through the pores.

spired, they could not give out oxigen. However, though the appearance of perspiration has invariably proved either a cryptogamian plant; the bubbles which hold the perfumed liquor of leaves, and which are to be found in all leaves that are scented; the eggs of insects; the edges of the pores, &c.; I do not deny, that there may be a very trifling degree of insensible perspiration: for I think that sort of scurf, or jelly, found on the leaves, arises from it; but this is trifling, and scarcely worth mentioning.

A trifling perspiration.

The vine perspires from its stalk.

Of the innumerable quantity of plants I have examined, there is but one, that in my opinion *really does perspire*; and that not on the leaf, but the *stalk*. *This is the vine.* When the vine is extremely full of juice, a bubble appears on the stalk, which, magnified, *is not a plant*; but really issues from the vine as the proper juice of it, for I can see no stalk. With the same truth I should have mentioned it, if I had found hundreds; for to attain truth is my aim, and I am really attached to no system whatever. Mine are merely desultory discoveries, *not mine indeed*, but those of the *solar microscope*, to which I transfer all the honour, if there is any. As to the sickness of a plant, any person may perceive, when a plant has been gathered an hour or two, how damp and moist it grows; it is the same when placed under a glass, it droops and grows clammy.

I am, Sir,

Your obliged Servant,

AGNES IBBETSON.

Belleveu 26th June.

IV.

A numerical Table of elective Attractions; with Remarks on the Sequences of double Decompositions. By THOMAS YOUNG, *M.D. For. Sec. R.S.**

Attempts at numerical tables of elective attractions.

ATTEMPTS have been made, by several chemists, to obtain a series of numbers, capable of representing the mutual

* Philos. Trans. for 1809, Part I, p. 148. For a Memoria Technica of the double elective attractions, communicated by the learned author, see Journal, Vol. XXII, p. 304.

tual attractive forces of the component parts of different salts; but these attempts have hitherto been confined within narrow limits, and have indeed been so hastily abandoned, that some very important consequences, which necessarily follow from the general principle of a numerical representation, appear to have been entirely overlooked. It is not impossible, that there may be some cases, in which the presence of a fourth substance, beside the two ingredients of the salt, and the medium in which they are dissolved, may influence the precise force of their mutual attraction, either by affecting the solubility of the salt, or by some other unknown means, so that the number, naturally appropriate to the combination, may no longer correspond to its affections; but there is reason to think, that such cases are rare; and when they occur, they may easily be noticed as exceptions to the general rules. It appears therefore, that nearly all the phenomena of the mutual actions of a hundred different salts may be correctly represented by a hundred numbers, while, in the usual manner of relating every case as a different experiment, above two thousand separate articles would be required.

A series of numbers found, answering very generally.

Having been engaged in the collection of a few of the principal facts relating to chemistry and pharmacy, I was induced to attempt the investigation of a series of these numbers; and I have succeeded, not without some difficulty, in obtaining such as appear to agree sufficiently well with all the cases of double decompositions which are fully established, the exceptions not exceeding twenty, out of about twelve hundred cases enumerated by Fourcroy. The same numbers agree in general with the order of simple elective attractions, as usually laid down by chemical authors; but it was of so much less importance to accommodate them to these, that I have not been very solicitous to avoid a few inconsistencies in this respect; especially as many of the bases of the calculation remain uncertain, and as the common tables of simple elective attractions are certainly imperfect, if they are considered as indicating the order of the independent attractive forces of the substances concerned. Although it cannot be expected, that these numbers should be accurate measures of the forces which they represent, yet they may be

Common tables of simple elective attractions imperfect.

 be

be supposed to be tolerable approximations to such measures; at least if any two of them are nearly in the true proportion, it is probable, that the rest cannot deviate very far from it: thus, if the attractive force of the phosphoric acid for potash is about eight tenths of that of the sulfuric acid for barita, that of the phosphoric acid for barita must be about nine tenths as great; but they are calculated only to agree with a certain number of phenomena, and will probably require many alterations, as well as additions, when all other similar phenomena shall have been accurately investigated.

The facts may be represented independent of hypothesis.

There is, however, a method of representing the facts, which have served as the bases of the determination, independently of any hypothesis, and without being liable to the contingent necessity of any future alteration, in order to make room for the introduction of the affections of other substances; and this method enables us also to compare, upon general principles, a multitude of scattered phenomena, and to reject many which have been mentioned as probable, though doubtful, with the omission of a very few only, which have been stated as ascertained. This arrangement simply depends on the supposition, that the attractive force, which tends to unite any two substances, may always be represented by a certain constant quantity.

There must be a sequence in the simple attractions.

Errours in the common tables.

From this principle it may be inferred, in the first place, that there must be a sequence in the simple elective attractions. For example, there must be an errour in the common tables of elective attractions, in which magnesia stands above ammonia under the sulfuric acid, and below it under the phosphoric, and the phosphoric acid stands above the sulfuric under magnesia, and below it under ammonia: since such an arrangement implies, that the order of the attractive forces is this; phosphate of magnesia, sulfate of magnesia, sulfate of ammonia, phosphate of ammonia, and again phosphate of magnesia; which forms a circle, and not a sequence. We must therefore either place magnesia above ammonia under the phosphoric acid, or the phosphoric acid below the sulfuric under magnesia; or we must abandon the principle of a numerical representation in this particular case.

In

In the second place, there must be an agreement between the simple and double elective attractions. Thus, if the fluoric acid stands above the nitric under barita, and below it under lime, the fluate of barita cannot decompose the nitrate of lime, since the previous attractions of these two salts are respectively greater, than the divellent attractions of the nitrate of barita and the fluate of lime. Probably, therefore, we ought to place the fluoric acid below the nitric under barita; and we may suppose, that, when the fluoric acid has appeared to form a precipitate with the nitrate of barita, there has been some fallacy in the experiment.

The simple and double attractions must agree.

The third proposition is somewhat less obvious, but perhaps of greater utility: there must be a continued sequence in the order of double elective attractions; that is, between any two acids, we may place the different bases in such an order, that any two salts, resulting from their union, shall always decompose each other, unless each acid be united to the base nearest to it: for example, sulfuric acid, barita, potass, soda, ammonia, strontia, magnesia, glycina, alumina, zirconia, lime, phosphoric acid. The sulfate of potass decomposes the phosphate of barita, because the difference of the attractions of barita for the sulfuric and phosphoric acids is greater than the difference of the similar attractions of potass; and in the same manner the difference of the attractions of potass is greater than that of the attractions of soda; consequently the difference of the attractions of barita must be much greater than that of the attractions of soda, and the sulfate of soda must decompose the phosphate of barita: and in the same manner it may be shown, that each base must preserve its relations of priority or posteriority to every other in the series. It is also obvious, that, for similar reasons, the acids may be arranged in a continued sequence between the different bases; and when all the decompositions of a certain number of salts have been investigated, we may form two corresponding tables, one of the sequences of the bases with the acids, and another of those of the acids with the different bases; and if either or both of the tables are imperfect, their deficiencies may often be supplied, and their errours corrected, by a repeated comparison with each other.

A continued sequence in the order of double attractions.

Corection of errours in tables.

In

Tables formed from cases collected by Fourcroy.

In forming tables of this kind from the cases collected by Fourcroy, I have been obliged to reject some facts, which were evidently contradictory to others, and these I have not thought it necessary to mention; a few, which are positively related, and which are only inconsistent with the principle of numerical representation, I have mentioned in notes but many others, which have been stated as merely probable, I have omitted without any notice. In the table of simple elective attractions, I have retained the usual order of the different substances; inserting again in parentheses such of them as require to be transposed, in order to avoid inconsequences in the simple attractions: I have attached to each combination marked with an asterisk the number deduced from the double decomposition, as expressive of its attractive force; and where the number is inconsistent with the corrected order of the simple elective attractions, I have also enclosed it in a parenthesis. Such an apparent inconsistency may perhaps in some cases be unavoidable, as it is possible, that the different proportions of the masses, concerned in the operations of simple and compound decomposition, may sometimes cause a real difference in the comparative magnitude of the attractive forces. Those numbers, to which no asterisk is affixed, are merely inserted by interpolation, and they can only be so far employed for determining the mutual actions of the salts to which they belong, as the results which they indicate would follow from the comparison of any other numbers, intermediate to the nearest of those, which are more correctly determined. I have not been able to obtain a sufficient number of facts relating to the metallic salts, to enable me to comprehend many of them in the tables.

Divisions of attractions.

It has been usual to distinguish the attractions, which produce the double decompositions of salts, into necessary and superfluous attractions; but the distinction is neither very accurate, nor very important: they might be still farther divided accordingly as two, three, or the whole of the four ingredients concerned are capable of simply decomposing the salt in which they are not contained; and if two, accordingly as they are previously united or separate: such divisions would however merely tend to divert the attention from the natural operation of the joint forces concerned.

5 It

It appears to be not improbable, that the attractive force of any two substances might, in many cases, be expressed by the quotient of two numbers appropriate to the substances, or rather by the excess of that quotient above unity; thus the attractive force of many of the acids for the three principal alkalis might probably be correctly represented in this manner; and where the order of attractions is different, perhaps the addition of a second, or of a second and third quotient, derived from a different series of numbers, would afford an accurate determination of the relative force of attraction, which would always be the weaker, as the two substances concerned stood nearer to each other in these orders of numbers; so that, by affixing, to each simple substance, two, three, or at most four numbers only, its attractive powers might be expressed in the shortest and most general manner. Expression of the attractive force of two substances.

I have thought it necessary to make some alterations in the orthography generally adopted by chemists, not from a want of deference to their individual authority, but because it appears to me, that there are certain rules of etymology, which no modern author has a right to set aside. According to the orthography universally established throughout the language, without any material exceptions, our mode of writing Greek words is always borrowed from the Romans, whose alphabet we have adopted: thus the Greek vowel Υ, when alone, is always expressed in Latin and English by Y, and the Greek dipthong ΟΥ by U, the Romans having no such dipthong as OU or OY. The French have sometimes deviated from this rule, and if it were excusable for any, it would be for them, since their *u* and *ou* are pronounced exactly as the Υ and ΟΥ of the Greeks probably were: but we have no such excuse. Thus the French have used the term *acoustique*, which some English authors have converted into "acoustics;" our anatomists, however, speak, much more correctly, of the "acustic" nerve. Instead of glucine, we ought certainly, for a similar reason, to write glycine; or glycina, if the names of the earths are to end in *a*. Barytes, as a single Greek word, means weight, and must be pronounced bárytes; but as the name of a stone, accented on the second syllable, it must be written barites; and the pure earth may properly be called barita. Yttria I have altered to Itra, because no Latin word begins with a Y. Chemical orthography.

Table of the Sequences of the Bases with the different Acids.

In all mixtures of the aqueous solutions of two salts, each acid remains united to the base which stands nearest to it in this table.

SULFURIC ACID.

NITRIC	MURIATIC	PHOSPHORIC	FLUORIC	SULFUROUS	BORACIC	CARBONIC	(NITROUS)	(PHOSPHOROUS)	(ACETIC)
Barita	Barita	Barita	Barita	Barita	Barita	Potass	Barita	Barita	Lead
Strontia	Strontia	Potass	Potass	Potass	Potass	Soda	Strontia	Potass	Mercury
Lime	Lime	Soda	Soda	Soda	Soda	Barita	Lime	Soda	Iron
(Silver ?)	Potass	Ammonia	Strontia	Strontia	Strontia	Strontia	Potass	Ammonia	Potass
(Mercury ?)	Soda	Strontia	Ammonia	Ammonia (4)	Ammonia (5)	Ammonia (6)	Soda	Strontia	Soda
Potass	(Mercury ?)	Magnesia (3)	Magnesia	Magnesia (4)	Magnesia	Lime	Magnesia ?	Magnesia	Magnesia
Soda	(Iron ?)	Glycina	Glycina	Glycina	Lime	Magnesia	Ammonia	Glycina	
Zinc	Magnesia	Alumina	Alumina	Alumina	Glycina	Glycina	Glycina	Alumina	Lead
Iron	Ammonia (2)	Zirconia	Zirconia	Zirconia	Alumina	Alumina	Alumina	Zirconia	Zinc
Copper	Glycina	Lime	Lime	Lime	Zirconia	Zirconia	Zirconia	Lime?	Copper
Magnesia	Alumina (2)								
Ammonia (1)	Zirconia								
Glycina	(Copper ?)								
Alumina									
Zirconia									

(1) Ammonia stands above magnesia when cold. (2) A triple salt is formed. (3) Perhaps magnesia ought to stand lower. (4) A compound salt is formed, and when hot, magnesia stands above ammonia. (5) Fourcroy says, that sulfate of strontia is decomposed by borate of ammonia. (6) With heat, ammonia stands below lime and magnesia.

Nitric Acid.	Nitric and Muriatic Acids.				
Barita	Potass	Barita	Potass	Barita (10)	Potass
Potass	Soda	Potass	Soda	Potass	Soda
Soda	Ammonia	Soda	Ammonia	Soda	Barita (10)
Strontia	Magnesia	Ammonia	Magnesia	Ammonia	Ammonia (7,11)
Lime	Glycina	Magnesia	Glycina	Magnesia	Magnesia (7)
Magnesia (7)	Alumina	Glycina	Alumina	Glycina	Strontia
Ammonia (7)	Zirconia (8)	Alumina	Zirconia	Alumina	Lime
Glycina	Barita	Zirconia	Barita	Zirconia	Glycina
Alumina	Strontia	Strontia (9)	Strontia	Strontia	Alumina
Zirconia	Lime	Lime	Lime	Lime	Zirconia
MURIATIC	PHOSPHORIC	FLUORIC	SULFUROUS	BORACIC	CARBONIC

(7) A triple salt is formed. (8) Fourcroy says, that the muriate of zirconia decomposes the phosphates of barita and strontia. (9) According to Fourcroy's account, the fluate of strontia decomposes the muriates of ammonia, and of all the bases below it; but he says in another part of the same volume, that the fluate of strontia is an unknown salt. (10) According to Fourcroy's account of these combinations, barita should stand immediately below ammonia in both of these columns. (11) With heat, the carbonate of lime decomposes the muriate of ammonia.

Phosphoric Acid.

Barita	Lime	Barita	Potass	Barita
Lime	Barita	Lime	Soda	Lime
Potass	Potass	Potass	Barita	Potass
Soda	Soda	Soda	Lime (13)	Soda
Strontia	Strontia	Strontia	Strontia	Strontia
Magnesia	Magnesia	Ammonia (12)	Ammonia	Magnesia
Ammonia	Ammonia	Magnesia	Magnesia	Glycina ?
Glycina	Glycina	Glycina	Glycina	Alumina
Alumina	Alumina	Alumina	Alumina	Zirconia
Zirconia	Zirconia	Zirconia	Zirconia	
FLUORIC	SULFUROUS	BORACIC	CARBONIC	(PHOSPHOROUS)

(12) According to Fourcroy, the phosphate of ammonia decomposes the borate of magnesia. (13) Fourcroy says, that the carbonate of lime decomposes the phosphates of potash and of soda.

Fluoric Acid.

Lime	Lime	Potass
Potass	Barita	Soda
Soda	Strontia	Lime
Magnesia	Potass	Barita
Ammonia	Soda	Strontia
Glycina	Ammonia	Ammonia (14)
Alumina	Magnesia	Magnesia
Zirconia	Glycina	Glycina
Strontia	Alumina	Alumina
Barita	Zirconia	Zirconia
SULFUROUS	BORACIC	CARBONIC

(14) According to Fourcroy, the carbonate of ammonia decomposes the fluates of barita and strontia.

Sulfurous Acid.

Barita	Potass	Lime	Zirconia
Strontia	Soda	Strontia	Alumina
Potass	Barita (15)	Barita	Glycina
Soda	Strontia	Zirconia	Ammonia
Ammonia	Ammonia	Alumina	Magnesia
Magnesia	Lime	Glycina	Strontia
Lime	Magnesia	Magnesia	Soda
Glycina	Glycina	Ammonia	Potass
Alumina	Alumina	Soda	Barita
Zirconia	Zirconia	Potass	Lime
BORACIC	CARBONIC	(NITROUS)	(PHOSPHOROUS?)

Boracic Acid.

Potass
Soda
Lime
Barita
Strontia
Magnes-
A-

(15 Fourcroy says, that the sulfite of barita decomposes the

Table of the Sequences of the Acids with different Bases.

	BARITA.			STRONTIA.					LIME.				POTASS		MAGNESIA.
													SODA		
Sulfuric	S	C	S	S	C	S	P	S	C	P	P	P	MAGNESIA=AMMONIA	S	B
Nitric	N	S	P	N	SS	P	S	P	P	F	F	F	GLYCINA	N	C
Muriatic	M	P	SS	M	F	SS	SS	SS	F	B	B	SS	ALUMINA	M	P
Phosphoric	SS	SS	N	SS	P	F	F	F	B	SS	C	S	ZIRCONIA	P	F
Sulfurous	P	N	M	C	B	B	B	B	SS	S	SS	B	Each with every subsequent base in this order.	F	SS
Fluoric	C	M	F	B	S	C	C	N	S	C	S	N		SS	S
Boracic	B	F	B	F	M	N	N	M	M	N	N	M		B	N
Carbonic	F	B	C	P	N	M	M	C	N	M	M	C		C	M
STRONTIA	LM	PT	MG	LM	PT	MG	AM	GL	PT	MG	AM	GL			AM
		SD	AM		SD			AL	SD			AL			
			GL					ZR				ZR			
			AL												
			ZR												

Comparative use of this table.

The comparative use of this table may be understood from an example: if we suppose, that the nitrate of barita decomposes the borate of ammonia, we must place the boracic acid above the nitric, between barita and ammonia in this table, and consequently barita below ammonia, between the fluoric and boracic in the former: hence the boracic and fluoric acids must also be transposed between barita and strontia, and between barita and potass; or if we place the fluoric still higher than the boracic in the first instance, we must place barita below ammonia between the nitric and fluoric acids, where indeed it is not impossible that it ought to stand.

Numerical Table of elective Attractions.

BARYTA.		STRONTIA.		POTASS.	SODA.	LIME.	
Sulfuric acid	1000*	Sulfuric acid	903*	Sulfuric acid		Oxalic acid	960
Oxalic	950	Phosphoric	827*	894*	885*	Sulfuric	868*
Succinic	930	Oxalic	825	Nitric 812*	804*	Tartaric	867
Fluoric		Tartaric	757	Muriatic 804*	797*	Succinic	866
Phosphoric	906*	*Fluoric*		Phosphoric		Phosphoric	865*
Mucic	900	Nitric	754*	801*	795*	Mucic	860
Nitric	849*	Muriatic	748*	Suberic? 745	740	Nitric	741*
Muriatic	840*	(Succinic)	740	Fluoric 671*	666*	Muriatic	736*
Suberic	800	(Fluoric)	703*	Oxalic 650	645	Suberic	735
Citric		*Succinic*		Tartaric 616	611	Fluoric	734*
Tartaric	760	Citric?	618	Arsenic 614	609	Arsenic	733½
Arsenic	733½	Lactic	603	Succinic 612	607	*Lactic*	732
(Citric)	730	*Sulfurous*	527*	Citric 610	605	Citric	731
Lactic	729	*Acetic*		Lactic 609	604	Malic	700
(Fluoric)	706*	*Arsenic*	(733½)	Benzoic 608	603	Benzoic	590
Benzoic	597	Boracic	513*	Sulfurous 488*	484*	*Acetic*	
Acetic	594	(Acetic)	480	Acetic 486	482	Boracic	537*
Boracic	(515)*	Nitrous?	430	Mucic 484	480	Sulfurous	516*
Sulfurous	592*	Carbonic	419*	Boracic 482*	479*	(Acetic)	470
Nitrous	450			Nitrous 440	437	Nitrous	425
Carbonic	420*			Carbonic 306*	304*	*Carbonic*	423*
Prussic	400			Prussic 300	298	Prussic	290

MAGNESIA.		AMMONIA.		GLYCINA?		ALUMINA.	ZIRCONIA?
Oxalic acid	820	Sulfuric acid	808*	Sulfuric acid	718*	709*	700*
Phosphoric		Nitric	731*	Nitric	642*	634*	626*
Sulfuric	810*	Muriatic	729*	Muriatic	639*	632*	625*
(Phosphoric)	736*	Phosphoric	728*	Oxalic	600	594	588
Fluoric		Suberic?	720	Arsenic	580	575	570
Arsenic	733	Fluoric	613*	Suberic?	535	530	525
Mucic	732½	Oxalic	611	Fluoric	534*	529*	524*
Succinic	732¼	Tartaric	609	Tartaric	520	515	510
Nitric	732*	Arsenic	607	Succinic	510	505	500
Muriatic	728*	Succinic	605	Mucic	425	420	415
Suberic?	700	Citric	603	Citric	415	410	405
(Fluoric)	620*	Lactic	601	*Phosphoric*	(648)*	(642)*	(636)*
Tartaric	618	Benzoic	599	Lactic	410	405	400
Citric	615	Sulfurous	433	Benzoic	400	395	390
Malic?	600?	Acetic	432	Acetic	395	391	387
Lactic	575	Mucic	431*	Boracic	388*	385*	382*
Benzoic	560	Boracic	430*	Sulfurous	355*	351*	347*
Acetic		Nitrous	400	Nitrous	340	336	332
Boracic	459*	Carbonic	339	Carbonic	325*	323*	321*
Sulfurous	439*	Prussic	270	Prussic	260	258	256
(Acetic)	430						
Nitrous	410						
Carbonic	366*						
Prussic	280						

Acids.

Sulfuric.		Nitric.		Muriatic.		Phosphoric.	
Barita	1000*	Barita	849*	Barita	840*	Barita	906*
Strontia	903*	Potass	812*	Potass	804*	Strontia	827*
Potass	894*	Soda	804*	Soda	797*	*Lime*	(865)*
Soda	885*	Strontia	754*	Strontia	748*	Potass	801*
Lime	868*	Lime	741*	Lime	736*	Soda	795*
Magnesia	810*	Magnesia	732*	Ammonia	729*	*Ammonia*	(728)*
Ammonia	808*	Ammonia	731*	Magnesia	728*	Magnesia	736*
Glycina	718*	Glycina	642*	Glycina	639*	Glycina	648*
Itria	712	Alumina	634*	Alumina	632*	Alumina	642*
Alumina	709*	Zirconia	626*	Zirconia	625*	Zirconia	636*
Zirconia	700*						

Fluoric.		Oxalic.		Tartaric.	Arsenic.		Tungtic.
Lime	734*	Lime	960	867	Lime	733¾	Lime
Barita	706*	Barita	950	760	Barita	733½	Barita
Strontia	703*	Strontia	825	737	Strontia	733¼	Strontia
Magnesia	(620)*	Magnesia	820	618	Magnesia	733	Magnesia
Potass	671*	Potass	650	616	Potass	614	Potass
Soda	660*	Soda	615	611	Soda	609	Soda
Ammonia	613*	Ammonia	611	609	Ammonia	607	Ammonia
Glycina	531*	Glycina?	600	520	Glycina	580	Glycina
Alumina	525*	Alumina	584	515	Alumina	575	Alumina
Zirconia	524*	Zirconia?	588	510	Zirconia	570	Zirconia

Succinic.		Suberic.		Camphoric.	Citric.	
Barita	930	Barita	800	Lime	Lime	731
Lime	866	Potass	745	Potass	Barita	730
Strontia?	740	Soda	740	Soda	Strontia	618
(Magnesia)	732¼	Lime	735	Barita	Magnesia	615
Potass	612	Ammonia	720	Ammonia	Potass	610
Soda	607	Magnesia	700	Glycina?	Soda	605
Ammonia	605	Glycina?	535?	Alumina	Ammonia	603
Magnesia		Alumina	530	Zirconia?	Glycina?	415?
Glycina?	510	Zirconia?	525?	Magnesia	Alumina	410
Alumina	505				Zirconia	405
Zirconia?	500					

Lactic.		Benzoic.		Sulfurous.		Acetic.	
Barita	729	White oxid of arsenic		Barita	592 *	Barita	594
Potass	609			Lime	510 *	Potass	486
Soda	604	Potass	608	Potass	488 *	Soda	482
Strontia	603	Soda	603	Soda	481 *	*Strontia*	480
Lime	(732)	Ammonia	599	*Strontia*	(527) *	*Lime*	470
Ammonia	601	Barita	597	Magnesia	439 *	Ammonia	432
Magnesia	575	Lime	580	Ammonia	433 *	Magnesia	430
Metallic oxids		Magnesia	560	Glycina	355 *	Metallic oxids	
Glycina	410	Glycina?	400?	Alumina	351 *	Glycina	395
Alumina	405	Alumina	395	Zirconia	347 *	Alumina	391
Zirconia	400	Zirconia?	390?			Zirconia	387

Mucic?

Mucic?		Boracic.			Nitrous?		Phosphorous.
Barita	900	Lime	537	*	Barita	450	Lime
Lime	830	Barita	515	*	Potass	440	Barita
Potass	484	Strontia	513	*	Soda	437	Strontia
Soda	480	*Magnesia*	(459)	*	Strontia	430	Potass
Ammonia	431	Potass	482	*	Lime	425	Soda
Glycina	425	Soda	479	*	Magnesia	410	Magnesia?
Alumina	420	Ammonia	430	*	Ammonia	400	Ammonia
Zirconia	415	Glycina	388	*	Glycina	340	Glycina
		Alumina	385	*	Alumina	336	Alumina
		Zirconia	382	*	Zirconia	332	Zirconia

Carbonic.			Prussic.	
Barita	420	*	Barita	400
Strontia	419	*	Strontia	
Lime	(423)	*	Potass	300
Potass?	306	*	Soda	298
Soda	304	*	Lime	290
Magnesia	(306)	*	Magnesia	280
Ammonia	329	*	Ammonia	270
Glycina	325	*	Glycina?	260
Alumina	325	*	Alumina?	258
Zirconia	321	*	Zirconia?	256

V.

Experiments on Sulphur and its Decomposition; by Mr. CURAUDAU, *Professor of Chemistry applicable to the Arts, and Member of several learned Societies**.

WHEN bodies we attempt to decompose have experienced no alteration from the chemical agents, to the action of which they have been subjected, we are obliged to class them as simple bodies. The idea of simple substances, however, though there must be such, is but little reconcilable with the different phenomena of decomposition and recomposition, which nature is incessantly producing before our eyes, and I have never considered as simple all that are generally deemed so. On the contrary I have always thought, that the substances constituting the mineral kingdom, of whatever kind, are compounds; and that the principles of

Bodies supposed simple.

None in the mineral kingdom.

* Journal de Physique, July, 1808, p. 12. Mr. Davy's decomposition of sulphur by the Voltaic pile is given at p. 321, of our present number.

which

which they are composed are the same, as those that enter into the composition of substances, that belong to the vegetable and animal kingdoms. But let me not be mistaken. The state in which we are acquainted with certain principles is very far from the great condensation they must experience, before they enter into the composition of the mineral kingdom. Accordingly the compounds of those that result from a union of these principles must differ, in proportion as they recede from the former state, or approach the latter. This in fact we observe in the vegetable kingdom. The essential oils, for example, must be considered as compounds, in which the principles are very near the gaseous state; while the elements that constitute the resins and fixed oils are in a state of the greatest condensation, with respect to the kingdom to which they belong. But this greatest condensation of the principles, that form the different compounds of the vegetable kingdom, is far removed from the first degree of condensation of the elements that constitute the substances of the mineral kingdom. Accordingly the indestructibility of the latter seems connected with the difficulty of causing principles to retrograde towards a state of less condensation, that have the very opposite tendency

In which the elementary matters are greatly condensed.

In the vegetable kingdom they are less so.

Indestructibility of mineral bodies.

What I have just said of the different degrees of condensation, in which the principles that constitute all natural bodies exist, I advanced ten years ago in the first paper I had the honour to present to the Institute on the composition of alkalis: and I have seen with pleasure, that Mr. Berthollet, in adopting this opinion in his Chemical Statics, has taken it out of the rank of hypotheses.

This property, owing to the powerful affinity of their principles, merits consideration.

As to the indestructibility of mineral substances, to which I ascribe the difficulty of causing the principles that constitute them to retrograde toward a state of less condensation, this too is an opinion, which appears to me to merit all the attention of chemists. In fact, what power, except that of the mutual attraction of the principles that compose all the substances of the mineral kingdom, can enable them to resist the eminently dilatable action of caloric? Thus fire, to effect the decomposition of mineral substances,

substances, must be employed as an auxiliary, and not as an immediate agent.

The decomposition of sulphur, which constitutes the object of this paper, will furnish an application of the principle I have just laid down. However, before we attempt the decomposition of a substance, it is requisite, to have some notion of its composition, that may indicate the nature of the experiments to be made. With respect to sulphur for instance, I had observed, that sulphuric acid strongly saturated with nitrous gas gave a blue colour to water acidulated with it. From the appearance of this colour I inferred, that carbon must be one of the component parts of sulphur: and then considering the property this substance has to dissolve in oils, I suspected, that sulphur might be a compound of carbon and hidrogen. These conjectures were very far from a demonstration; but from these I could proceed as data, either to attack the principles themselves, or to combine them with a third principle, which by its union with them would form a compound already known.

Decomposition of sulphur, as of other bodies, proceeded on by induction.

Nitrogen, for example, appeared to me well adapted to give rise to the compound I should wish to obtain, if hidrogen and carbon were component parts of sulphur.

Nitrogen

In fact, from a combination of these two principles with nitrogen must not a compound be produced analogous to the prussic radical? and would not this product, the elements of which are known, indicate those of sulphur?

should produce with it something like the prussic radical.

To verify how far my conjectures were well founded, I made the following experiment.

Experiment to prove this.

I subjected to calcination in an iron tube four parts of animal charcoal with two parts of sulphate of potash, the whole being intimately mixed. I heated this mixture to a cherry red, and having suffered it to cool to three fourths, I threw it into a large quantity of water.

Animal charcoal and sulphate of potash calcined,

and lixiviated.

When I had filtered the liquor, it was of a green colour, inclining to blue according to the light in which it was viewed. It had but a slight smell of hidrosulphuret. Its taste, though different from that of the prussic radical, produced on the palate a sensation resembling that, by which this radical is characterised.

The lixivium

I tried

not precipitated by acids, I tried afterward whether acids would precipitate sulphur from it, but even the oximuriatic scarcely rendered it turbid. They only evolved from it a peculiar smell, insupportably fetid. However as the nature of the solution indicated the presence of sulphur, I was willing to ascertain, whether it contained any. With this view I let fall into it a few drops of a solution of sulphate of iron at a maximum of oxidation, which immediately occasioned a black precipitate, that was speedily changed to blue by an additional quantity of the solution of the sulphate.

But blue with sulphate of iron.

The sulphur had formed a compound analogous to the prussic radical.

From these different experiments, and particularly from the property of the solution, I no longer doubted, that the sulphur had entered into combination with the nitrogen, and formed a compound analogous to the prussic radical.

Sulphuric acid with nitrous gas precipitated sulphur.

Having afterward examined, what action sulphuric acid saturated with nitrous gas would have on this solution, I remarked, that this acid produced a copious yellow precipitate in it, which to the eye had all the appearance of sulphur, and emitted a similar smell when thrown on live coals. This solution, like those before examined with acids, contained the prussic radical; and the precipitate here mentioned was nothing but this radical, which at the moment of its formation might be converted into Prussian blue by combining it with a few drops of solution of sulphate of iron.

This substance analogous to the prussic radical.

This compound then clearly indicates a substance analogous to the prussic radical, but differing from it in being more fixed, since the strongest acids do not separate it from its solution, while all of them readily decompose the prussiate of potash. Were this the only property, that characterised the radical of which I am speaking, it would be sufficient, to distinguish it from the prussic.

Its fixedness.

With regard to the great degree of fixedness of this new radical, it may be ascribed to the hidrogen, the condensation of which appears to be as strong in this compound, as it is in sulphur; a condensation however, which nitrogen can diminish in forming ammonia with the hidrogen by the decomposition of prussiate of iron.

Is carbon or hidrogen pre-

As to the question, whether carbon or hidrogen be the predominent principle in sulphur, it is obvious, that the process

process I employed to decompose it affords little means of finding the proportions of the two principles.

dominant in sulphur?

There is one observation however, that may throw some light on this question. I have remarked, that the solutions of sulphuretted nitrogen of potash [*azote sulfuré de potasse*] all contain an excess of carbon, which they let fall, if the liquor remain exposed to the open air: whence I have inferred, that the nitrogen did not find in the sulphur the proportion of carbon necessary for the formation of the prussic radical.

Probably hidrogen.

In the next paper I shall have the honour of communicating to the Institute I shall make known the elements of phosphorus, and of iron. I shall likewise notice in it the alkaline metals, in which it is said there is no carbon.

Future researches.

VI.

Experiments in Continuation of those on the Decomposition of Sulphur; by the Same.*

HAVING been informed, that the experiments related in my paper on the decomposition of sulphur have not appeared sufficiently decisive, to authorize the conclusion I have drawn from them, I am impatient to make known fresh facts, that may serve to confirm the results I obtained.

Experiments thought inconclusive.

Exp. 1. Instead of lixiviating the residuum of the calcination of animal charcoal and sulphate of potash, as was mentioned in my paper on sulphur, let it be intimately mixed with one fifth of sulphur, very dry and well levigated; and heat the mixture, either in a gunbarrel or in a stone retort. If the gasses produced in this operation be collected, it will be found, that a great deal of ammoniacal gas is evolved from the commencement of the experiment, to which will succeed hidrogen gas, and carburetted hidrogen gas. When nothing more is given out, extinguish the fire, and, as soon as the vessel is cold, lixiviate the matter it contains in about ten times its weight of water,

Principles of sulphur combined with nitrogen form the prussic radical.

* Journal de Physique, August 1808, p. 117.

and then filter. This lixivium differs from the former in being of a deeper colour, which announces, that carbon is dissolved in it in a larger proportion. It differs from it likewise in containing but little of the prussic radical. However, if it remain a few months in contact with the air, it will acquire more and more the property of precipitating the solution of sulphate of iron of a blue colour; which shows, that the principles of sulphur combined with nitrogen are capable of forming the prussic radical.

Remarkable phenomena

But what is particularly remarkable in this experiment is the hidrogen produced during the operation; also the carbon, which is dissolved in a large quantity in the lixivium; and lastly the almost total destruction of the prussic radical.

accounted for.

In the first place the hidrogen disengaged from a mixture, which gave out none previous to the addition of the sulphur, must necessarily be a product of the latter substance. In the second place, the carbon dissolved in the lixivium must likewise have belonged to the sulphur, since this is the only substance added to the mixture. And lastly the almost total destruction of the prussic radical is explicable by the presence of hidrogen in the sulphur, which, combining with the nitrogen, produces ammonia, that soon escapes from the mixture by iss volatility.

2d experiment.

Exp. 2. Solution of azotized sulphuret of potash acidulated with sulphuric acid, when mixed with a sufficient quantity of sulphate of iron at a maximum of oxidation, yields from a fourth to a third more prussian blue, than the same solution would give if acidulated with sulphuric acid saturated with nitrous gas.

Not justly explained.

Such a difference in the results could not fail to engage my attention, since, from the hypothesis of the disoxigenation of nitrous gas, this, instead of diminishing the proportion of prussian blue, on the contrary should have increased it. I judged from this, that the explanation, which had been given of the phenomenon in question, was not accurate; and that it must result from some other cause, than that on which it had been said to depend.

To ascertain how far this conjecture was well founded, I made

I made several experiments, among which the following appeared to me the most conclusive.

Exp. 3. The solution of azotized sulphuret of potash strongly acidulated with sulphuric acid saturated with nitrous gas yields a copious precipitate of sulphur, while all the other acids scarcely throw down any. 3d experiment.

Several chemists, to explain this truly remarkable property of nitrous gas, have supposed, that this gas was decomposed; and that its oxigen, by combining with the hidrogen that holds the sulphur in solution, favours the precipitation of the sulphur. Explained

Yet if it were true, that oxigen had the property of precipitating sulphur from its solution, why does not the oximuriatic acid act in the same manner as the nitrous gas? Can oxigen possess two such opposite properties, particularly when it acts in similar circumstances? This explanation then presents an anomaly far from favourable to the different hypotheses opposed to the consequences I have drawn from my experiments. It is proper therefore to examine the question in another point of view. erroneously.

In the first place nitrous gas does not act in the solution of azotized sulphuret of potash by oxigenizing the hidrogen of the sulphuret: for this solution, far from containing a surplus of hidrogen beyond the composition of the sulphur, is on the contrary deprived of a part of that which constitutes the sulphur. Accordingly it is by hidrogenizing the dishidrogenized carbon of the sulphur, that the latter is precipitated from its solution, which is very different from the explanation that has been given of this phenomenon. Thus the nitrous gas acts on the solution of azotized sulphuret of potash only in consequence of the affinity this gas has for oxigen, and of that which the dishidrogenized carbon of the sulphur has for hidrogen; an action that concurs at the same time to decompose the water, and with which is combined that exerted by the sulphur on the oxigen. accounted for.

VII. On

VII.

On the Camera Lucida. In a Letter from Mr. T. SHELDRAKE.

To Mr. NICHOLSON,

SIR,

Camera lucida. HAVING been much pleased with the description of the Camera Lucida in your 70th Number, I procured one of the instruments, and made experiments to ascertain the extent of its merits when compared with those of the Camera Obscura. I beg leave to send the result of these experiments, for the information of your readers in general, and in hopes that they may induce the ingenious inventor of the Camera Lucida to bring it still nearer to perfection.

Defects of the camera obscura. The defects of the camera obscura are, that it is cumbersome to carry about and set up for use; that the objects it reflects are, under some circumstances, deficient in point of brilliancy, and that the objects are, under some circumstances, a little distorted from the truth of perspective. For these defects, the skilful artist, who chooses to make use of the instrument, will know how to provide a proper remedy. The drawings that are said to have been made by Abyssinian Bruce* by the assistance of this instrument, the

Bruce a good draughtsman. * It was once fashionable to accuse Mr. Bruce of every kind of breach of veracity: among other things it was said, that he could not draw, and that the drawings he showed as his own were not his, but made by another person. Time has done him justice in many particulars, and if any one still believes that which was said of his drawings, I may, perhaps, contribute a mite towards doing him justice on that head.

Between twenty and thirty years ago there was a sale of drawings at Hutchins's Rooms, King Street, Covent Garden, among them were many drawings, some finished, and others only sketches, which the Auctioneer publicly declared at the sale to have been made by a Mr. Bruce, who had been on a public mission to one of the States of Barbary, and was *then absent on a journey to Abyssinia.*

My father purchased some of these drawings, so that I had them several

the drawings that were certainly made by Mr. Daniel by means of this instrument, and the drawings which are said to be made for different panoramas by the same means, afford convincing proofs,* that it may be of great practical utility in delineating objects with truth and *facility*, greatly superior to what can be practiced even by eminent artists without its assistance. Drawings made by it.

The great advantage of the camera obscura is, that it fixes the objects to be represented upon the surface, so that when the artist has taken his station, and arranged his instrument, he has nothing to do but run his pencil over the objects which he sees lie under his hand, and, in proportion to his capacity for drawing with correctness and facility the objects which lie before him, will his drawings be masterly, beautiful, and correct. What advantages has the camera lucida to oppose to the disadvantages of the camera obscura, or to put in competition with the advantages which the latter instrument is known to possess? Its advantages.

The camera lucida is portable in a very small compass; it represents objects with more brilliancy and distinctness than the camera obscura; and it represents them either singly or in combination, with perfect truth and correctness of perspective. What disadvantages has it then to counterbalance these particulars in which it is evidently superior, in a very great degree, to the camera obscura? Advantages of the camera lucida.

This will, perhaps, be best illustrated by referring to the annexed sketch from nature, which I have drawn with the naked eye; which I attempted to draw with the camera lucida, but could not, and which I have no doubt that I could have drawn with more correctness, facility, and expedition in the camera obscura, than in any other manner. Its disadvantages

several years under my eye; they consisted of figures drawn from nature in the fashionable dress of the time, the sketches drawn with much truth and spirit, the finished drawings tinted with so much taste, that I have no doubt the hand that made them was equal to any thing that was afterward produced as Bruce's, and as they were publicly sold as his before he had acquired any public reputation, or excited the tongue of envy to injure him, there is every reason to believe, that they were actually drawn by Mr. Bruce. These drawings were favourites with me so long as I had access to them; but my father's collection was sold after his death, and I know not what became of them.

When

instanced. When I had taken my stand, arranged my paper, and fixed the camera lucida upon it, I had, upon looking into the eye glass, a distinct view of the whole scene, as perfect as the instrument would represent it; but a different arrangement was necessary, before I could have a *chance* of copying, or, if you please, drawing it: I was to alter the position of the eye glass, so that I should, in the upper part of it, see such of the objects as I was to imitate; and, in the lower part, a distinct representation of the paper and pencil with and upon which I was to draw; these two divisions will admit of different proportions, but, to speak in general terms, we may say, the upper part contains a correct view of part of the objects that are to be drawn, the lower part contains a correct view of the paper on which they are to be drawn, and the pencil by which the drawing is to be made: the operation to be performed is, to look upon the representation of the objects, and the representation of the pencil and paper at the same moment, and to copy exactly upon the lower, what is seen upon the upper part of the object glass; this every man will do in proportion to the power he has of imitating the forms of objects that are placed before him. The essential difference between the camera obscura and the camera lucida is, that the former fixes upon the paper the whole of the picture at one view, and the artist has only to pass his pencil over it to render it permanent, which he has the power to do with more correctness and expedition, and equal facility, as if he was drawing without the use of the instrument. The camera lucida, on the contrary, places before the eye a certain portion of the objects to be imitated, and a certain portion of the paper on which the imitation is to be drawn: the difference between the two operations will be exactly as the difference between tracing and drawing against the window, and copying the same drawing if placed before you upon the table: this is the difference upon a view of the whole proceeding, but, upon descending to minutiæ, other circumstances bear still more against the camera lucida.

Difference between the two.

The process farther described. The circle *Fig.* 2, Pl. X, contains a representation of so much of the view as can be seen at the same time, with so much of

of the paper on which, and the pencil by which it is to be imitated: of course the draughtsman will copy correctly on the lower part those objects which he sees in the upper part of the glass; but these objects constitute but a small part of the whole view; if the remainder is to be attained it must be with great trouble and difficulty: it is true that by moving my head to one side, and looking diagonally across the eye glass, I could see objects that were not visible upon looking directly into it, and thus by moving my head from one side to the other I could get all the horizontal lines, and those lines which approach to the horizontal position upon the paper, so that by this method I could get all the horizontal lines that were within the range of the instrument or the drawing: but it was impossible, by any artifice to do as much with the perpendicular lines, or those which approach to the perpendicular direction, without altering the position of the glass, and in doing this it was found impossible to connect the different portions of the scene that were viewed upon changing the position of the glass, with a degree of truth comparable to what may be attained by the camera obscura without any trouble at all.

The process farther described.

The reader will perhaps comprehend the difficulty if he imagines the great tree in the foreground to be divided horizontally into four or more parts, each of which must be seen by itself and drawn by itself: the glass must then be shifted so as to see and draw another portion without seeing that which had first been drawn, and so on till the whole was completed. Independent of the trouble and waste of time that would be necessary to shift the glass, if it could be done with accuracy, the circumstance of not being able to see the whole of the scene while one is drawing it, and of course comparing the effect of the whole is extremely unpleasant: the instrument must be removed from the paper before the effect of the drawing could be seen, and if it should be necessary to correct it, it is next to impossible to replace it with sufficient accuracy to avoid making false lines, and of course destroying the truth of the representation.

I have stated the inconveniencies that *I* have found, in making

Method of obviating the

inconveniences desirable.

making use of this instrument, and for which I could not find a remedy; others have, as I am informed, found the same inconveniences, and not been able to obviate them; some may have been more fortunate; and if they have, they will render a very acceptable service by pointing out the means of removing these defects: but, if they should do so, I believe it will still be impossible to produce a view, of any magnitude, by means of the camera lucida, with as much ease, expedition, and in as masterly a manner as an able artist can, if he pleases, draw in the camera obscura. This opinion I must entertain, till I see drawings as masterly in point of execution as Mr. Daniel's views in India, made by means of the camera lucida; I mention Mr. Daniel's views on this occasion, because I have been credibly informed, that they were all drawn in the camera obscura, and, as they are well known, they form a good public standard of comparison.

Mr. Daniel's views.

An instrument for taking views still desirable.

It appears then, that a perfect instrument to be used as a delineator is still a desideratum, and will be obtained when the separate advantages of the camera obscura and the camera lucida can be united in the same instrument, and not be diminished by any of the inconveniences to which each of them is at present subject.

I am, Sir,

Your most obliged Servant,

T. SHELDRAKE.

50, *Strand,*
July 6th, 1809.

References to the Drawing.

Fig. 1. Sketch from nature as it may be seen and drawn immediately in the camera obscura.

Fig. 2. Part of the same view as seen in the camera lucida; the upper half contains a portion of the horizontal lines in the view as reflected in the glass: the lower half shows the pencil imitating the same lines upon the paper, it is obvious that by looking diagonally into the glass the view may be extended so as to take in a portion of those lines which cannot be seen when looking directly into the glass.

Fig. 3. Part of the tree seen in the upper half reflected in

Mr. T. Sheldrake on the Camera Lucida.

Fig. 1.

Fig. 2.

Fig. 3.

in the glass, the pencil copying the same parts upon the paper in the lower half. It is evident that no more of this object can be copied at one time than can be seen by looking directly into the glass, of course the whole tree cannot be seen at once, and cannot be copied without shifting the instrument several times, so as to take it by separate pieces, which cannot be *seen at one time*, consequently there is great danger of losing the truth of the whole, while one is employed on each part.

REMARK by W. N.

IT is certainly the intention and instruction of the inventor of the camera lucida, that the tracing should be made upon that part of the paper where the picture and the point of the pencil can both be seen coincident, and not that a copy should be taken in the manner described by Mr. Sheldrake. This requires an attention to the small stop, which regulates the quantities of light which enter the pupil from the prism, and from the paper in the same direction; but I have not found it difficult to manage the position of the eye, which is the principal circumstance—and this will perhaps be as easily acquired by a few trials, as by any minute description of the process, which may be derived from Dr. Wollaston's paper in the 17th Vol. of our Journal, p. 1.

Method of drawing by the camera lucida.

VIII.

Remarks on some of the Definitions and Axioms in Barrow's Euclid. In a Letter from WILLIAM SAINT, *Esq.*

To Mr. NICHOLSON.

SIR, *Cromer in Norfolk, August 4th*, 1809.

IN reading over, a few days since, the 7th book of the English edition of Dr. Barrow's Euclid, several objections occurred to me against some of the definitions and axioms, which I noted down. On reviewing these objections, I must

Remarks on Barrow's Euclid.

must confess, that, *to me*, they appeared to have some weight; I resolved therefore to send them to you, accompanied with such *remarks* (for I dare not aspire to call them notes critical and geometrical) as appeared most applicable.

Submitted to the reader.

These objections and remarks, Sir, are submitted to you, and (should you deem them worthy of insertion in your widely circulated Journal) to your geometrical readers, with the greatest humility.

I am, Sir,

Your obliged and constant reader,

W. SAINT,

Of the Royal Military Academy, Woolwich.

Definition 6.

Definition 6.

"An even number is that which may be divided into two equal parts."

Definition 7.

Definition 7.

"But an odd number is that which cannot be divided into two equal parts; or that which differeth from an even number by unity."

Remarks.

Against these definitions it has been objected, that they are deficient in the word *integral;* which, it has been thought by some, should have been inserted between the words "equal parts" in each definition: for, it has been urged, any *odd* number is divisible into *two equal parts;* as 5, for instance may be divided into two equal parts $2\frac{1}{2}$ and $2\frac{1}{2}$. To this objection however these definitions are not liable, for number is defined to be "a multitude composed of units," and part to be "a number of a number:" therefore a part also must be "composed of units," and hence the objection is obviated.

Definition 8.

Definition 8.

"A number evenly even is that which an even number measureth by an even number."

Definition

Definition 9.

" But a number evenly odd is that which an even number measureth by an odd number." Definition 9.

There appears to be something erroneous in these definitions, since the same number may be found to apply to both of them; for instance, the number 40 is evenly even, because the even number 4 measures it by the even number 10; it is also evenly odd, because the even number 8 measures it by the odd number 5. These definitions would perhaps be less exceptionable, if expressed thus: *Definition* 8. A number evenly even is that which may be divided into two equal parts, having each part an even number. *Definition* 9. But a number evenly odd is that which may be divided into two equal parts, having each part an odd number. Remarks.

Definition 15.

" One number is said to multiply another, when the number multiplied is so often added to itself, as there are units in the number multiplying, and another number is produced. Definition 15.

This definition appears to be improperly expressed: for if, for instance, it were required to multiply the number 3 by the number 2, it is necessary, to obtain the product according to the definition, to add the number 3 to itself so often as there are units in the number 2, that is to say twice to itself; now the number 3 added *once* to itself gives 6, and added *twice* to itself gives 9; thus 9 would be obtained for the product of 3 multiplied by 2, which is evidently erroneous. Perhaps this definition would be better thus: one number is said to be multiplied by another, when it is taken or repeated as many times as there are units in that other. To those, however, who may be disposed to contend, that the words " taken or repeated" do not sufficiently define the operation intended; and who may farther insist, that multiplication is only *a continued addition*, Euclid's definition may perhaps be preferred, if the words *less one* be inserted after the word *multiplying*. Remarks.

Definition 23.

" One number is said to measure another by a third number, which, when it either multiplies, or is multiplied by the measuring number, produces the number measured." Definition 23.

This

Remarks. This definition seems to be objectionable on this ground, that it defines a number A, to measure another number B, by a third number C, when *either* C multiplied by A, or A multiplied by C, produces the number B. Now the possibility, that C×A can be equal to A×C forms the subject of the 16th proposition of the *very book* to which this definition is *prefixed.* To say the least, therefore, this definition is *out of order:* and as Euclid does not appear to have made any use of it, till after the 16th proposition, so certainly it ought not to have been given till the truth of the proposition virtually implied in it had been demonstrated; that is to say, till it had been proved, that C multiplied by A is equal to A multiplied by C, to which proposition it might have formed a corollary.

Axiom 7.

Axiom 7. "If one number, multiplying another, produce a third, the multiplier shall measure the product by the multiplied; and the multiplied shall measure the same by the multiplier."

Remarks. The first part of this axiom is admissible, since it only implies, that, if any number, A, be first multiplied by any other number, B, and then divided by the same number, B, the quotient will be A,—a truth which is evident from the opposite effects of multiplication and division. The latter part of this axiom appears to be objectionable, for it does not, like the former part, first suppose an operation to be performed upon a number A, and then the effect of that operation to be done away, or withdrawn by another operation of a *directly opposite nature;* for though by this latter part it is required to multiply A by B as before, yet it is not required afterward to divide by B, but by A: and though it may be an obvious truth, that A first multiplied by B, and then divided by B, will give A; yet it is by no means so obvious, that A multiplied by B, and then divided by A, will give B, for here the operations of multiplication and division are by *different numbers.* By the former part of this axiom, if B be first multiplied by A, and then divided by A, the result will be B; and if the latter part of it were *self evident,* namely, that A multiplied

plied by B, and then divided by A, would give B also, it would be $\frac{B \times A}{A} = \frac{A \times B}{A}$, or $B \times A = A \times B$; hence it appears, that the latter part of this axiom virtually implies the truth of the 16th proposition, and is therefore objectionable on the same grounds as the 23d definition.

Axiom 8.

"If one number measure another, that number by which it measureth shall measure the same by the units that are in the number measuring, that is, by the number itself that measures." Axiom 8.

This axiom implies, that if $\frac{A}{B} = C$, then $\frac{A}{C} = B$. Now this is really more of a proposition than an axiom. By the former part of the last axiom it may indeed be *inferred*, that, since $\frac{A}{B} = C$, A must be $= C \times B$; because $\frac{C \times B}{B} = C$; but, as it has before been shown, it by no means follows because $\frac{C \times B}{B} = C$, that therefore $\frac{C \times B}{C} = B$. This axiom therefore is objectionable upon the same grounds with the last. Remarks.

Axiom 9.

"If a number measuring another, multiply that by which it measureth, or be multiplied by it, it produceth the number which it measureth. Axiom 9.

This axiom implies, that, if a number A measures another number B by a third number C, then A multiplied by C, or C multiplied by A, gives the same product B; that is to say, this axiom implies the truth of the 16th proposition, and is therefore objectionable on the grounds before stated. Remarks.

Proposition 16.

As there has been frequent occasion to refer to this proposition in the preceding remarks, it may not be improper to observe here, that it is one of those which has engaged the attention of several eminent mathematicians of the present day, and among others the celebrated Legendre, who, in his "*Essai sur la Théorie des Nombres*," has given a demonstration Proposition 16 has engaged the attention of many eminent mathematicians.

demonstration of the same; from which it may be concluded, that Mr. Legendre himself did not consider Euclid's demonstration of this proposition as *perfectly satisfactory*. Indeed it must be confessed, that in Euclid's demonstration, as given by Dr. Barrow at least, there is an air of *obscurity*, which renders it difficult to be understood. For the satisfaction of such of your readers as may not be in possession of an edition of Euclid containing the 7th book, it may be proper here to give both the enumeration and demonstration of this proposition, as they are found in Dr. Barrow.

Proposition.

Proposition 16. "If two numbers, A, B, mutually multiplying themselves produce any numbers AB, BA; the numbers produced, AB and BA, shall be equal the one to the other."

B 4	A 3
A 3	B 4
AB 12	BA 12

Demonstration.

Euclid's demonstration. For because $AB = A \times B$ *(a)* therefore shall 1 be as often in A, as B in AB, *(b)* and by consequence alternately 1 shall be as often in B as A in AB. But because $BA = B \times A$, *(a)* therefore shall 1 be as often in B, as A in BA; therefore as often as 1 is in AB, so often is 1 in BA; and *(c)* so $AB = BA$. W. W. D.

a 15 *def.* 7
b 15 7
c 4 *ax.* 7

Remarks. With respect to this demonstration it must be observed, that the attentive student meets with a difficulty in the very beginning; for why does it follow, because $AB = A \times B$, that 1 shall be as often in A as B in AB? That $AB = A \times B$ is an identical proposition, and implies no more than that A multiplied by B is equal to A multiplied by B, from which no inference can be drawn. The next step of the demonstration, namely, "And by consequence alternately 1 shall be as often in B as A in AB, is deduced from the preceding by virtue of the 15th proposition, which proves, that, if 1 be contained in B as often as D is contained in E, then 1 is contained in D as often as B is contained in E. The demonstration proceeds with, "but because $BA = B \times A$, therefore shall 1 be as often in B, as A in BA." Now this is

is objectionable upon the same principle as the first step of the demonstration. The next step is in these words: "Therefore as often as 1 is in AB, so often is 1 in BA." But this does not appear to be the most natural and obvious inference from what has been previously attempted to be proved; for, if it had been satisfactorily shown, that 1 is contained as often in B as A in AB, and that 1 is contained as often in B as A in BA, the natural inference it appears would be, that A is contained in AB as often as A is contained in BA, and so finally AB=BA.

From the objections here stated the following demonstration is easily derived, which is submitted to the consideration of the lovers of geometrical accuracy with the greatest humility, as seeming to afford a more satisfactory proof of the proposition than the one above given.

In this demonstration it may be proper to observe, that, to avoid any ambiguity, the sign of multiplication, or ×, should be read by the words *multiplied by*. It has been thought better also, instead of *referring* to the proposition, definition, or axiom, on which any of the steps in the process depend, to insert these at length.

Demonstration.

Since by Axiom 5 "unity measures every number by the units that are in it, that is, by the same number," therefore 1 measures A, A times; and since by the first part of Axiom 7, "If one number multiplying another produces a third, the multiplier shall measure the product by the multiplied;" therefore B shall measure A×B, A times; hence 1 shall be as often in A, as B in A×B: but by Proposition 15, if 1 measures A as often as B measures A×B, then 1 shall measure B as often as A measures A×B, or 1 shall be as often in B, as A in A×B: Again by Axiom 5, as above quoted, 1 measures B, B times, and by Axiom 7, A measures B×A, B times, therefore 1 shall be as often in B as A in B×A; but it was shown above, that 1 shall be as often in B as A in A×B; therefore, as often as A is in B×A, so often is A in A×B: but by Axiom 4 "Those numbers, of which the same number, or equal numbers, are the same parts, are equal amongst themselves;" therefore B×A is equal to A×B. W. W. D. New demonstration.

IX. *Account*

IX.

Account of a New Acid, obtained from Ginger. In a Letter from a CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

Acid from ginger.

BY the following process an acid (which I consider as new, and would propose calling the zingiberic) was obtained from ginger.

Process for obtaining it.

One ounce of the best white ginger was infused during two or three days, in six ounces of nitrous acid; after which rather more than an equal quantity of water was added, and the whole was kept at the heat of 212° adding water to supply the loss by evaporation, till the nitrous smell had disappeared. Carbonate of lead was then added to saturation, and the solution filtered. The lead was in the next place precipitated by sulphuric acid, and a second filtration was made.

Its properties.

By evaporating the filtered liquor, an acid, similar in appearance to short white pieces of raw silk, was obtained, which oxidates zinc and iron, and dissolves potash, soda, ammonia, barytes, strontian, lime, magnesia, and the oxides of zinc, iron, lead, and copper.

Its combination with magnesia.

The only farther account I can at present give of its salts is, that the (perhaps super-) zingiberate of magnesia has a taste intermediate between that of acetite of lead, and triple supersulphate of alumine.

Its difference from other acids.

The zingiberic acid differs from the sulphuric, sulphurous, carbonic, oxalic, tartarous, citric, mucous, succinic, and camphoric acids, in forming a soluble salt with barytes and lime;

From the nitric, nitrous, muriatic, acetic, acetous, sebacic, malic, and prussic, by remaining in the solid form at 212°;

From the benzoic and suberic, by its greater solubility;

And it does not, like gallic acid, precipitate copper of a brown colour.

A CORRESPONDENT.

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215	7	*for* preserved *read* pursued.
220	22	*for* as in seeds *read* as well as in seeds.
228	6 from bottom	*for* determined *read* diminished.
350	5 from bottom	*read* Fig. 13. Section just above the seed vessel. *a, a,* the calyx. *b, b,* the corolla. *c, c, c, c,* four stamens. *d,* the pistil. Fig. 14. Bottom of the seed vessel of the dianthus *a,* the calyx, &c.

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And of British Memoirs abridged or extracted, Humphry Davy, Esq. Sec. R. S. F. R. S. Ed. and M. R. I. A.; Capt. W. Bolton; Capt. H. L. Ball; Mr. John Tad; Mr. W. Barlow; J. G. Children, Esq. F. R. S.; Wm. Henry, M. D. F. R. S. V. P. of the Lit. and Phil. Soc. and Physician to the Infirmary at Manchester; James Rennel, Esq. F. R. S.

The Engravings consist of 1. Captain Bolton's improved Jury Mast; 2. Captain H. L. Ball's Method of Fishing an Anchor; 3. Captain Ball's improved Anchor; 4. Mr. J. Tad's Method of causing a Door to open over a Carpet; 5. Mr. W. Barlow's Wrench for Screw Nuts of any Size; 7. The Sting of the Nettle, highly magnified, in its natural State, emitting its Poison, and when broken; 8. The Awn of the Indian Grass, used in Captain Kater's Hygrometer; 9. The Leaf and Stem of the Sensitive Plant, showing their Structure; 10. The Spiral Wire and its Case greatly magnified; 11. Luminous Meteors, seen during a Thunderstorm, by James Staveley, Esq.; 12. Diagrams to illustrate the Theory of Aerial Navigation, by Sir George Cayley, Bart.; 13. A Machine that will ascend into the Air of itself by mechanical Means; 14. A Machine with which a Man may raise himself into the Air; 15. Figures illustrating the Crystallization of Endellion, by the Count de Bournon; 16. Diagrams for a Demonstration of the Cotesian Theorem, by Mr. P. Barlow; 17. Various Delineations and Sections of Grafts and Buds, from original Drawings after Nature, by Mrs. Agnes Ibbetson; 18. Branch of a Portugal Laurel, from which the Bark had been accidentally separated; 19. Different Structures of several Kinds of Wood.

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A

JOURNAL

OF

ATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1809.

ARTICLE I.

the Union of Tan and Jelly: *by* JOHN BOSTOCK, *M.D.*

To Mr. NICHOLSON.

IR,

DURING the course of the last spring I was engaged in t of experiments, which may be considered as a conti- tion of those formerly made on the analysis of animal ds*. My object was to enable the operator to apply the s, which indicate the existence of the principal consti- its of these fluids, albumen, jelly, and mucus, so as not to discover the *qualities* of the compound, but the *ntities* of its ingredients. The results of my experi- its have been, upon the whole, unsuccessful; and I have present chiefly to announce the failure of the different edients, which I employed to attain my object. It may however, be altogether useless, to lay my experience Purpose of the author's inquiry.

* See Journal, vol. IX, p. 244.

before your readers; not merely because I have it in my power to state some few facts, that may be considered as an addition to our stock of knowledge, but still more, because I may induce some one more skilful than myself, to point out a method of accomplishing what I have hitherto attempted without success.

Jelly.

Its characters.

The substance upon which I first operated, and to which I shall principally confine my attention in the present paper, is jelly; the characteristics of which are its solubility in water, its forming an insoluble compound with tan, and the property which its aqueous solution possesses of concreting by cold, and being redissolved by the application of heat.

Inquiry whether the precipitate by tan be proportionate to the quantity present.

The problem which I was anxious to solve was, whether the compound of tan and jelly be uniform, so that by saturating the gelatinous part of a solution with tan, and collecting the precipitate, we may, from its weight, (the quantity of tan employed being known) ascertain the amount of the jelly previously contained in the fluid. From the experiments that had been performed on the subject, particularly those of Mr. Biggin and Mr. Davy, I conceived, that this would be found to be the case.

Mr. Biggin's attempt to ascertain the proportion of tan.

The object of Mr. Biggin's experiments was to ascertain the proportion of tan in different barks, for which purpose he formed similar infusions of them, and precipitated the tan from each by a solution of glue. He employed the solution of glue always of the same strength, and by collecting the precipitates, he judged of the quantity of tan that had united itself to the glue, and thus of the proportion of it in the bark*. The experiments are important, as comparing the different barks with each other, and thus ascertaining their respective value as substances to be employed in the manufacture of leather; but it is obvious, that, unless the compound of tan and glue be uniform, they do not show the absolute quantity of tan in any given weight of bark.

Mr. Davy shows the precipitate is proportionate to the strength of the solution.

Mr. Davy, in his experiments on astringent substances, has pointed out, with his accustomed sagacity, the different effects that are produced in the union of solutions of tan and jelly, according to their degree of concentration; and has proved, that in

* Phil. Trans. 1799, p. 260.

proportion

proportion to the strength of the solution, either of jelly or of tan, will be the weight of the precipitate obtained*. It would appear, that, when the solutions are much diluted, the attraction of both the jelly and the tan for the water, to a certain extent, counteracts their attraction to each other, and thus prevents a portion of them from being removed from the fluid. Mr. Davy, however, as well as Mr. Biggin, evidently seems to have conceived, that the substance which was precipitated in all instances possessed the same properties, and consisted of a uniform compound of the two ingredients. This opinion is the very foundation of the method which he employed in his analyses, and is directly asserted in different parts of his papers †.

Both suppose the precipitate to be a uniform compound.

With this impression it was, that I entered upon a set of experiments, which may be considered as the converse of those of Mr. Biggin and Mr. Davy. The object of these chemists was, by the agency of jelly, to remove all the tan from a vegetable infusion, and to estimate its quantity from the weight of the precipitate; while mine was, by means of tan, to ascertain the quantity of jelly that was contained in any animal fluid. In pursuing this investigation, the first point was to determine upon the most proper substance to employ as the reagent; for as it is difficult, if not absolutely impossible, to procure tan in a state of perfect purity, it became necessary to discover some vegetable infusion, which should always possess similar properties, and in which the quantity of tan should be known, without having recourse to any long calculation. My attention was naturally, in the first instance, directed to galls; and I expected, that by employing equal weights, infusing them in equal quantities of water, and for an equal length of time, fluids would have been formed always containing equal quantities of tan. But upon making repeated trials, I find that this is not the case; and it would appear from all the experience I have had upon the subject, that two parcels of galls will scarcely ever be procured, which will precisely agree in their

The author's proceeding the converse of theirs.

A uniform reagent requisite.

Galls.

Not uniform in their nature,

* Phil. Trans. 1803.

† Phil. Trans. 1803. Nicholson's Journal, vol. V, p. 259, 269, & alibi.

nature. If finely powdered galls be infused for two hours in 8 times their weight of boiling water, an infusion is formed, which is generally transparent, of a deep brown colour, and which contains about one tenth of its weight of solid matter. But although this is the usual result of the process, it is by no means constantly so. Frequently the infusion will be thick and muddy, will not be rendered clear by being passed through the filter, nor will it become so after standing at rest for several days; its colour also varies considerably, the brown tinge existing in different shades of intensity, and occasionally being exchanged for a bottle green. The quantity of solid matter contained in the fluid is seldom precisely the same in any two trials; although it is generally about one tenth, yet I have occasionally found it no more than one fourteenth. Although it may appear at first view somewhat singular, that such different effects should be produced by the same substance; yet, when we attend to the visible difference, that exists in gall nuts, we shall easily conceive how these variations may take place. The structure of galls appears to have been little attended to, and they have generally been spoken of as homogeneous bodies, before the accurate description of their several parts, that is given by the Mr. Aikins in their late valuable publication*.

or homogeneous in their structure.

An extract of them does not answer.

As it appeared impossible to employ a recent infusion of galls for the standard fluid, I thought of evaporating the infusion, and making use of a solution of the dried residuum. But I found, that this residuum, although formed from a perfectly transparent infusion, is not capable of being completely redissolved, owing to some change that has been effected on one or more of its constituents, probably the extract, by which it becomes no longer soluble in water. This circumstance forms an insuperable objection to the employment of the dried residuum as a standard, because the quantity of matter, depending upon the variable proportion of the soluble and insoluble part, or of the tan and extract, will scarcely ever be found the same in any two specimens upon which we may operate.

* Aikins' Chem. Dict. Art. Gall nut.

The

Mr. Hatchett's artificial tan

The infusion of galls, however prepared, seemed inadequate to the purpose of affording an accurate test for jelly, I thought therefore of employing the artificial tan discovered by Mr. Hatchett, because, being a substance formed by a specific chemical action, it may be supposed always to possess the same chemical properties. It was accordingly prepared by digesting powdered charcoal in nitric acid, and the result coincided entirely with the description of Mr. Hatchett; it was readily dissolved both in water and alcohol, it precipitated jelly from its solution, and also the nitromuriate of gold, the muriate of tin, the superacetate of lead, and the oxisulphate of iron. All these properties show its strong resemblance to the infusions obtained from astringent vegetables. I was however disappointed in not finding it to answer the purpose that I had in view. Although the artificial tan very readily afforded a precipitate from a gelatinous solution, yet the jelly seemed to be only imperfectly thrown down, the fluid remained muddy after the operation, and the precipitated matter could not be completely separated from it. This circumstance I found to take place with different portions of the artificial tan, which were each of them prepared with every attention to Mr. Hatchett's directions; and, I conceive, depends upon a quantity of undecomposed acid, which remains attached to the tan, and which cannot be entirely removed from it. This excess of acid was always found in my experiments, and must probably have existed in Mr. Hatchett's preparations, for he points out their property of reddening litmus as one that is characteristic of them *. To whatever cause we may ascribe it, it seemed to be a sufficient objection to the use of this substance as a test for jelly.

formed an imperfect precipitate,

owing to the presence of undecomposed acid.

Catechu does not answer.

Catechu was next tried, but without any better success. Independent of the difference which exists between different specimens of this substance, which is considerably greater than what is found in the infusion of galls, I have never met with any catechu which is entirely soluble in water. In the different trials that I made to procure standard

* Phil. Trans. for 1805, p. 215.

solutions

solutions of catechu, a portion appeared to be only suspended in the fluid, so that it remained muddy, and neither became transparent by standing, nor was the insoluble part removed by passing through a filter. The infusions of catechu likewise became altered by exposure to the atmosphere more rapidly than those of galls, a considerable portion of the substance that had been dissolved being gradually deposited. This deposition goes on so rapidly, as to exhibit an appearance something similar to the saline vegetation of certain salts, the catechu creeping up along the sides of the glass to some distance above the surface of the fluid, I was not able to detect any difference between the part of the catechu which is retained in solution, and that which is deposited, except that the last was of a lighter colour, and was less soluble in water; they both produced precipitates with jelly and the muriate of tin. The precipitate which the catechu forms with jelly, like that produced by the artificial tan, does not in general form a compact or solid mass, but makes the fluid turbid without entirely subsiding from it, nor is it rendered transparent by being passed through a filter. This circumstance, as well as its imperfect solubility, renders catechu inapplicable as a test for jelly.

Spontaneous decomposition of its infusions.

Its precipitate like that of artificial tan.

Extract of rhatany.

The next substance that I tried was the extract of rhatany, a preparation said to be brought from the Portuguese settlements in South America; and, in consequence of its tonic quality, lately proposed as an addition to the materia medica. It contains a large proportion of tan; from the experiments that I have made upon it, larger than any other astringent extract with which we are acquainted; it appears to be more homogeneous in its consistence, it is completely soluble in water, and seems to remove jelly from its solution more readily than the other substances which I had tried. These properties pointed it out as the most nearly approaching to what I was in search of.

Preparation of the jelly.

Before giving an account of the result of the union of tan and jelly, it will be necessary to make some remarks upon the preparations of this latter substance. It has been stated upon the highest authority, that of Mr. Hatchett and Mr. Davy,

Davy*, that isinglass consists nearly of pure jelly. I am not disposed to question the general fact; but I may mention, as the result of my own experience, that this is not always the case; and that, even in a majority of instances, the isinglass that is procured from the ſhops will be found to contain a considerable proportion of insoluble matter, which I conceive to be of the nature of coagulated albumen. The proportion of the matter soluble in water, which I regard as pure jelly, and of the insoluble part, is very various. In one instance, where the isinglass was boiled with twenty times its weight of water, the jelly that was formed, instead of holding in solution 5 per cent of solid matter, was found to contain not more than 3·8, and although the addition of more water carried off some of the substance which had been left at the first boiling, still more than $\frac{1}{10}$ of the isinglass was left, apparently incapable of farther solution. This difficulty is obviated by boiling isinglass in water, pouring off the jelly, and evaporating it to dryness; by which means a substance is procured, that is always ready for experiments. But as this operation is attended with some trouble, I wish to substitute for it a solution of glue, according to the process employed by Mr. Biggin. Glue is entirely soluble in water, and therefore does not present the objection that attaches to isinglass, yet there are some circumstances, which seem to render glue less eligible for the purpose of experiments. From the mode in which glue is prepared it might be supposed, that it would contain a quantity of albuminous matter; and I was confirmed in this opinion by finding, that a solution of it has a precipitate formed in it by being boiled with the oximuriate of mercury†. The quantity of muriate of soda that exists in glue must be considered as an impurity, which may have some effect upon the combination of jelly and tan. A more important circumstance, however, and one which appears to have been disregarded by those who have employed glue as a test for tan is, that, as it is usually prepared, it contains a considerable proportion of

Isinglass variable in the proportion of its gelatine,

but this may be separated.

Glue contains albumen,

muriate of soda,

* Hatchett, Phil. Trans. 1800; Davy, Phil. Trans. 1803.

† This circumstance had been noticed by Dr. Thomson, Chem. Vol. V, p. 479.

water.

and much water.

water. By permitting glue divided into small pieces to remain in a heat of about 150° for 24 hours, I found that it had lost 10½ per cent of its weight. And even although we might have the glue in a state of complete dryness and purity, I should doubt whether it be a proper substance to employ on the present occasion. Although it possesses the properties which characterize jelly, yet a solution of glue will be found to differ from a solution of isinglass, while they both contain the same proportion of solid matter. This difference is the most remarkable with respect to their power of concretion. A solution of glue, which I found by evaporation to contain $\frac{1}{15}$ of its weight of solid matter, although strongly adhesive, remained quite fluid when cold, whereas a similar solution of isinglass jelly would have been perfectly concrete. Glue also differs from isinglass in being considerably more soluble in cold water. Glue broken into small pieces, and digested in ten times its weight of water, at the temperature of the atmosphere, was in 48 hours entirely broken down, and so far dissolved, that the upper part of the fluid strongly precipitated the infusion of galls. Pieces of isinglass treated in the same manner were softened, and had their bulk increased, but the fluid was scarcely affected by tan. These circumstances led me to regard glue as different from isinglass jelly, and as possessing in an inferior degree the characteristic properties of jelly.

Glue differs considerably from isinglass.

Experiments made with the soluble parts of isinglass and extract of rhatany.

From these different circumstances I determined to employ the soluble parts of isinglass, and the extract of rhatany, in my future experiments on the combination of tan and jelly. But before I enter upon a description of the results, that were obtained by the union of these substances, I think it necessary to point out the difficulty, which occurs in the prosecution of these experiments, particularly in the collecting of the precipitate. When the tan and jelly are not employed in a state of considerable concentration, and when they are not added together in that proportion, which seems to form the most perfect compound, the precipitate separates slowly from the fluid, or sometimes remains permanently suspended; and when it is passed through a filter, it adheres to the paper so strongly, that it cannot be com-

Difficulty of collecting the precipitate.

pletely

pletely removed from it. Nor could I obviate this objection by weighing the paper before and after the fluid had passed through it, and thus calculating the weight of the precipitate. I found that in this case the paper acquired weight, not only from the precipitated matter, but likewise from what was still retained in solution. When an infusion of rhatany in the proportion of 1 to 10 was passed through a paper filter, the filter when dried was found to have acquired an addition of not less than $\frac{1}{10}$ of its former weight. A solution of jelly of the same strength passed with difficulty through the paper, and a large part was detained by it. Hence it follows, that, except in those cases where the fluids neutralize each other, so as to precipitate all their contents, we cannot ascertain the amount of the precipitate from the weight gained by the filter. What has been said will be sufficient to ſhow, that perfect accuracy cannot be attained in these processes, even were the compound of tan and jelly in all cases a uniform substance.

The precipitate not uniform.

I was soon however convinced, that the substance formed by the union of tan and jelly varies considerably according to the circumstances under which it is formed, particularly according to the proportion in which the two ingredients are presented to each other. Without entering into a detail of the numerous trials, that I made upon this subject, I ſhall think it sufficient to give an account of one experiment, that may serve as a specimen of the rest. I must here remark, that, although my experiments agreed sufficiently to satisfy me respecting the nature of the conclusions that were to be deduced from them, yet I never performed two, in which the results exactly coincided.

Experiment.

Three equal portions of the extract of rhatany were dissolved in ten times their weight of water; and three portions of jelly from isinglass were procured, bearing respectively the proportions of 8, 4, and 2 to the three portions of rhatany. These were also dissolved in equal quantities of water, kept soluble by heat, and added to the three portions of rhatany. Copious precipitates were produced in all of them, and after standing for some time, the supernatant fluids became clear. The precipitates were collected and dried by exposure to the same degree of heat. All the residual

dual fluids precipitated jelly, proving that they contained a quantity of uncombined tan; but the precipitation was of course much less copious in the one which had received the smallest quantity of jelly. The weights of the precipitates were to each other in the ratio of 16, 9·5, and 7. As in all the cases the whole of the jelly had entered into combination, the proportion of the jelly to the tan might be estimated. In the first experiment, i. e. where 8 parts of jelly and 10 of tan were employed, the jelly and tan in the compound were nearly equal; where 4 parts of jelly had been added to 10 of the rhatany, the proportion in the compound was as 42 to 58; and where only 2 parts of jelly had been employed, the compound consisted of 28·5 parts of jelly to 71·5 of rhatany. From these experiments we learn, that in proportion as the tan exists in excess more of it becomes united to the jelly; so that if we were to attempt to estimate the amount of the jelly in any fluid by the weight of the compound which it forms with tan, we should much overrate the quantity of the jelly. Having found, that, where the solutions are employed in a state of considerable concentration, a compound is formed consisting of nearly equal weights of the two ingredients, we might conclude, that the quantity of jelly in the third experiment was in the proportion of 3·5, while in fact it was no more than as 2.

When the tan is in excess, more unites with the jelly.

Differences of the precipitates.

The physical properties of the precipitates were considerably different, so as to indicate a difference in their chemical composition. The first precipitate, which was composed of nearly equal parts of the two ingredients, was of a dark red colour, of a hard and brittle consistence, and presented a shining fracture. The second was also hard, but rather tough, and it had a brown hue; the third, containing the smallest quantity of jelly, was of a bright reddish brown, and could be pulverized between the fingers. In order to establish more clearly the difference between these precipitates, they were subjected to the action of such reagents as might have the power of removing from them the excess of tan, and leave the compound in its most perfect state. This seemed to be effected by boiling the third precipitate in a large quantity of water, in consequence of which process the fluid was found to have acquired the property of copiously precipitating

precipitating jelly. The water exhibited a reddish tinge, and was also slightly affected by the addition of iron, showing, that it contained a minute portion of gallic acid; no effect was, however, produced on it by the muriate of tin. The boiled precipitate now approached in its appearance to the one which was composed of equal parts of tan and jelly; it was of a deeper colour and harder consistence. The first of these three precipitates was boiled in the same manner that this third had been, but the water was not in the least degree affected by jelly. It may appear singular, that any part of a substance, which had been precipitated from water, should be dissolved by it, but it probably depends upon the action of the greater mass of the fluid; and the fact is confirmed by Mr. Davy's remark, that the stronger the solutions are upon which we operate, the more completely will their solid contents be separated from them.

Accurate results not to be obtained in this process.

From the foregoing observations and experiments we may infer, that the method of detecting the quantity of jelly in any fluid, by the precipitate which it forms with tan, cannot be employed with any prospect of obtaining accurate results; nor can jelly be depended upon for the purpose of obtaining the amount of the tan in any astringent vegetable infusion. In the animal analysis this deficiency will probably be found of little importance; for, notwithstanding the proportion of jelly which enters into our solids, and which may be readily extracted from them by water, I am inclined to believe, that nothing, which is properly entitled to the name of jelly, will be found to exist in any of our fluids. When I first began these investigations I was induced to form a contrary opinion, and a contrary doctrine is maintained in our most valuable systematic works. I have, however, endeavoured to prove, that jelly is not found in the blood, where it has been supposed to exist in the largest quantity*; I do not find any trace of it in the albumen ovi, in the saliva, in the fluid of the hydrocephalus, of spina bifida, or of ascites, nor in the liquor amnii. By far the largest proportion of animal matter in all these fluids is albumen, existing sometimes in its coagulated, and sometimes

No jelly in the animal fluids,

but albumen,

* Medico-chirurgical Trans. V. I, p. 47.

in

and mucus? in its uncoagulated state. There appears, however, to be some animal substance beside the albumen, at least in the greatest part of them, to which I have hitherto assigned the name of mucus, but whether properly or not, must be the subject of future consideration.

I am, Sir,

Your obedient servant,

Liverpool, Aug. 3, 1809. J. BOSTOCK.

II.

The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, &c. By HUMPHRY DAVY, *Esq. Sec. R. S. F. R. S. Ed. and M. R. I. A.*

(Continued from vol. XXIII, p. 334.)

6. *Experiments on the Decomposition and Composition of the Boracic Acid.*

Boracic acid decomposed.

IN the last Bakerian Lecture* I have given an account of an experiment, in which boracic acid appeared to be decomposed by Voltaic electricity, a dark coloured inflammable substance separating from it on the negative surface.

Attempt to effect this in quantities.

In the course of the spring and summer, I made many attempts to collect quantities of this substance for minute examination. When boracic acid, moistened with water, was exposed between two surfaces of platina, acted on by the full power of the battery of five hundred, an olive-brown matter immediately began to form on the negative surface, which gradually increased in thickness, and at last appeared almost black. It was permanent in water, but soluble with effervescence in warm nitrous acid. When heated to redness upon the platina it burnt slowly, and gave off white fumes, which slightly reddened moistened litmus paper; and it left a black mass, which, when examined by

* Phil. Trans. for 1808, p. 43; or Journal, vol. XX, p. 331.

the

the magnifier, appeared vitreous at the surface, and evidently contained a fixed acid.

These circumstances seemed distinctly to show the decomposition and recomposition of the boracic acid; but as the peculiar combustible substance was a nonconductor of electricity, I was never able to obtain it, except in very thin films upon the platina. It was not possible to examine its properties minutely, or to determine its precise nature, or whether it was the pure boracic basis; I consequently endeavoured to apply other methods of decomposition, and to find other more unequivocal evidences upon this important chemical subject.

The combustible substance obtained only in thin films.

I have already laid before the Society an account of an experiment*, in which boracic acid, heated in contact with potassium in a gold tube, was converted into borate of potash, at the same time that a dark coloured matter, similar to that produced from the acid by electricity, was formed. About two months after this experiment had been made, namely, in the beginning of August, at a time that I was repeating the process, and examining minutely the results, I was informed, by a letter from Mr. Cadell at Paris, that Mr. Thenard was employed in the decomposition of the boracic acid by potassium, and that he had heated the two substances together in a copper tube, and had obtained borate of potash, and a peculiar matter concerning the nature of which no details were given in the communication†.

Boracic acid decomposed by Thenard.

That the same results must be obtained by the same methods of operating, there could be no doubt. The evidences for the decomposition of the boracic acid are easily gained; the synthetical proofs of its nature involve more complicated circumstances.

I found, that, when equal weights of potassium and boracic acid were heated together in a green glass tube, which had been exhausted after having been twice filled with hidrogen, there was a most intense ignition before the temperature was nearly raised to the red heat; the potassium entered into vivid inflammation, where it was in contact with the

Potassium and boracic acid heated together.

* Phil. Trans. Part II, 1808, p. 343; or Journal, vol. XXI, p. 375.

† Gay Lussac and Thenard's paper is given in our last volume, p. 260.

the boracic acid. When this acid had been heated to whiteness, before it was introduced into the tube, and powdered and made use of while yet warm, the quantity of gas given out in the operation did not exceed twice the volume of the acid, and was hidrogen.

Large quantities could not be used.

I could only use twelve or fourteen grains of each of the two substances in this mode of conducting the experiment; for when larger quantities were employed, the glass tube always ran into fusion from the intensity of the heat produced during the action.

Effect of naphtha.

When the film of naphtha had not been carefully removed from the potassium, the mass appeared black throughout; but when this had been the case, the colour was of a dark olive-brown.

Proper proportion of the two.

In several experiments, in which I used equal parts of the acid and metal, I found that there was always a great quantity of the former in the residuum, and by various trials, I ascertained that twenty grains of the potassium had their inflammability entirely destroyed by about eight grains of boracic acid.

Apparatus.

For collecting considerable portions of the matters formed in the process, I used metallic tubes furnished with stopcocks, and exhausted after being filled with hidrogen.

When tubes of brass or copper were employed, the heat was only raised to a dull red; but when iron tubes were used, it was pushed to whiteness. In all cases the acid was decomposed, and the products were scarcely different.

Results in a copper tube.

When the result was taken out of a tube of brass or copper, it appeared as an olive coloured glass, having opaque, dull olive-brown specks diffused through it.

It gave a very slight effervescence with water, and partially dissolved in hot water, a dark olive coloured powder separating from it.

In an iron tube.

The results from the iron tube, which had been much more strongly heated, were dark olive in some parts, and almost black in others. They did not effervesce with warm water, but were rapidly acted upon by it, and the particles separated by washing were of a shade of olive, so dark as to appear almost black on white paper.

Solutions.

The solutions obtained, when passed through a filter, had a faint

a faint olive tint, and contained subborate of potash, and potash. In cases when instead of water a weak solution of muriatic acid was used for separating the saline matter from the inflammable matter, the fluid came through the filter colourless.

Largest quantities collected in brass tubes.

In describing the properties of the new inflammable substance separated by washing, I shall speak of that collected from operations conducted in tubes of brass, in the manner that has been just mentioned; for it is in this way, that I have collected the largest quantities.

Its properties.

It appears as a pulverulent mass of the darkest shades of olive. It is perfectly opaque. It is very friable, and its powder does not scratch glass. It is a nonconductor of electricity.

Heated in air.

When it has been dried only at 100° or 120°, it gives off moisture by increase of temperature; and, if heated in the atmosphere, takes fire at a temperature below the boiling point of olive oil, and burns with a red light and scintillations like charcoal.

Heated in vacuo.

If it be excluded from air and heated to whiteness in a tube of platina, exhausted after having been filled with hidrogen, it is found very little altered after the process. Its colour is a little darker, and it is rather denser; but no indications are given of any part of it having undergone fusion, volatilization, or decomposition. Before the process its specific gravity is such, that it does not sink in sulphuric acid; but after, it rapidly falls to the bottom in this fluid.

Its combustion in oxigen gas,

The phenomena of its combustion are best witnessed in a retort filled with oxigen gas. When the bottom of the retort is gently heated by a spirit lamp, it throws off most vivid scintillations like those from the combustion of the bark of charcoal, and the mass burns with a brilliant light. A sublimate rises from it, which is boracic acid; and it becomes coated with a vitreous substance, which proves likewise to be boracic acid; and after this has been washed off, the residuum appears perfectly black, and requires a higher temperature for its inflammation than the olive coloured substance; and by its inflammation produces a fresh portion of boracic acid.

and in oximuriatic acid gas.

In oximuriatic acid gas the peculiar inflammable substance

stance occasions some beautiful phenomena. When this gas is brought into contact with it at common temperatures, it instantly takes fire, and burns with a brilliant white light; a white substance coats the interior of the vessel in which the experiment is made, and the peculiar substance is found covered by a white film, which by washing affords boracic acid, and leaves a black matter, which is not spontaneously inflammable in a fresh portion of the gas; but which inflames in it by a gentle heat, and produces boracic acid.

Heated in hidrogen or nitrogen.

The peculiar inflammable substance, when heated nearly to redness in hidrogen, or nitrogen, did not seem to dissolve in these gasses, or to act upon them; it merely gained a darker shade of colour, and a little moisture rose from it, which condensed in the neck of the retort in which the experiment was made.

Its action on fluids containing oxigen:

On the fluid menstrua containing oxigen it produced effects, which might be looked for from the phenomena of its agency on gasses.

nitric acid,

When thrown into concentrated nitric acid, it rendered it bright red, so that nitrous gas was produced and absorbed; but it did not dissolve rapidly, till the acid was heated; when there was a considerable effervescence, the peculiar substance disappeared, nitrous gas was evolved, and the fluid afforded boracic acid.

sulphuric acid,

It did not act upon concentrated sulphuric acid, till heat was applied; it then produced a slight effervescence; the acid became black at its points of contact with the solid; and a deep brown solution was formed, which, when neutralized by potash, gave a black precipitate.

muriatic acid,

When heated in a strong solution of muriatic acid, it gave it a faint tint of green; but there was no vividness of action, or considerable solution.

& acetic acid.

On acetic acid heated it had no perceptible action.

It combined with fixed alkalis.

It combined with the fixed alkalis, both by fusion and aqueous solution, and formed pale olive coloured compounds, which gave dark precipitates when decomposed by muriatic acid.

Its action on sulphur,

When it was kept long in contact with sulphur in fusion, it slowly dissolved, and the sulphur acquired an olive tint.

phosphorus,

It was still less acted upon by phosphorus, and after an hour's

hour's exposure to it, had scarcely diminished in quantity, but the phosphorus had gained a tint of pale green.

It did not combine with mercury, when they were heated together. and mercury.

These circumstances are sufficient to show, that the combustible substance obtained from boracic acid by the agency of potassium is different from any other known species of matter; and it seems, as far as the evidence extends, to be the same as that procured from it by electricity; and the two series of facts seem fully to establish the decomposition, and recomposition of the acid. Differs from any known matter.

From the large quantity of potassium required to decompose a small quantity of the acid, it is evident that the boracic acid must contain a considerable proportion of oxigen. I have endeavoured to determine the relative weights of the peculiar inflammable matter and oxigen, which compose a given weight of boracic acid; and to this end I made several analytical and synthetical experiments; I shall give the results of the two, which I consider as most accurate. Boracic acid contains much oxigen.

Twenty grains of boracic acid and thirty grains of potassium, were made to act upon each other by heat in a tube of brass; the result did not effervesce when washed with diluted muriatic acid; and there were obtained after the process, by slight lixiviation in warm water, two grains and about six sixteenths of the olive coloured matter. Now thirty grains of potassium would require about five grains of oxigen, to form thirty-five of potash; and according to this estimation, boracic acid must consist of about one of the peculiar inflammable substance, to nearly two of oxigen. Apparently in one instance 2 p. oxigen to 1 base:

A grain of the inflammable substance in very fine powder, and diffused over a large surface, was set fire to in a retort, containing twelve cubical inches of oxigen; three cubical inches of gas were absorbed, and the black residuum, collected after the boracic acid had been dissolved, was found to equal five eighths of a grain. This, by a second combustion, was almost entirely converted into boracic acid, with the absorption of two cubical inches and one eighth more of oxigen. The thermometer in this experiment was at 58° Fahrenheit, and the barometer at 30·2. In another

 Accord-

1·8 oxigen to 1 base. Oxide with 24·8 per cent of oxigen.

According to this result, boracic acid would consist of one of the inflammable matter to about 1·8 of oxigen; and the dark residual substance, supposing it to be simply the inflammable matter combined with less oxigen than is sufficient to constitute boracic acid, would be an oxide, consisting of about 4·7 of inflammable matter to 1·55 of oxigen.

Sources of errour both in the analysis & synthses.

These estimations, I do not however venture to give as entirely correct. In the analytical experiments, there are probably sources of errour, from the solution of a part of the inflammable matter; and it possibly may retain alkali, which cannot be separated by the acid. In the synthetical process, in which washing is employed, and so small a quantity of matter used, the results are still less to be depended upon; they must be considered only as imperfect approximations.

Is the base of boracic acid simple or compounded?

From the general tenour of the facts it appears, that the combustible matter obtained from boracic acid bears the same relation to that substance, as sulphur and phosphorus do to the sulphuric and phosphoric acids. But is it an elementary inflammable body, the pure basis of the acid? or is it not, like sulphur and phosphorus, compounded?

The dark olive substance most probably a compound.

Without entering into any discussion concerning ultimate elementary matter, there are many circumstances, which favour the idea, that the dark olive substance is not a simple body; its being nonconducting, its change of colour by being heated in hydrogen gas, and its power of combining with the alkalis; for these properties in general belong to primary compounds, that are known to contain oxigen.

Heated with potassium.

I heated the olive coloured substance with potassium, there was a combination, but without any luminous appearance, and a gray metallic mass was formed; but from the effect of this upon water I could not affirm, that any oxigen had been added to the metal, the gas given off had a peculiar smell, and took up more oxigen by detonation than pure hidrogen, from which it seems probable, that it held some of the combustible matter in solution.

Exposed to the action of potassium in ether,

It occured to me, that, if the pure inflammable basis were capable of being deoxigenated by potassium, it would probably possess a stronger affinity for oxigen than hidrogen, and therefore be again brought to its former state by water.

I made

I made another experiment on the operation of potassium on the olive coloured substance, and exposed the mixture to a small quantity of ether, hoping that this might contain only water enough to oxigenate the potassium; but the same result occurred as in the last case; and a combination of potash and the olive coloured substance was produced, insoluble in ether.

I covered a small globule of potassium with four or five times its weight of the olive coloured matter, in a platina tube exhausted, after being filled with hidrogen; and heated the mixture to whiteness: no gas was evolved. When the tube was cooled, naphtha was poured into it, and the result examined under naphtha. Its colour was of a dense black. It had a lustre scarcely inferior to that of plumbago. It was a conductor of electricity. A portion of it thrown into water occasioned a slight effervescence; and the solid matter, separated, appeared dark olive, and the water became slightly alkaline. Another portion examined, after being exposed to air for a few minutes, had lost its conducting power, was brown on the surface, and no longer produced an effervescence in water. in vacuo,

Some of the olive inflammable matter, with a little potassium, was heated to whiteness, covered with iron filings, a dark metalline mass was formed, which conducted electricity, and which produced a very slight effervescence in water, and gave by solution in nitric acid, oxide of iron and boracic acid. and in contact with iron filings.

The substance which enters into alloy with potassium, and with iron, I am inclined to consider as the true basis of the boracic acid. True basis of the acid.

In the olive coloured matter this basis seems to exist in union with a little oxigen; and when the olive coloured substance is dried at common temperatures, it likewise contains water. Olive coloured matter.

In the black nonconducting matter, produced in the combustion of the olive coloured substance, the basis is evidently combined with much more oxigen; and in its full state of oxigenation it constitutes boracic acid. Black matter.

From the colour of the oxides, and their solubility in alkalis, from their general powers of combination, and from the conducting The boracic basis probably metallic.

conducting nature and lustre of the matter produced by the action of a small quantity of potassium upon the olive coloured substance, and from all analogy; there is strong reason to consider the boracic basis as metallic in its nature, and I venture to propose for it the name of *boracium*.

7. *Analytical Inquiries respecting Fluoric Acid.*

First experiments on fluoric acid gas. I have already laid before the Society the account of my first experiments on the action of potassium on fluoric acid gas*.

I stated, that the metal burns when heated in this elastic fluid, and that there is a great absorption of the gas.

Since the time that this communication was made, I have carried on various processes, with the view of ascertaining accurately the products of combustion, and I shall now describe their results.

Fluoric acid gas introduced to potassium, When fluoric acid gas, that has been procured in contact with glass, is introduced into a plate glass retort, exhausted after being filled with hidrogen gas, and containing potassium, white fumes are immediately perceived. The metal loses its splendour, and becomes covered with a grayish crust.

and heated. When the bottom of the retort is gently heated, the fumes become more copious; they continue for some time to be emitted, but at last cease altogether.

An addition of hidrogen to the gas. If the gas is examined at this time, its volume is found to be a little increased, by the addition of a small quantity of hidrogen.

Second application of heat, and temperature raised. No new fumes are produced by a second application of a low heat; but when the temperature is raised nearly to the point of sublimation of potassium, the metal rises through the crust, becomes first of a copper colour and then of a bluish black, and soon after inflames and burns with a most brilliant red light.

* Phil. Trans., Part II, 1808, p. 343; [Journal vol. XXI, p. 375.] The combustion of potassium in fluoric acid I have since seen mentioned in the number of the Moniteur, already so often quoted, as observed by M. M. Gay Lussac and Thenard; but no notice is taken of the results. [They are given in our present number, p. 29.]

After

After this combustion, either the whole or a part of the fluoric acid, according as the quantity of potassium is great or small, is found to be destroyed or absorbed. A mass of a chocolate colour remains at the bottom of the retort; and a sublimate, in some parts chocolate, and in others yellow, is found round the sides, and at the top of the retort. *Product of the combustion.*

When the residual gas afforded by this operation is washed with water, and exposed to the action of an electrical spark mixed with oxigen gas, it detonates and affords a diminution, such as might be expected from hidrogen gas. *Residual gas, hidrogen.*

The proportional quantity of this elastic fluid differs a little in different operations. When the fluoric acid has not been artificially dried, it amounts to one sixth or one seventh of the volume of the acid gas used; but when the fluoric acid has been long exposed to calcined sulphate of soda, it seldom amounts to one tenth. *Its proportion.*

I have endeavoured to collect large quantities of the chocolate coloured substance for minute examination; but some difficulties occurred. *Attempt to collect large quantities of the product.*

When I used from eighteen to twenty grains of potassium, in a retort containing from twenty to thirty cubical inches of fluoric acid gas, the intensity of the heat was such as to fuse the bottom of the retort, and destroy the results.

In a very thick plate glass retort, containing about nineteen cubical inches of gas, I once succeeded in making a decisive experiment on ten grains and a half of potassium, and I found, that about fourteen cubical inches of fluoric acid disappeared, and about two and a quarter of hidrogen gas were evolved. The barometer stood at 30·3, and the thermometer at 61° Fahrenheit; the gas had not been artifically dried. In this experiment there was very little sublimate; but the whole of the bottom of the retort was covered with a brown crust, and near the point of contact with the bottom, the substance was darker coloured, and approaching in its tint to black. *Successful one.*

When the product was examined by a magnifier, it evidently appeared consisting of different kinds of matter: a blackish substance, a white, apparently saline substance, and a substance having different shades of brown and fawn colour. *Product a compound.*

The

A nonconductor.

The mass did not conduct electricity, and none of its parts could be separated, so as to examine as to this property.

Action on water.

When a portion of it was thrown into water, it effervesced violently, and the gas evolved had some resemblance in smell to phosphuretted hidrogen, and was inflammable.

Heated in contact with air,

When a part of the mass was heated in contact with air, it burnt slowly, lost its brown colour, and became a white saline mass.

and in oxigen.

When heated in oxigen gas in a retort of plate glass, it absorbed a portion of oxigen, but burnt with difficulty, and required to be heated nearly to redness; and the light given out was similar to that produced by the combustion of liver of sulphur.

Examination of the water,

The water which had acted upon a portion of it was examined; a number of chocolate coloured particles floated in it. When the solid matter was separated by the filter, the fluid was found to contain fluate of potash, and potash.

and of the residuum.

The solid residuum was heated in a small glass retort in oxigen gas; it burnt before it had attained a red heat, and became white. In this process oxigen was absorbed, and acid matter produced. The remainder possessed the properties of the substance formed from fluoric acid gas holding siliceous earth in solution, by the action of water.

Experiments on small quantities not decisive as to the pure basis.

In experiments made upon the combustion of quantities of potassium equal to from six to eleven grains, the portion of matter separable from the water has amounted to a very small part of a grain only; and operating upon so minute a scale, I have not been able to gain fully decided evidence, that the inflammable part of it is the pure basis of the fluoric acid; but with respect to the decomposition of this body by potassium, and the existence of its basis at least combined with a smaller proportion of oxigen in the solid product generated, and the regeneration of the acid by the ignition of the product in oxigen gas, it is scarcely possible to entertain a doubt.

Decomposition of the fluoric acid analogous to that of the

The decomposition of the fluoric acid by potassium seems analogous to that of the acids of sulphur and phosphorus. In neither of these cases is the pure basis, or even the basis in

in its common form, evolved; but new compounds result, and in one case sulphurets, and sulphites, and in the other phosphurets, and phosphites of potash, are generated. sulphuric and phosphoric.

As silex was always obtained during the combustion of the chocolate coloured substance obtained by lixiviation, it occurred to me, that this matter might be a result of the operation, and that the chocolate substance might be a compound of the siliceous and fluoric basis in a low state of oxigenation with potash; and this idea is favoured by some trials that I made to separate silex from the mass, by boiling it in concentrated fluoric acid; the substance did not seem to be much altered by the process, and still gave silex by combustion. Silex always obtained: perhaps a result of the operation.

I endeavoured to decompose fluoric acid gas in a perfectly dry state, and which contained no siliceous earth; and for this purpose I made a mixture of one hundred grains of dry boracic acid, and two hundred grains of fluor spar, and placed them in the bottom of an iron tube, having a stopcock and a tube of safety attached to it. Fluoric acid decomposed by the boracic,

The tube was inserted horizontally in a forge, and twenty grains of potassium, in a proper iron tray, introduced into that part of it where the heat was only suffered to rise to dull redness. The bottom of the tube was heated to whiteness, and the acid acted upon by the heated potassium, as it was generated. After the process was finished, the result in the tray was examined. and made to act on potassium as it was generated.

It was in some parts black, and in others of a dark brown. It did not effervesce with water: and when lixiviated, afforded a dark brown combustible mass, which did not conduct electricity, and which, when burnt in oxigen gas, afforded boracic and fluoric acid. It dissolved with violent effervescence in nitric acid; but did not inflame spontaneously in oximuriatic acid gas. Product.

I have not as yet examined any of the other properties of this substance; but I am inclined to consider it as a compound of the olive coloured oxide of boracium, and an oxide of the fluoric basis. A compound of the boracic and fluoric oxides.

In examining the dry fluoric acid gas, procured in a process similar to that which has been just described, it gave very evident marks of the presence of boracic acid. The dry gas contains boracic acid.

As

Attempt to obtain the chocolate coloured substance

As the chocolate coloured substance is permanent in water, it occurred to me, that it might possibly be producible from concentrated liquid fluoric acid at the negative surface in the Voltaic circuit.

from the acid of fluor spar expelled by the sulphuric in leaden vessels.

I made the experiment with platina surfaces, from a battery of two hundred and fifty plates of six inches, on fluoric acid the densest that could be obtained by the distillation of fluor spar and concentrated sulphuric acid of commerce in vessels of lead. Oxigen and hidrogen were evolved, and a dark brown matter separated at the deoxidating surface; but the result of an operation conducted for many hours merely enabled me to ascertain, that it was combustible, and produced acid matter in combustion; but I cannot venture to draw the conclusion, that this acid was fluoric acid, and it was not impossible, that some sulphurous or sulphuric acid might likewise exist in the solution.

Olive coloured substance from boracic acid heated in fluoric gas.

I heated the olive coloured inflammable substance, obtained from the boracic acid, in common fluoric acid gas in a plate glass retort; the temperature was raised till the glass began to fuse; but no change, indicating a decomposition, took place.

Potassium and fluor spar heated in hidrogen.

I heated six grains of potassium with four grains of powdered fluor spar in a green glass tube filled with hidrogen; there was a slight ignition, a minute quantity of hidrogen gas was evolved, and a dark gray mass was produced, which acted upon water with much effervescence, but left no solid inflammable residuum.

(To be concluded in our next.)

III.

Remarks on the Boracic Acid, addressed to the first Class of the Instisute, December the 19th, 1809 by F. R. CURAUDAU, *Professor of Chemistry applicable to the Arts, and Member of several literary Societies**.

If boracic acid be decomposed by potassium,

THE process, by which Messrs. Thenard and Gay-Lussac have announced, that they decomposed the boracic acid, though

* Journal de Physique, March, 1809, p. 256.

though the same as they made known the 21st of June last, has acquired a fresh interest, from the explanation they have given of the phenomena, that take place during the experiment*. In fact, if, agreeably to these chemists, boracic acid be decomposed by the alkaline metals, and lose its acid properties by the subtraction of the oxigen, which is admitted to enter into its composition, this conclusion must be formed, that potash is an oxigenized substance; and that the alkaline metals are not, as I think I have proved, a compound of alkali with hidrogen and carbon, or, if you please, with hidrogen solely. We must equally infer, that the silex, with which I have shown the alkaline metals may readily be decomposed†, is likewise an oxigenizing substance, which, instead of being the instrument of a decomposition, is decomposed itself. These points at least follow from the explanation they have given of the decomposition of the boracic acid. In this point of view however my experiment of the decomposition of the alkaline metals by means of silex is interesting, since it would prove this substance to be an oxide.

this is an oxide and not a compound of potash, hidrogen, and carbon: and silex is an oxigenizing substance.

However, as in admitting such an hypothesis we cannot explain all the phenomena observed during the decomposition of the boracic acid by means of the alkaline metals, I conceived it would not be amiss to make some fresh experiments on this subject; in order to ascertain on the one hand, whether it were true, that the boracic acid is an oxigenized substance; and to discover on the other, if possible, what became of the hidrogen and carbon of the alkaline metal, which disappear in this experiment, without Thenard and Gay Lussac having told us any thing of what they conjecture in this respect. The result of my labours I now submit to the examination of the class, hoping, that it will perceive in my zeal no other motive, than that of paying a fresh tribute to science.

This hypothesis does not explain all the phenomena.

Among the experiments I have attempted, the following particularly attracted my attention.

As boracic acid readily decomposes the metal of potash,

Boracic acid

* See Journal, vol. XXIII, p. 260.

† See Art. VI of our present number.

I thought

should prevent the formation of potassium.

I thought, that, by adding this acid to a mixture capable of producing the metal of potash, it would not only prevent its production, but likewise be converted into the new substance obtained by decomposing the metal of potash by boracic acid.

It does so.

To prove how far this conjecture was well founded, I introduced into a gunbarrel the result of the detonation of six parts of vegetable charcoal, four of refined borax, and two of nitrate of potash. I afterward tried to extract the alkaline metal, but, as I had foreseen, no metal was disengaged.

The product lixiviated

When the matter was cold, I lixiviated it with a sufficient quantity of boiling water, in order to take up all the soluble substances it might contain. I afterward evaporated the solution till it was completely concentrated, and let it cool: Having obtained from the liquor only part of the borax I had employed; and the liquid itself, after having been slightly acidulated, yielding me but very little boracic acid; I concluded, that the surplus of this acid had remained combined with the charcoal, and must be in the state, in which Messrs. Thenard and Gay-Lussac found that, which they had treated with the metal of potash. What seemed still to support this opinion was, I observed the residuum of the calcination was of a black bottle green.

does not yield the whole of the borax, part of its acid having combined with charcoal.

The insoluble residuum treated with nitric acid,

To satisfy myself whether in fact the boracic acid were contained in the insoluble residuum, I poured on the coal, while still wet, a certain quantity of nitric acid. I afterward subjected the mixture to the action of a gentle heat. A brisk effervescence soon took place, which I ascribed to the oxigenation of the substance, that has been designated by the name of *bore**, which resumed its former state.

yielded the remaining proportion of boracic acid.

When the acid ceased to act on the residuum, I lixiviated the mixture. The liquors I obtained being afterward suitably evaporated, I obtained by cooling the remainder of the boracic acid, which was nearly the whole contained in the borax I had employed.

This experiment does not prove the boracic acid to

This experiment, which I have repeated several times with the same success, though it has a great analogy with that, in which the alkaline metal is made to act immediately on the

* Journal, vol. XXIII, p. 262.

ɔoracic acid, is far from proving however, as has been contain oxi-
·ted, that this acid is an oxigenized body. In fact, if it gen.
', why, when treated alone with charcoal, does it not ex-
ence the same decomposition, as when it is combined
an alkali? How too can the alkali, which according to
ypothesis is itself an oxide, promote the disoxigenation
nother oxide? Should it not be on the contrary, from
ery nature of my experiment, an obstacle so much the
ter to the decomposition of the boracic acid; as all the
when combined with a base are less adapted for de-
position? This experiment then must afford an instance
n anomaly so much the more striking, if it were to the
en that we must ascribe the action of the boracic acid
e metal of potash. It would equally involve a mani-
contradiction, if we were to admit, that the new sub-
e, into which the boracic acid is transformed, is more
le than the acid was, before its state was changed.

'e see then, that the experiment of the decomposition The substance
orate of soda by means of carbon is far from proving, called *bore* not
the new substance, which Messrs. Thenard and Gay- the boracic
ac obtained from boracic acid, is the radical of that radical,

We see too, it proves still less, that oxigen is one of and the alkalis
constituent principles of the alkalis, as the celebrated not oxides.
lish chemist, Davy, continues to believe.

hus the experiments, from which I think I have demon- The new me-
ed, that the alkaline metals are nothing but a compound tals compounds
e alkali with hidrogen and carbon, acquire fresh force; of hidrogen,
so that the facts, which would seem to be adverse to carbon, and al-
, on the contrary confirm the deductions I have made kali.
them.

r instance, does not the decomposition of the alkaline Their decom-
ls by boracic acid, instead of proving, that the pheno- position by bo-
a observed during this experiment are to be ascribed to racic acid
en, on the contrary show, that this principle acts no proves these.
in it? and that it is rather a supracomposition of the
cic acid, than the loss of one of its principles, which oc-
ns the new properties it acquires?

owever, if this supracomposition of the boracic acid be *Bore* a supra-
admitted, how shall we explain why there is no hidro- compound.
or next to none, disengaged during the decomposition

of

of the alkaline metals by this acid? How too happens it on the hypothesis of it disoxigenation, that no water is produced? Can we admit the disoxigenation of one substance, and the dishidrogenation of another at the same time, without producing water sufficient to be collected, and have its weight calculated? Undoubtedly not. Thus, were this the only objection to the decomposition of boracic acid, it would suffice to prove, that the new state, in which this acid is obtained, is not owing to its disoxigenation. But as there are still many other objections, which the philosophy of the science suggests, we cannot do otherwise than consider the new substance, into which the boracic acid is converted, as a combination of this acid with the hidrogen and the carbon, that it has taken from the alkaline metal.

This accounts for the hidrogen appearing in no form.

According to this theory we find no difficuly in explaining, why, during the action of the boracic acid on the metal of potash, neither water nor hidrogen is disengaged; while on the hypothesis of the disoxigenation of this acid, we know not what becomes of the hidrogen, which the alkaline metal must necessarily lose.

This explanation, independently of its accounting for all the phenomena, has the farther advantage of leading to a more simple definition of an important point in chemistry, on which the opinion of chemists is not yet thoroughly fixed.

Phenomena of the combustion of *bore*.

With respect to the phenomena exhibited by the combustion of the substance, that produces the boracic acid, they are owing to the oxigenation of the hidrogen and carbon, which this acid had abstracted from the metal of potash; so that by the subtraction of these two principles it becomes boracic acid again, as by the same subtraction the alkaline metal had again become an alkali.

Hidrogen and carbon more dense in *bore* than in potassium.

If we consider too, that hidrogen and carbon, in their state of combination with boracic acid, are less oxigenizable than they were when combined with the alkali, every thing leads us to believe, that this arises from the two principles having acquired a fresh degree of condensation at the instant of their union with the boracic acid: and what appears to give some foundation to this conjecture is, that, at the moment when the combination takes place, the matter instantaneously

instantaneously becomes incandescent, a state that announces a great emission of caloric, and consequently a sudden condensation of some principles.

I shall not terminate this note, without imparting to the institute a fact, that appears to me very important, but from which I shall refrain from drawing any inference. It is as follows.

Metallic globules produced in a mixture of charcoal and borax,

I have observed, that in several experiments I made to decompose borate of soda by means of charcoal, metallic globules were produced, which appeared to be formed in the midst of the mixture: but as I found, that this metallic product was of the same nature as the vessel in which I made my experiments, I intend to repeat them in a tube of platina, in order to ascertain, whether those of iron, which I employed, did not concur in the formation of the metallic globules I obtained.

and in other cases.

However, this is not the only occasion, on which I have found similar globules. I had before remarked them in the mixtures I had made for the purpose of producing the alkaline metals with charcoal.

IV.

Abstract of a Paper on the Decomposition and Properties of the Fluoric Acid, presented the 9th of January to the Mathematical Class of the Institute, by Messrs. GAY-LUSSAC *and* THENARD*.

Potassium applied to the decomposition of fluoric acid.

MESSRS. Gay-Lussac and Thenard, having decomposed the boracic acid by means of the metal of potash†, could not but try the same method of decomposing the fluoric and muriatic acids, the constituent principles of which were not yet known. This they have effected with respect to the fluoric acid, and they now make public the principal results of their labours.

* Journal de Physique, January 1809, p. 95. For Mr. Davy's experiments on the decomposition of fluoric acid, see p. 20, of our present number.

† See Journal, Vol. XXIII, p. 260.

Our

Attempts to procure pure fluoric acid.

Our first care, say they, was to obtain pure fluoric acid: but as this acid exists only combined with lime, and no one has yet been able to separate it, without its entering into combination with some other body, we were obliged to make a great number of trials, that procured to us the advantage of observing several facts, the most remarkable of which are the following.

Gas procured from fluate of lime and boracic acid produces vapour with all gasses containing water.

When air is placed in contact with the fluoric gas disengaged from a redhot iron tube containing fluate of lime and glacial boracic acid, vapours are formed as dense as those arising from muriatic acid gas and ammonical gas. It produces the same with all the other gasses, except the muriatic acid gas, provided those gasses have not been dried. But it does not alter the transparency of any of them, if they have remained some time in contact with lime, or muriate of lime. In the first case, where there is a production of strong vapours, the volume of gas diminishes equally, and only a few hundredths at the temperature of 7° [44·6° F.]. In the second case, where the gasses retain their transparency, their bulk does not alter. Hence we may infer, that fluoric acid gas is an excellent mean for indicating the presence of hygrometrical water in gasses; and that all contain some, except the muriatic acid gas, fluoric gas, and probably ammoniacal gas.

No water precipitated from it by cold.

For this reason, if we expose fluoric gas to a cold of 15° or 19° [5° above or 2·2° below 0 F.], we find no trace of liquid separated; while on exposing sulphurous acid gas, carbonic acid gas, &c., to the same degree of cold, water is suddenly deposited.

It has a great affinity for water.

The dense vapours, produced by fluoric gas in the gasses that contain hygrometrical water, announce in it a great affinity for this fluid: and indeed it is no exaggeration to say, that water can absorb more of it than of muriatic acid gas, and probably more than two thousand times its bulk.

Properties of water saturated with it.

When water is thus saturated with it, it is limpid, fuming, and exceedingly caustic. About a fifth part of what it contains may be abstracted from it by heat; but, do what we will, it is impossible to get more. It then resembles concentrated sulphuric acid: it has its causticity and appearance: like it its boiling point is much above that of water, and it condenses entirely in striæ, though it contains still

still perhaps sixteen hundred times its bulk of gas. Is it not hence extremely probable, if not even demonstrated, that the sulphuric and nitric acids would be in the state of gas, if they were pure? and that they are indebted for the liquid state, in which we see them, to the water they contain?

Sulphuric and nitric acids always contain water.

Though our fluoric gas has a great affinity for water, and contains none, since it is obtained from matters perfectly dry, &c.; yet it cannot dissolve or convert into gas the smallest quantity. We kept a quart of fluoric gas in contact with a single drop of water over mercury for several hours; and this drop, instead of disappearing, increased in size. Hence it is proved, that this gas cannot contain water in any manner, either in the hygrometrical state, or in a state of combination. Ammoniacal gas is precisely in the same situation, at least with respect to combined water. But it is not the same with muriatic acid gas: this it is true contains no hygrometrical water, but it contains water intimately combined, as Messrs. Heuri and Berthollet first showed. By passing muriatic gas, in a gentle heat, through litharge, melted and reduced to a coarse powder, we have accomplished the extraction of this water, and caused it to appear in streams. From the experiments we have made on the direct combination of a certain quantity of this acid with an excess of oxide of silver, it must form about a fourth of its weight.

Fluoric gas cannot dissolve water.

Ammoniacal gas similar.

Muriatic acid gas contains water in combination.

The other gasses do not comport themselves with water like the preceding. No one contains combined water, but all contain hygrometrical water. Hence it follows, that fluoric acid gas and ammoniacal gas contain neither hygrometrical water, nor combined water*: that muriatic acid gas contains no hygrometrical water, but does contain combined water; and that all the other gasses contain only hygrometrical water.

All other gasses contain water uncombined.

What is most striking in these results is to see, that muriatic acid gas contains water, and that the fluoric and am-

Proportions of water in muriatic acid gas,

* It is certain, that, from the experiments of Mr. Berthollet jun. ammoniacal gas contains no combined water; but Gay-Lussac and Thenard do not yet venture to affirm, that it contains no water in the hygrometrical state.

moniacal

moniacal gasses contain none; and particularly to find, that the muriatic acid gas contains it in such proportions, that, if it were entirely decomposed by a metal, all the acid would be absorbed by the oxide, and converted into a metallic muriate. This, as we have satisfied ourselves, takes place, when muriatic acid is gradually and successively passed through several redhot gun barrels filled with iron turnings.

Or rather the elements of water.

The more we reflect on all these phenomena, the more difficult we find it to account for them. Is it not possible however, that oxigen and hidrogen may be two of the constituent principles of muriatic acid, and that they are not in the state of water in it, but that this is formed when the acid enters into combination with bodies, so that in the muriates it is quite different from what it is in the state of gas? Be this as it may, it is certain, that all the muriates indecomposable by fire, and which contain little or no water, cannot be decomposed, even at a very high temperature, either by the vitreous acid phosphate of lime, or by the boracic acid; that thus the acid is retained with very great force in the muriates; and that sulphuric acid itself, if deprived of water, very probably could not decompose them. But we will quit this hypothesis, and return to an examination of the properties of our fluoric gas.

Action of fluoric gas on vegetable matter.

We have considered already its physical properties, its action on the air, on all the gasses, and on water. Let us now consider how it acts on vegetables matters. These it attacks at least as powerfully as the sulphuric acid; and, like this acid, appears to act on them by occasioning water to be formed, for it chars them. Thus it readily converts alcohol into an ether, which we purpose to investigate; and instantly blacken the driest paper, diffusing a vapour, which is owing to the water that is formed and absorbs it.

A very potent acid.

Every thing then demonstrates to us, that this fluoric gas is one of the most powerful acids, and that it is not inferior in strength and causticity to concentrated sulphuric acid;

But it was a compound of the fluoric and boracic acids.

yet it has no action on glass. Hitherto we had supposed, that it was pure: but then we suspected that it contained something, which prevented its action on silex; and in fact

soon

soon found, that it held in solution a pretty large quantity of boracic acid.

The fluoric acid arising from the decomposition of fluate of lime by boracic acid not being pure, we attempted to prepare it by decomposing this salt by the acid phosphate of lime. We obtained but very little; and what we did obtain contained in the first place the small quantity of silex, that existed in our fluate of lime, and secondly a certain portion of the acid phosphate of lime itself. What is remarkable in this process is, that, when we used a siliceous fluate of lime, the decomposition of the salt was very rapid, in consequence of the action of the silex on fluoric acid, and gave rise to a great deal of siliceous fluoric gas.

Fluate of lime and acid phosphate of lime yielded a fluoric acid gas with silex in it.

Considering then, that the fluoric gas arising from the fluate of lime and boracic acid contained no water, and was not capable of dissolving any, we thought, contrary to the generally received opinion, that the case would probably be the same with that prepared in leaden vessels by means of concentrated sulphuric acid.

Fluate of lime decomposed by sulphuric acid in leaden vessels

But instead of obtaining the acid in the state of gas by this means, we had it in a liquid state, and possessing the following properties. In the air it emits dense vapours: with water it heats and even enters suddenly into ebullition: it scarcely comes into contact with glass before it destroys its polish, heats strongly, boils, and is converted into siliceous gas. Of all its properties the most extraordinary is its action on the skin. It scarcely touches this when it disorganizes it. A white spot is immediately seen, a great pain is soon felt: the parts adjacent to the point touched speedily grow white and painful; and in a little time a blister is formed, covered by a thick white skin, and containing matter.

yielded only a liquid acid.

Its properties.

Singular action on the skin.

However small the quantity the phenomena equally take place; only they proceed more slowly, so that sometimes they are not observed till seven or eight hours after the contact; and still the burn will be sufficiently severe, to cause acute pain, deprive the patient of sleep, and excite fever. The effects of these burns, as we are convinced by our own experience, may be stopped by the immediate application of a weak solution of caustic potash; which we know too, by

Remedy against burns.

 expe-

experience, to be an excellent remedy against common burns.

Action of this liquid on potash.

It may readily be supposed, that we did not neglect to place such an active liquid in contact with the metal of potash. This experiment was made in a copper tube. At first we threw a piece the size of a small hazel nut into a small quantity of this liquid; and immediately a very loud detonation ensued, with a great evolution of light and heat. Afterward, desirous of knowing what was the cause of these phenomena, we caused the fluid to arrive at the metal gradually. In this way but little heat is produced, and we could collect the products of the experiment. These products were hidrogen, fluate of potash, and water. Consequently this active liquid is a compound of fluoric acid and water.

It combines with all substances, and is the strongest of acids.

We see then, that this acid tends to combine with all substances, and that it forms with them solid, liquid, or gaseous compounds, according as it retains more or less elasticity, or expansive force. It is the only acid with which this is the case: and this property is even a proof, that it is the strongest and most active of acids.

Fluoric acid not obtainable pure.

Since we cannot in any way obtain fluoric acid pure, we can only study it when in combination with some substance. We must take it then combined with this or that substance, according to the result we wish to obtain.

Siliceous fluoric acid forms triple salts with alkalis, earths, and oxides.

If the object be to unite it with alkalis, earths, or metallic oxides, we must be careful not to employ siliceous fluoric acid, for in this case we should obtain triple salts. Thus, on pouring ammonia into acid fluate of silex, we obtain a triple salt nearly insoluble, yet in great measure volatile. Thus too, on pouring muriate of barytes into acid fluate of silex, we obtain after some time a crystalline precipitate, insoluble in a great excess of nitric acid, which might be mistaken for sulphate of barytes, and is nothing but fluate of silex and barytes.

For decomposition the gas should be employed.

But when, instead of wanting to combine fluoric acid with these substances, we wish to decompose it, as we purposed to do by means of the metal of potash, it is evident, that we ought not to employ liquid fluoric acid, on account of the water present with it; and that we should prefer, either

ther the fluoric gas holding in solution boracic acid; or rather the siliceous fluoric gas, because the foreign matter in this, containing nothing combustible, cannot lead us into errour, and can be of no injury farther than giving an addition of this matter. Accordingly we employed these gasses, and chiefly the siliceous fluoric gas, in our experiments on the decomposition of the fluoric acid, of which we shall now proceed to give an account.

Action of potassium on siliceous fluoric gas.

When the metal of potash is placed in contact with siliceous fluoric gas at the common temperature, it undergoes no perceptible alteration, except becoming slightly dull on the surface: but if it be melted, it soon thickens, and burns vividly, with the extrication of much heat and light. In this combustion there is a great absorption of fluoric acid, very little hidrogen gas is disengaged, the metal disappears, and a solid substance of a reddish brown colour is produced.

The product treated with water.

If this substance be treated with cold water, hidrogen gas is evolved, though it appears no longer to contain any metal. If, after having treated it with cold water, it is treated with hot, more hidrogen gas is evolved, but less than the first time; and on the whole scarcely a third as much as the metal itself would yield with water is obtained. If the waters of elutriation be added together and evaporated, we obtain from them nothing but fluate of potash with excess of alkali; and if we examine the residuum, which, when well washed, is still of a reddish brown colour, we find it to possess the following properties. When thrown into a silver crucible at a cherry red heat, it burns vividly, and disengages a little acid gas; after which, from being insoluble in water, it becomes partly soluble. The portion that dissolves is fluate of potash; that which does not dissolve is siliceous fluate of potash.

Residuum burned in air,

and in oxigen gas.

If, instead of making the experiment in a crucible, it be done in a small bent glass jar filled with oxigen gas, and heated gradually, the inflammation is more vivid than in common air, a great quantity of oxigen is absorbed, and the gas that remains after the combustion is nothing but pure oxigen, with the addition of a little fluoric acid. The

 product

product is solid, as in the preceding experiment, and is formed of fluate of potash and silex.

The fluoric gas is either decomposed, or combines with potassium without oxiding it.

It is now evident, that, since little or no hidrogen gas is evolved on burning the metal of potash in fluoric acid gas, this combustion cannot be ascribed to water. Hence in this experiment either the fluoric acid is decomposed, or it combines with the metal without oxiding it. These two hypotheses being the only ones that can be formed, let us discuss them in succession. If it were the metal, that combined entire with the fluoric acid, the probable result would be a very inflammable compound, which with water would give out as much hidrogen as the metal itself. But we obtain only a third of what ought to be evolved. Besides, a combination of this kind is contrary to all the facts on all possible suppositions; whether we consider the action of the fluoric acid on the metals and alkalis, or the action of the metal of potash on all the other acids. Hence we must conclude, that the fluoric acid is probably decomposed.

It is probably decomposed.

Consequently in this decomposition must be formed a compound of the fluoric radical with potash and silex. It appears, that, when this radical is combined only with potash, it is capable of decomposing water like the phosphurets; but that, when it is combined with potash and silex, it does not decompose it, no doubt because this triple compound is insoluble.

Potassium easily burned in fluoric gas in small quantities,

Be this as it may, it is extremely easy, to effect the combustion of the metal of potash in fluoric gas. When we would burn only a small quantity of the metal, the operation may be performed very conveniently over mercury in a little glass vessel blown by a lamp, to the top of which the metal is conveyed on an iron rod, and which is heated by a burning coal till the inflammation commences.

or in large.

But if we would burn large quantities of the metal, the operation should be performed in a jar holding about a quart. This is first to be filled to within two fingers breadths with fluoric acid gas. The metal is then to be conveyed into it by means of an iron wire properly bent. A small capsule, which may be made of a crucible by removing a portion of the sides, being heated to a cherry red, is then to be introduced, holding it in a pair of tongs; and when

when it is emptied of the mercury by shaking it, the metal of potash is immediately to be placed in it, and it will presently burn with great force. The combustion being finished, and the capsule cooled, it is to be taken out, and the matter separated with a small spatula. This done, another portion of metal may be burned in this little capsule in the same jar; provided a quantity of fluoric acid, equal to what was absorbed in the first combustion, be passed up into it. A third and a fourth combustion may be accomplished in the same way. There is nothing to prevent this, since the jar may always be kept equally full of fluoric acid gas, and the metal is easily procured at pleasure, by following the process we have recommended. We will add, however, that for the complete success of these experiments, great care must be taken, to remove the oil from the surface of the metal with blotting paper; otherwise it will be decomposed, and give out a little hidrogen gas and carbon. In fact this inconvenience cannot be entirely avoided; and whatever precaution be taken, there is always a portion of oil interposed between the particles of the metal: but the quantity is so small, that it need not be regarded, and cannot be the source of any errour in the results. To this oil is owing the property of rendering lime-water turbid, that the metals of potash and soda sometimes possess.

Care must be taken to free the potassium from oil.

V.

Description of a Process, by means of which Potash and Soda may be metallized without the Assistance of Iron; read before the French Institute the 18*th of April,* 1808; *by* F. R. CURAUDAU*.

THE decomposition of the alkalis, which I never considered as simple bodies, having long been an object of research with me, I was eager to repeat the experiment, in

Alkalis long supposed to be compounds.

* Journal de Physique, April 1808, p. 320.

Their metallization by means of iron does not always succeed.

which Messrs. Thenard and Gay-Lussac announced potash and soda could be converted into metals by means of iron. Not having obtained more satisfactory results however than others, whom I have known to repeat the same experiments, I thought it right to pursue the researches I had already begun on the same subject, and the success of which appeared to me the more certain, as already the beautiful experiments of Mr. Davy had thrown great light on some phenomena, which I had observed, but which I could not before explain.

Is the prussiate of potash a compound of the metal with carbon?

In fact, if, according to the hypothesis of the celebrated English chemist, potash and soda be metallic oxides, is it not more than probable, that the prussic calcinations are simply the combination of this metal with charcoal? Such at least was my opinion at that time; and it will appear how far it was well founded, since I have accomplished the metallization of potash and of soda, by heating strongly the alkali with charcoal, a process which, it is obvious, ranks among the prussic calcinations.

The metal of the fixed alkalis obtainable by two processes.

The metallization of potash or soda taking place with either of the two mixtures I shall mention, and succeeding as well in stone retorts as in iron tubes, the first or second process may be employed indifferently. As to the nature of the vessel, I prefer iron, because it is more permeable to caloric, and less subject to fusion than the stone ware, particularly when the latter is penetrated with alkali; an inconvenience, that prevents the operation from being continued to the end, which does not happen so frequently with iron.

Process the first.

1st process.

Mix intimately four parts of animal charcoal well powdered with three of carbonate of soda, dried on the fire without having been fused; and mix the whole with a sufficient quantity of linseed oil, but not so as to form a paste.

Process the second.

2d process.

Take two parts of flour, and mix them intimately with one part of carbonate of soda prepared as in the preceding process,

process, and add to this mixture as much linseed oil as it will bear without ceasing to be pulverulent.

Manipulation.

Whatever be the kind of vessel employed to calcine this matter, and whether it be the first or second mixture, we must always begin with heating it gradually: but as soon as the matter is obscurely red, the fire may be increased, till a fine sky blue light, surrounded with a greenish aureola, is perceived in the interior of the retort or iron tube. To this light will soon succeed a very copious vapour, which obscures all the interior of the vessel. This is the metal, which is disengaged from the mixture. The fire must then be urged no farther, for at this temperature the retort begins to fuse; and if the iron resist better, it is because the alkali penetrates it less readily than it does the stone ware, and likewise because the heat it receives is sooner transmitted to the matter within.

Collection of the metal.

To collect the metal in proportion as it forms, introduce into the vacuum of the apparatus a rod of iron well cleaned; and, as it must not have time to grow red hot, take it out again in four or five seconds: it will then be found covered with metal, to remove which the rod is to be plunged instantly into a glass cucurbit filled with essence of turpentine. This cucurbit should be immersed in a tub of water, to prevent the essence from boiling: and notwithstanding this precaution, it will be heated so much sometimes as to take fire on the immersion of the iron rod.

Circumstances necessary to ensure success.

To execute these processes well, three persons are necessary. One should take care of the fire and work the bellows. The most active should collect the metal as it is produced, and with the utmost celerity plunge the iron rods into the essence. The third must separate the metal that is on the rods, and then plunge them into water; not only to cool them, but also to remove the alkali, that may have escaped metallization, or been formed by combustion previous to the immersion of the metal in the essence of turpentine. He must likewise take care, to wipe the rods perfectly dry, that he who collects the metal may have nothing else to do.

These processes, while the metal is producing, requires in the operators a dexterity not inferior to the celerity I have

of iron, which would tend to confirm the opinion I have given in my paper on the decomposition of the alkalis.

Two experiments demonstrate the presence of carbon in the new metals.

But I stop here, not to anticipate the question whether the metal of the alkalis contain carbon; for since I had the honour to address a note to the class, in which I mentioned two experiments, that appeared to me well fitted to demonstrate the presence of carbon in the alkaline metals, doubts on this important point have arisen. I request the class therefore, to allow me to make two experiments in its presence, against which I think nothing can be urged.

The first is the separation of the carbon contained in the metal of the alkalis without combustion: the second is the oxidation of the carbon, so as to convert it directly into carbonic acid.

That of hidrogen not so evident.

As to the hidrogen, it is not so easy to demonstrate its presence; particularly for one like me, who must be ten times in the right, to prove one truth.

The alkalis not being oxides the principal object.

However, if I demonstrate, that the alkalis are not oxigenized bodies, I shall have attained my object; and the question, whether hidrogen enter into the composition of the alkaline metals will be but a secondary consideration, which I propose to examine in another point of view.

I now proceed to the experiments, which may render us better acquainted with the nature and properties of the alkalis in the metallic form.

Grounds of the author's process.

Exp. 1. To prove the presence of carbon in the alkaline metals, it was necessary for me to have recourse to the action of a substance, with which the alkalis have more affinity, than they have with the principles that constitute them metals; and which at the same time should be incapable of furnishing any element, that would combine with those I sought to separate from the metallized alkalis. By these means I was sure of having the carbon separate, and thus furnishing a new proof, that the carbonic acid produced in burning the metal in lime-water arises from the oxigenation of the carbon.

Silex

Silex, from its indestructibility, the state of purity in which it is obtainable, and particularly its affinity for the alkalis, appeared to me to unite all the properties, that I wished

wished to find in the substance, which was to be employed in my experiment.

In fact, having heated silex in a glass tube with a little of the alkaline metal, it combined with the alkali, and set free the carbon.

decomposes the metals, & sets charcoal free.

The carbon thus separated no longer took fire in the air; it required the assistance of heat.

Exp. 2. This experiment is that to which I alluded in the note I had the honour to address to the Class. It consists in enclosing in a thin bit of lead a ball of the metal of soda, and then immersing it in a vessel filled with lime-water. The metal thus confined is obliged to oxigenize itself at the expense of the oxigen of the water. Two affinities concur, to effect this decomposition: the first is that of the alkali for water, the second that of carbon for oxigen; an affinity so much the more energetic, as in this state the carbon exhibits to us a very remarkable instance of its great propensity to become oxided; a propensity, which I shall refrain from explaining at present, for the consequences I should deduce from it would no doubt appear premature, considering the present state of our chemical knowledge. I therefore defer till another opportunity the communication of my ideas on this great and important question.

Sodium enclosed in lead, & immersed in limewater, is decomposed, & forms carbonic acid.

If in this second experiment I recommend taking the metal of soda, it is on account of its solidity, which allows it to be handled; and because its destruction is more slow, an advantage, that allows us to observe the phenomenon of the decomposition of water for some time. If, on the contrary, the experiment were made with the metal of potash, the decomposition of the water would be instantaneous; which, on the one hand, would oppose the combination of carbonic acid with lime-water, and on the other would force the gasses resulting from the decomposition of the metal to break the obstacles opposed to it by the lead, in which they would be included.

Sodium preferable to potassium for this experiment.

We see then, that the metal of potash is eminently combustible, and that of soda is less so; a property explicable by the difference of affinity of these alkalis for water.

Cause of their different properties.

One remark that I have made, and that will form the subject of a very curious experiment, is, that, in collecting the metal

Detonation of potassium in water.

metal of potash by means of iron rods, very loud detonations may be produced, the intensity of which is very similar to that of gunpowder employed in ten times the quantity.

Experiment. The following is the method of repeating this experiment with success. Instead of immersing the iron rods into essence of turpentine, the instant they are removed from the gun barrel to collect the metal, they must be plunged suddenly and perpendicularly into a bucket of water. An explosion will then take place, the loudness of which will be in proportion to the quantity of metal, and the diameter of the iron rod.

General conclusions. From the experiments and observations I have had the honour of communicating to the class, it follows:

1st, That the conversion of the alkalis into metals is not a disoxigenation of those substances; and that, on the contrary, it is a combination of the alkalis with new elements.

2dly, That the affinity of the alkaline metals for oxigen is merely a chemical illusion, occasioned by a substance, the existence of which was not suspected.

3dly, That carbon is one of the constituent principles of the alkaline metals, since it can be obtained separate from them at pleasure, or converted into carbonic acid by oxigenation.

4thly, That, if the specific gravity of the alkaline metals be less than that of water, it is because hidrogen probably accompanies the carbon in this combination.

5thly, That the disoxigenation of substances, attempted to be effected by means of the alkaline metals, will always yield equivocal results, until we have a knowledge of all the elements, that compose these singular substances.

VII.

Improved Method of Forming Jury Masts: by Captain WILLIAM BOLTON, *of the Royal Navy**.

SIR,

Jury masts easily provided. HEREWITH you will receive the model of a plan for fitting ships' jury masts, to be formed from the spare spars usually

* Trans. of the Society of Arts, vol. XXVI, p. 167. The silver medal of the Society was voted to Captain Bolton for this improvement.

usually carried on board King's ships, and in every merchantman that is properly found. By having jury masts so fitted, ships will be enabled to carry as much sail as on the usual regular mast; the great use of which I need not dwell on, only observing, that it may be of great importance to fleets after a general action, or when in want of proper lower masts, either at home or abroad, and enable ships, after the loss of their mast, to prosecute their voyage, or service, without any deficiency of sail. and capable of carrying as much sail as the proper masts.

I beg you will be pleased to lay it before the Society, and I have the honour to be,

Sir,

Your obedient humble servant,

WM. BOLTON.

REMARKS.

In the model in the Society's possession the main mast is broken about one third of its length above the deck, proper partners are secured on the deck, in which a hand mast and spare main top mast are fixed on each side of the broken main mast, and secured thereto by two spare caps, morticed on a square made in its centre. A strengthening cap, movable on these additional masts, connects them, and the upper parts of these masts are secured firmly by trustle trees in the main top. The foot of a spare fore topmast passes through a cap made from strong plank, morticed into the heads of the two temporary masts above mentioned, goes through the main top, and rests in the movable strengthening cap, which connects those two masts, and enables the fore topmast to be raised to any height which the main top will admit, and be then firmly secured by the upper cap, the main top, and the strengthening cap below it. The fore topmast being thus adjusted, the cross trees and topgallantmast are mounted upon it, which completes the whole business. Method of constructing the jury masts.

Two caps are the only things necessary to be made expressly for the purpose, the other articles being usually ready on board the ship.

In

Explanation of the plate. In Pl. I. figs. 1, 2, and 3, A A represent the partners or pieces of timber, which are bolted to the quarter deck for the mast to rest upon. B is the stump of the lower mast, which is cut square at the top, and of the same size as the head of the mast originally was; upon this square, the main and spare lower caps *a a* are fixed; two mortices must be cut in the partners A A to receive squares made at the lower ends of the two temporary masts D D, which are supported by the caps *a a*, one of them is a spare main topmast, the other a hand mast; these two support the main top E, additional squares being made on the tressel trees to receive each of them. *b* is a cap shown in fig. 2, made of four inch plank doubled for the purpose, and fitted upon the heads of the masts D D, for a fore topmast F F, the heel of which rests in a mortice made in the stump of the lower mast; it is also steadied by a double cap G, separately shown in fig. 3, on which it fids finally on the top. The topgallantmast H is fixed to the mast F by the top and cap in the usual manner. The figures 2 and 3 show the caps separated from the masts, and are the only things necessary to be made for the purpose: and the object of the cap, fig. 2, is to steady and to prevent any wringing of the lower jury masts, and to fid the topmast whenever it is reefed. The fore topmast FF appears in two separate pieces, on account of its length.

VIII.

An Improvement in the Construction of Anchors, to render them more durable and safe for Ships: with an improved Mode of Fishing Anchors. By Captain H. L. BALL, *of the Royal Navy*.*

SIR,

Anchor stocks expensive and frequently fail. THE great expense of timber in the navy for anchor stocks, and the frequency of their failing or giving way in the

* Trans. of the Society of Arts, Vol. XXVI, p 170. The silver medal of the Society was voted to Capt. Ball for these improvements.

the centre, where the square of the anchor is let into the stock, have induced me to offer to the Society of Arts &c. a plan of an anchor, which may be cheaper in construction, and more likely to hold in various situations than those in common use.

The model I have sent will sufficeintly explain my intention, and show how beneficial it may be in strengthening the anchor stocks. I wish much to notice to you its probability of holding in the ground longer than other anchors, on account of the additional weight of the stock; and this will more particularly be the case in banks which shelve suddenly down from the shore, such as at St. Helena, Cawsand Bay, and indeed in most of the islands in the West Indies. The proportion of additional iron, as explained by my model, is in all anchors to be twice and a half the diameter of the shank from each side at the stock, and of course this mode will supply the place of the present nuts, which are only intended to prevent the stock from slipping in and out, whenever it becomes loose, which accident anchors are very liable to in hot climates. My anchor stocks will save a considerable quantity of the finest timber, and give much greater security.

The improvement strengthens them and makes them hold better.

I likewise beg leave to offer to the Society a model of a double fish hook, for the purpose of fishing the anchor, an operation which, in the common mode of doing it, is frequently attended with accidents both to the ship and crew, from the anchor suddenly slipping unexpectedly in raising it to its proper position.

Accidents liable to happen in fishing anchors.

I flatter myself that these improvements will meet with the Society's approbation.

I am, Sir,

Your most obedient humble Servant,

Lower Mitcham, H. L. BALL.
Feb. 13, 1808.

This anchor, in external appearance, differs very little from the common anchor; the improvement consists in the forming and fixing of the shank of the anchor to the stock. The stock *a a*, Pl. I. figs. 6 and 7, is made of two pieces of oak bolted together, and well secured by hoops. In the common

The anchor described.

mon method, in order to prevent the anchor stock from slipping off the shank, a square projection *b b*, fig. 8, is forged upon the shank; this is improved by Captain Ball, as shown in fig. 6, where this projection *d d* is extended on each side of the shank, far enough to receive two bolts through each of these extensions, which bolts hold firmly together the two pieces of timber that form the stock, and secure the stock fast to the shank. Two iron hoops, fig. 7, *e e*, are driven on the stock between the bolts, and *f f f f* are other hoops, and *g g g g* are treenails to strengthen the whole.

Improved method of fishing the anchor.

Fig. 4, represents Captain Ball's method of fishing an anchor. Fig. 5 shows an enlarged view of his double hooks used for this purpose.

In the usual operation of heaving an anchor, it is drawn up by the cable until it appears above water: the cable will not now raise it higher, it is therefore bowsed up by the cat block *a*, fig. 4, from the cat head *b*, the cable *d* being slackened out as it rises. When it is got up as high as the cat block will raise it, a strong hook, called the fish hook, fastened to a rope *e*, which is suspended by a tackle from the shrouds, is hooked to the anchor at the bottom of the shank, and thus the arms of the anchor are elevated above the stock, until one of the flukes is brought up to the timber heads *f f*, to which it is made fast by a rope and chain, called the shank painter. In this operation the fish hook sometimes slips and occasions mischief, to remedy which, Captain Ball has applied two hooks instead of one, which keep firmer hold. These hooks are shown upon an enlarged scale at *g g* fig. 5, attached to the rope *e*; each of these hooks takes one of the arms of the anchor, close to the shank, and holds it firmly. *i i* are two small lines made fast to the hooks, to direct them so as to get proper hold of the anchor.

IX.

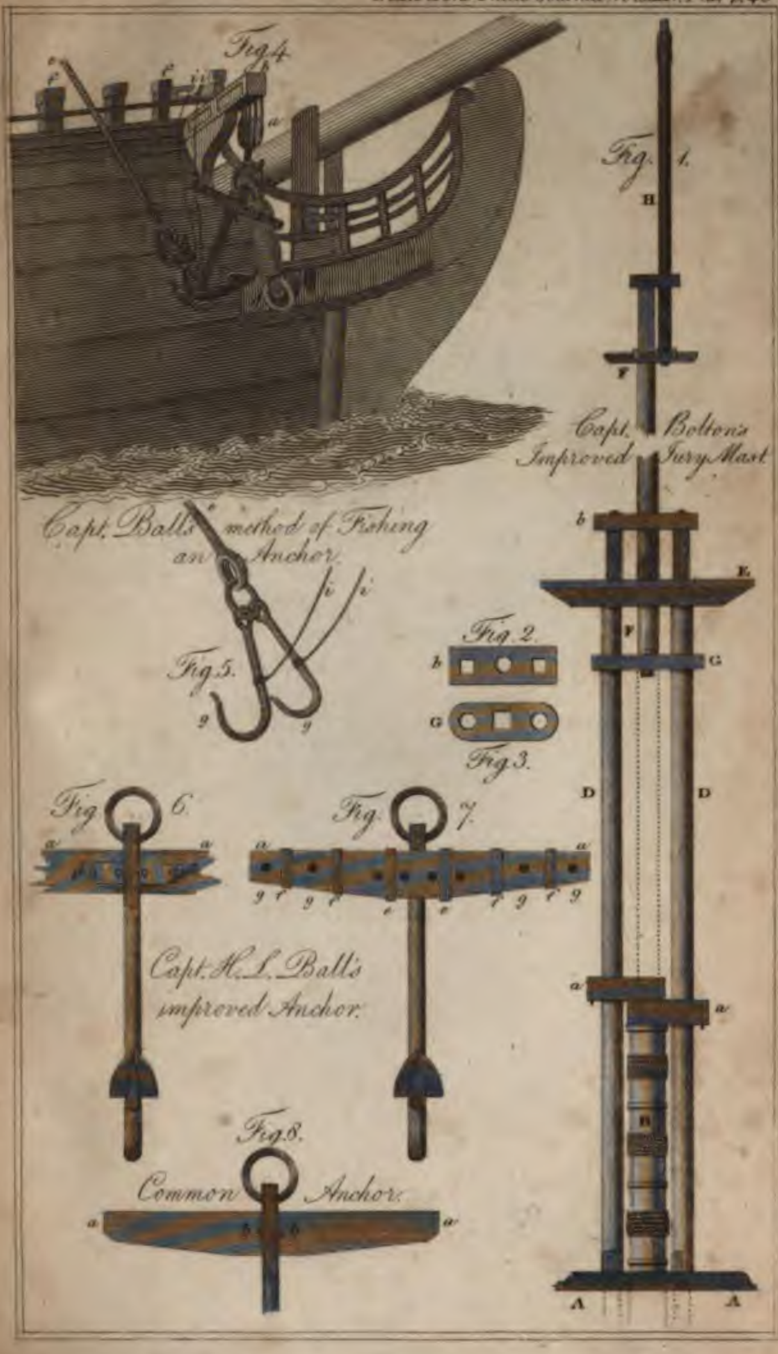
Fig. 4
Capt. Ball's method of Fishing an Anchor
Fig. 5.
Fig. 1.
Capt. Bolton's Improved Jury Mast
Fig. 2.
Fig. 3.
Fig. 6.
Fig. 7.
Capt. H. L. Ball's improved Anchor.
Fig. 8.
Common Anchor.

IX.

Observations on the Progress of Bodies floating in a Stream: with an Account of some Experiments made in the River Thames, with a View to discover a Method for ascertaining the Direction of Currents. By James Burney, Esq.

Having frequently noticed, that the heavy craft on the River Thames, during a calm and without the assistance of oars or of towing, made a progress faster than the stream of the tide on the surface, it led me to make inquiry as well into the fact as concerning the cause, and gave rise to some experiments, which, with the ideas they suggested, are here set down; no otherwise according to method than being in the order they occurred.

Heavy bodies float down a stream faster than the current.

On questioning the men belonging to several barges, which, unaided by wind, oar, or towing, were floating with and overtaking the stream, they all agreed in the general fact, as a circumstance familiar to them. They said likewise, that a laden barge made greater progress than a light barge; and this was corroborated by the evidence of the boats attached to them being drawn after them; for the barges overtook the moving water so fast as to have good steerage way. They attributed the difference in favour of a laden barge, to her having (as they expressed it) more hold of the tide than a light barge: by which it appears, that they supposed the stream of the tide was stronger underneath than on the surface. Adhesion to the atmosphere may retard the surface, except when the current of the atmosphere (the wind) goes in the same direction with the current of the tide; and then it may occasion an acceleration.

Laden barges float faster than light ones.

Monday, July the 18th, I went on board a barge half laden, which was floating down the river, but with steerage way, between Putney and Chelsea bridges. I conjectured the rate of the tide to be a mile and a half per hour: there was a very light air of wind in a direction contrary to the stream

This is not owing to a more rapid under current.

stream of the tide: but the barge, without any assistance of oars or towing, passed on, overtaking the stream, and her boat was towing astern. I fastened a riband to the end of a stick, and immersed it in the water about 20 inches, which was as low as the lowest part of the barge's bottom, and therefore sufficient to have shown, by the direction of the streamer, if the barge had been impelled forward by superior velocity of the under current, as in that case the streamer would have gone before the stick; but the streamer tended towards the stern, and was drawn after the stick: whence it was evident, that the barge's progress exceeded that of the stream underneath as well as on the surface, and that this excess was acceleration produced by some other cause*.

The surface of a stream an inclined plane.

As by the general law of gravitation the heaviest bodies descend with most velocity in a yielding medium, so it appears to be with bodies floating in a stream. The surface of a stream or current of water is not horizontal, but an inclined plane, and the inclination of the surface produces the current. Thus, when, by the attraction of the Sun or Moon, the sea is raised in some parts, it becomes depressed in others, and the water, seeking to regain its level, flows in a current from the superior parts.

The barges on the river in a calm therefore *slide downward* with the stream, and also on it.

Wherry outstripped by a barge.

A friend of mine in a wherry going to pass under London bridge, being closely preceded by a coal barge, was apprehensive of receiving damage from collision with the barge when under the bridge; but the waterman said the barge would shoot far enough ahead when she came to the indraught of the arch. And it happened accordingly; for

Under currents produced by local circumstances.

* No part of what is here said contradicts any received hypothesis concerning under currents. Some under currents proceed from visible causes, as when the wind blows for a length of time in one direction towards a coast, especially if it is an embayed coast, whereby the waters are accumulated and the surface near the shore is raised above the general level, till the pressure of the increased weight forces back the water underneath. Under currents, where the causes are not visible, may be supposed to be caused by inequalities in the bottom, in the same manner as eddies are caused by the projecting points of a coast interrupting the general course of a stream.

the

the barge, arriving first within the sterling heads, shot away from the wherry about 200 yards, by the superior momentum she acquired in the increased declivity.

Perpendicular and oblique pressure acting on a floating body.

A pressure perpendicular to the horizon applied to a body floating on a horizontal surface acts as increase of weight, having the effect only of making the body to which it is applied swim deeper, or occupy more space in the water. An oblique pressure, not strong enough to submerge the body, affects it in two directions; one downwards in the manner of weight, to which the body yields to a certain and definite extent; the other horizontal, in which direction the body continually gives way to the pressure. Almost every person has experienced the readiness of a boat to glide from under him, on putting his first foot in her. These two effects of an inclined pressure are separately in proportion to the whole pressure, one as the sine, the other as the cosine, of the angle of incidence is to radius.

Applied to a barge on the stream.

If to a body floating on a horizontal surface a pressure is applied in a direction making with the horizon an angle of 89° 59′, the proportion of the pressure which would act horizontally is to the whole pressure, as the sine of 1′ is to radius. And this proportion is $\frac{29}{100000}$ of the whole pressure. In like manner, if the surface inclines 1′ from the true horizontal level, weight applied to a body floating on that surface will give an impulse towards the declining part of the surface equal to $\frac{29}{100000}$ of the weight applied. Consequently, a barge having in her 100 tons weight, floating with the stream where the declivity of the surface is 1′, will receive an impulse towards the declining part of the surface equal to nearly 65lbs. which is little short of what is estimated to be the average pull of a horse.

Indication of the course and strength of a current.

Hence it seems naturally to follow, that two pieces of wood, equal in size but differing in weight, being placed in the water near to each other, would show if there was a current, by the heavier wood separating from the lighter in the direction of the stream. Likewise, that the quantity of separation in a given time might afford a measure for the strength of the current. And it is probable, that this would be found true in a smooth and equal running stream, where no interruption was caused by the wind.

E 2 I sup-

Experiment with an oak & fir ball:

I supposed, that the best form to be given the wood for making the experiment would be globular, as being less liable than any other to be affected by irregularities in the surface of the water. I caused two wooden balls to be made, one of oak 6 inches in diameter, the other of fir, and not so large. I chose a time when the quietness of the air was next to calm, and the surface of the water very smooth. The balls were put into the stream; the oak swam deep, leaving a very small portion uncovered; but the fir ball was found so very susceptible of motion from the lightest air of wind, that no conclusion could be obtained from this experiment.

with a staff loaded at one end.

It was suggested by Mr. Rickman, my associate in these experiments, and whose observations jointly with my own have furnished this paper, that for showing the direction of the current, a long staff of light wood, loaded at one end, might better answer the purpose than two unconnected floating bodies, because whenever it got out of the right direction it would have a tendency to correct itself.

On Wednesday, July 27th, we made some experiments on the river; but the weather was not favourable. Two sticks, one of them a common walking stick with a piece of lead fastened to one end, the other a hollow tube (a joint of a fishing rod) loaded internally at one end, were put into the stream (but not in any preconcerted or remarked direction) and they both took the direction of the stream, the heaviest end becoming the most advanced. They were taken up, and being again put into the water in a direction opposite to the stream, they gradually regained their former direction: in what time was not observed. In endeavouring again to repeat the experiment, two barges passing caused us to lose sight of our sticks, and we did not find them afterwards.

Passage of boats through the arch of a bridge.

About an hour after the flood had made through London bridge, I noticed from the top of the bridge the passage of some of the craft. When any one drew near the arch, she did not keep pace with the water before her, so that on looking only at her head, she seemed to have stern way; but at her stern she left the marks of her track behind her. Two barges and a small boat, the small boat being in the middle

middle, at small and nearly equal intervals, followed each other through. That which first came to the increased fall under the arch, being precipitated, left the others far behind, till in their turn they were in like manner precipitated. When they arrived out of the rapid part of the stream into the smooth water, I did not observe, that their relative position was altered from what it had been before they came to the bridge: but the small boat had made use of oars.

Altitude of the horizon differing with the tide.

It would answer other purposes than that of curiosity, to ascertain and form tables of the declivity of the surface at different velocities of current. The observations of altitudes at sea must be affected by currents, one part of the sensible horizon being higher than the other. A ship stationed in a tide which runs one way North, the other South, may expect to find the observed latitude vary with the tide.

Attempt to ascertain the declivity of the surface of the Thames.

In the afternoon of the same day that we made the experiment with the sticks (the 27th), we made a very imperfect attempt to discover what was the declivity of the surface above Westminster bridge, or rather what angle the plane of the surface on the flood made with it on the ebb, by marking at two distant stations at the same times, the height of the water during the flowing, and likewise during the ebbing of the tide. One station was at one of the posts close under the Speaker's garden: the other, on the same side of the river, at the ferry opposite Cumberland Gardens. The distance between the stations, according to the maps of London, is seven furlongs.

At the post near the Speaker's garden, the difference of the height of the river, taken at 4 P.M., the tide then flowing, and at 7 P.M., the tide ebbing, was 25·5 inches.

At the ferry opposite Cumberland Gardens, the difference of the heights taken at the times above mentioned was 17·3 inches.

The tide was lower at 7 o'clock than at 4. The stream at each of those times was running at a rate which we conjectured to be nearly three miles per hour: therefore a greater variation was expected than 8·2 inches, which in a distance of 7 furlongs will give only 30″ for the angular difference between the plane of the surface on the flood, and the

the plane on the ebb; so that the angle of declivity from the horizontal level, supposing it equal each way, was not more than 15″. Perhaps the difference would have been found greater, if the marks had been taken in mid stream, instead of close to the side of the river. The stoppage of Westminster bridge may likewise be supposed to occasion some swell in the part of the river immediately above it, during the ebb tide.

Experiments with sticks less satisfactory than before.

On Wednesday, August the 3d, we again made experiments with sticks; which proved less satisfactory than those we had before made. But, previous to describing farther operations, it is necessary to notice a consideration, which, when it first occurred, seemed an insurmountable objection to deriving any benefit from them. This was, the great difference between the surface in a river, and the surface in an open sea; so that an experiment, which might be found to succeed in the one, might scarcely be at all practicable in the other. To this objection it is reasonable to answer, or at least a reasonable encouragement to expect, that if a small stick will point the direction of the stream in a river, a long pole (a steering sail boom, for instance), will, in circumstances tolerably favourable, do so at sea. It seems within the rule of just proportion, that a spar as large as a steering sail boom as much exceeds a walking stick, as the irregularity of the surface at sea, in temperate weather, exceeds that of a river. In the experiment, two or more might be put into the sea at the same time, and if they agreed, there would be the greater reason for placing reliance on the result.

Long poles requisite at sea.

Experiment with heavy wood a foot beneath the surface.

Our next trial was with one of the South Sea island clubs, of a wood not buoyant, about three feet in length, and gradually tapering. It was buoyed at each end with cork, but with string enough to let it be about a foot under the surface; and that the corks at each end might be equally exposed to the air, they were so managed as to show equally and similarly above water. There is reason, however, to think it would have been more proper to have allowed the exposure to the air at each end to be proportioned to the weight sustained. In the manner the experiment was made, the club, being left to itself in the stream, did not take or keep

keep to any determinate direction. It was unfavourable to this experiment, that the club was not of greater length, and, perhaps, that its weight at each end was in the same proportion to its size.

We again tried with sticks loaded at one end. But the most that can be said of the results of this day's experiments is, that the loaded end evidently showed the most tendency to be downward with the stream. The sticks veered in their direction more than we had observed in the former trials. This brought to recollection a circumstance, to which we had not paid attention. The barges we had seen over-sliding the stream in a calm were kept in the right direction by their helm. Supposing a barge to be loaded at one end, and light at the other; without the help of the helm, the loaded end would probably not be found to keep her in the same direction with the stream, any more than the head sails only of a ship, being set, will, without the help of the helm, keep the ship before the wind. Sticks with one end loaded,

This consideration leads again to trials with two or more separate and unconnected bodies. An experiment which can easily be made in a ship, is with a hogshead, which can be filled after it is put into the sea, and a quart keg, which if the air should be quite calm i would be sufficient to half fill. This would approach the proportion of a barge and her small boat: but as no guidance can be given, the most regular shape (globular) seems the best. A hogshead & a small keg might be tried at sea.

The stream in the Thames above the bridges, from the unevenness and shallowness of the bottom, is unfavourable to experiments of the kind here recommended; and the superior convenience possessed by those whose constant occupation is on the waters, who have opportunities, without an hour's expense of time, to make experiments, which to other persons would cost days, have been inducements to publish the inquiry in its present state, to give it the best chance of being prosecuted with any effect. Experiments should be made by those who have favourable opportunities.

JAMES BURNEY.

James Street, Westminster,
August 20th, 1808.

The

The above paper was read at a meeting of the Royal Society, February 16th, 1809. In consequence of some observations which it produced, the following remarks, in addition, were presented, and read at the Society.

Weight impels the stream itself, as well as the body floating on it.

In a paper I had the honour to present to the Royal Society, on the progress made by some bodies floating in a stream, they descending faster than the stream itself, I endeavoured to show, that this progress was the effect of perpendicular pressure, producing impulse towards the declining part of the surface. The same cause, indeed, evidently applies to the production of the stream itself; consequently the surface, and whatsoever floats on it, are, in this respect, on an equal footing, and the whole agree in pressing forward and in opposing resistance to whatsoever endeavours to overtake them. Without some auxiliary cause, therefore, a floating body cannot overtake the stream.

Shape and direction of a body affects its velocity.

The different shapes of bodies, and likewise the directions in which they are placed with respect to the direction of the impelling power, expose them to more or less resistance. A barge floating crossways to the stream receives the progressive impulse with the least advantage, her whole length acting in resistance to her overtaking the stream. The same barge when endways with the stream, is acted upon by the same quantity of impelling power, and her progress is opposed by less resistance.

Weight adds more to the impulse than to the resistance.

With increase of weight, both the impelling power and the resistance are increased: but when the barge is lengthways with the line of the stream, weight added will increase the impelling power in a greater proportion than the resistance is increased. Hence the heavy barge in a calm will overtake the light barge.

This short explanation I beg to offer as an addition to my former remarks on the subject, and shall be glad if it assists in any satisfactory manner to account for vessels overtaking the stream in a calm.

May 15th, 1809.

X.

X.

New Method proposed for measuring a Ship's Rate of Sailing. By the same Gentleman.

A Line towing astern of a vessel, which is passing through the water, will pull against her head-way. As the ship's way increases, the pull of the line will increase; and *vice versa*. If this, with a proper scope of line (about 25 fathoms may probably be sufficient) shall be found to be a regulated quantity of pull corresponding in the same manner at all times to the rate of sailing, it will answer the purpose of a log. Many experiments have been made upon the same principle; but the most plain and easy one, of towing a measured length of line, has escaped trial; though less liable to give erroneous or variable results than any which can be made near a ship. By it, the rate of sailing may be obtained either constantly or occasionally, and can be taken with ease by one person: in which respect it would have great advantage over the common log, the use of which requires three persons.

A line towed astern of a ship will be a perpetual log.

By a trial made in a boat with about 20 fathoms of line, rather larger than log line, towing astern and fastened to a spring steelyard, the strength of the pull was found to vary with the rate of sailing, which however was not ascertained by measurement; but by estimation, the boat's rate of sailing during the trial varied between 2¼ knots and 5 knots per hour, and the pull of the line upon the steelyards was observed to vary from 2 lbs. to 5¼ lbs.; increasing and decreasing with the velocity. So great a variation in the strength of the pull gives all the advantage, which can be desired for forming a scale, and will allow of the experiment being made with smaller line.

Experiment.

If the proposed length of line is passed through a pulley so as to go clear out at the stern port or cabin window, and the inner end is fastened to a loose chain, of weight adapted to the purpose, on the deck under the pulley; or to a number of small weights made consecutive by short intervals of line, the chain or weights will be drawn up more or less according

The velocity indicated by weights,

cording to the ship's velocity. By a few comparisons of the quantity of weight raised from the deck with the rate of sailing, a scale may be marked.

or a spring and index.

In an improved state of the experiment, instead of using weights or a pulley, the inner end of the line (coming direct from the water) can be fastened to a spring, and communicate with an index that shall express the rate of sailing.

This machine (if so plain a contrivance deserve that name) may be put on constant duty, or dropped occasionally to ascertain the rate.

Objections which occur, are,

Objections, & answers to them.

1st. The line being liable to contraction or expansion as the temperature of the water varies. But it is scarcely to be supposed, that the greatest contraction or expansion of line from its mean state (after it has been properly stretched and seasoned) will occasion an alteration of a hundredth part in the force of the pull.

2d. That in a fresh wind the part of the line between the ship and the surface of the water, will be liable to some additional pull from being exposed to the wind. To this inconvenience, the log line in the common way of heaving the log is likewise exposed when the wind is much aft. In either case, when the ship is not right before the wind, the remedy is the same: which is, to throw the log or the line over from before the lee gangway, and to give a few fathoms more of stray line; for which however, in the new method proposed, it would be necessary to apply a correction, the quantity of which may be accurately ascertained.

3d. The motion of a ship in pitching. But this is not to be regarded as an objection; for the rate of sailing is to be estimated only by what the experiment shows when the ship is going steadily; in the same manner as in taking bearings, if the compass swings, we wait till it is quiet. Whenever the ship goes steadily for ten seconds together, or even five seconds, the pull of the line will be regulated to the average rate of sailing.

XI.

XI.

Method of preventing Doors from Dragging on Carpets, or admitting Air underneath them. By Mr. John Tad*.

SIR,

I Have taken the liberty of laying before the Society a model of my invention to prevent doors from dragging on carpets, and to keep out the current of cold air, which enters under such doors as are not close to the carpets underneath them.

Method of preventing air-tight doors from dragging on carpets.

I can affix this machinery to the bottom of any door, so that the door shall pass over the carpet with ease, and, when shut, be air tight. It obviates the necessity of screw rising hinges, and is less expensive than other inventions for the same purpose.

The machinery is constructed of a slip of well seasoned beech wood, equal in length to the width of the door; this slip is one inch and a quarter wide, and half an inch thick, and to be covered with green cloth on the inside; it is to be hung to the bottom of the door with three small brass hinges, and is drawn up by a concealed spring as the door opens, and is forced down when the door shuts, by one end of it, which is semicircular, pressing upon a concave semicircular piece of hard beech wood, fastened at the bottom of the door case, and which holds it down close to the floor or carpet, so as to exclude the air from entering under it. Hoping this invention will meet with the approbation of the Society, I remain, with respect,

Sir,

Your most humble Servant,

No. 4, *Little Hermitage Street,* JOHN TAD.
Wapping, Nov. 24, 1807.

A Certificate was received from Mr. William French, No. 280, Wapping, stating, that John Tad had fixed to two of his room doors the invention above mentioned, and

Certificate of its efficacy.

* Trans. of the Society of Arts, vol. XXVI, p. 196. Five guineas were voted to Mr. Tad, for this communication.

that

that he found it to answer to his satisfaction, both in permitting the doors to pass clear of the carpets, and in keeping out the air.

The method described.

Mr. Tad's invention consists in first cutting away the bottom of the door, so that it is about one inch and a quarter above the floor; this allows a sufficiency of room for the door to open over any carpet. To close the opening which would now be left under the door when shut, he proposes to fix beneath the door, by means of hinges, a slip of wood, of which *a b d e*, figs. 2 and 3, Plate II, is a section. Fig. 1 is a perspective view of the bottom of a door, with the invention annexed to it; fig. 2 is a section across the door when closed; fig. 3 is a view of the edge of the door when open; and fig. 4 is a section supposed to be made by cutting the door in two parts, edgeways. The hinges on which the slip turns, are fixed to the edge. In figs. 2 and 3, from *a* to *b* is exactly one inch and a quarter, so that when the ruler is turned down upon the hinges, it reaches the floor A A, as in fig. 2; in the other direction *a d* it is much less, being only half an inch, so that when it is turned up under the door, as in fig. 3, it leaves three quarters of an inch clear of the floor. It now remains to show how the ruler is turned up or down; it has always a tendency to rise up into the state of fig. 3, by the action of a steel wire spring, shown in figs. 2 and 4, which is concealed in a rebate cut in the bottom of the door; one end of the wire is screwed fast to the door at *f*, the other is inserted into an eye fastened into the slip at *g*. To throw it down into the position of figs. 2 and 4, the end *h*, fig. 4, of the slip farthest from the hinges of the door, is cut into a semicircle, as seen in fig. 3. When the door is just closed, this semicircle is received into a fixed concave semicircle *k*, fig. 3, cut in the end of a piece of wood *k l*, made fast to the door case; the line *m l*, fig. 3, represents the plane of the door when shut, and *p p* part of the door seen edgeways; as the door in shutting moves from *p* to *m*, the semicircular end of the slip *a b d e* presses against the end of the piece *k l*, and as the door proceeds, it turns down as in fig. 2, so that by the time the door is shut, the slip is turned quite down; the edge *e b* of the slip is cut into a segment

segment of a circle struck from the hinges on which it turns. The perspective view in fig. 1 shows that this contrivance, applied to any door, will not offend the eye, as it can scarcely be distinguished from an ordinary door. *K*, fig. 1, shows the concave semicircle of the piece of wood fastened to the doorcase, in which the semicircular end of the slip *e* is to be received.

XII.

Description of an improved Screw-wrench, to fit different sized Nuts, or Heads of Screws. By Mr. WILLIAM BARLOW*.

SIR,

PERMIT me to make a few observations on a shifting screw-wrench of my invention, which I beg leave to lay before the Society of Arts &c. through the hands of Mr. Brunel, inventor of the block machinery here.

I have found, from long experience, the imperfections of the various wrenches in common use, for the screw heads and nuts of engines in general, which are often materially injured for want of an instrument that would fit variety of sizes, and be applied with as much advantage as a solid wrench. I have had it in view to unite steadiness with conveniency in making such an instrument, and flattering myself that I have obtained both, I am desirous to communicate my invention to the Society, and have therefore sent an instrument on the principle I have actually used, and which has met with the approbation of my employers and other persons. Common screw-wrench defective.

This wrench, by means of a nut and screw, is adjusted with the greatest ease to the exact size required, and in that state rendered so steady, that in use it is found equal to a solid wrench. An improved wrench.

* Trans. of the Society of Arts, vol. XXVI, p. 199. Five guineas were voted to Mr. Barlow for this invention.

I have

I have, for several years, been intrusted with the care and repairs of many valuable engines of various descriptions, composing the block machinery in this dock-yard, and I have always considered it as an object of great importance, for the preservation and neat appearance of engines, to attend to all the means which would obtain these advantages, and such, I think would arise from the use of my universal wrench.

May be made of various sizes.

It is, perhaps, unnecessary to point out, that a wrench on this principle may be varied in its form and size so as to be rendered probably more convenient for some particular purposes for which such instruments are required.

I am, Sir,

Your obedient servant,

Portsmouth Dock Yard, WM. BARLOW.

March 1, 1808.

The instrument described.

This instrument is represented in Pl. II. Fig. 5 is a perspective view of it; fig. 6 a section of its head; and fig. 7 an external representation of the head. The screw head or nut to be turned is held between two jaws, one of which *a b d e* is forged in the same piece with the handle A A, the other, *f g*, is moveable between two chukes, and fastened to the fixed jaw by the strong screw *i*, which is fixed to the same jaw, passes through the moveable one, as shown in the section fig. 6, and has a nut screwed upon it; the other screw *h*, is tapped through the movable jaw, and its point presses upon the bottom of a cavity made in the fixed jaw shown at *m* in the section fig. 6. To make the wrench fit any particular screw head or nut, the nut upon the strong screw *i* must first be loosened, and the screw *h* screwed in or out of the movable jaw, until the opening *b g* is just the proper width to receive the screw head or nut to be turned by the wrench; the nut of the screw *i* is then to be screwed down, until it presses upon the jaw, and holds it perfectly tight.

XIII.

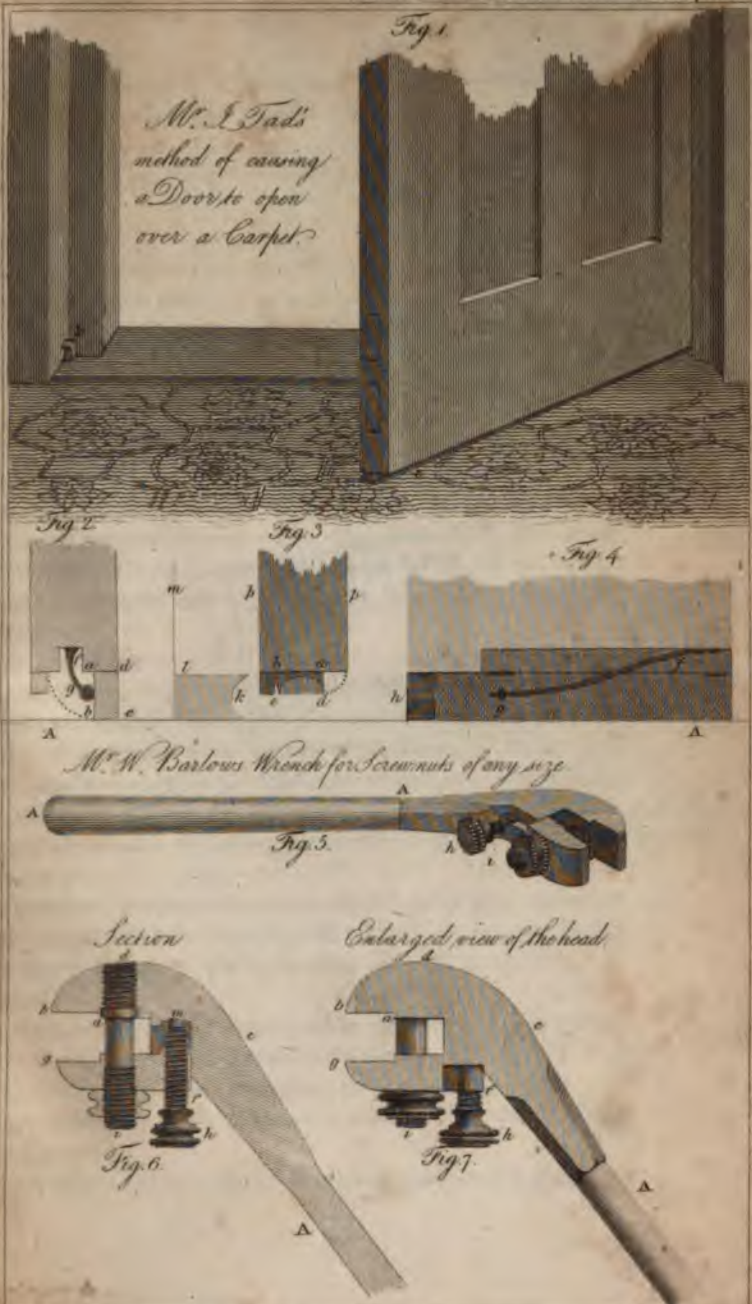
Fig. 1.
Mr. I. Tad's method of causing a Door to open over a Carpet.
Fig. 2
Fig. 3
Fig. 4
Mr. W. Barlow's Wrench for Screw-nuts of any size.
Fig. 5.
Section
Fig. 6.
Enlarged view of the head
Fig. 7.

XIII.

On the Measurement of Heights by the Barometer. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR, *July* 17*th*, 1809.

THE method of finding heights by the barometer bids fair to be of the greatest practical utility; especially since the improved construction of portable barometers, and the invention of more compendious modes of calculation than those formerly in use, have considerably diminished the difficulties, with which it was at first attended. Finding heights by the barometer of great practical utility.

It would be desirable, however, if the necessary calculations could be still farther simplified: for it must frequently happen, that observations of the heights of the barometer are made by travellers at times, when the mind, distracted by a variety of objects, or borne down by the fatigue of the body, may be ill calculated for even a moderate degree of exertion. Desirable to have the calculations still farther simplified.

For this purpose the following tables have been calculated, which, with little more than the mere trouble of inspection, will give the result true to the nearest foot. They may be printed on the surface of a common card, so that their bulk cannot be the least inconvenience to a traveller.

Table 1. Contains altitudes in feet answering to every tenth of an inch of the height of the barometer from 25 to 31 inches. Use of the following tables.

Table 2. Contains the proportional parts to be deducted for every additional hundredth of an inch, corresponding to the heights of the barometer marked in the first column.

Hence to find the approximate elevation of one station above another, nothing more is necessary, than to find from Tables 1 and 2 the elevations corresponding to the observed heights of the barometer, and subtract the less from the greater.

It would not be difficult to construct tables, which should give

give the result by mere inspection: but unless they should be continued to every hundredth of an inch (in which case they would make a volume) the trouble of subtraction is all that would be saved.

Table 3. Gives the correction for the expansion of air for every 1000 feet of altitude. It is calculated for every degree of the mean temperature from 72 to 32. It is probable that few observations will be made in this island, where the mean temperature is not within these limits.

This table is calculated from Table 5, p. 484, of Gregory's Mechanics, vol. 1.

With regard to the correction for the expansion of the mercury, it may be obtained without any sensible errour, by multiplying the difference of temperature in degrees of Fahrenheit by 2·75 feet, or 2 feet 9 inches.

Yours, &c.

J. B.

TABLE I.

Bar.	F.	B.	F.	B.	F.
25·0	5606	27·0	3600	29·0	1738
·1	5502	·1	3504	·1	1648
·2	5398	·2	3408	·2	1559
·3	5295	·3	3313	·3	1470
·4	5192	·4	3217	·4	1381
·5	5090	·5	3122	·5	1293
·6	4988	·6	3028	·6	1205
·7	4886	·7	2934	·7	1117
·8	4785	·8	2840	·8	1029
·9	4684	·9	2746	·9	942
26·0	4584	28·0	2653	30·0	855
·1	4483	·1	2559	·1	768
·2	4384	·2	2467	·2	681
·3	4285	·3	2375	·3	595
·4	4186	·4	2283	·4	509
·5	4087	·5	2191	·5	424
·6	3989	·6	2100	·6	338
·7	3891	·7	2009	·7	253
·8	3794	·8	1918	·8	169
·9	3697	28·9	1828	·9	84

TABLE II.

B.	
25·0	10·4
·2	—·3
·5	—·2
·7	—·1
26·0	—·0
·3	-9·9
·5	—·8
·8	—·7
27·1	—·6
·3	—·5
·6	—·4
·9	—·3
28·2	—·2
·5	—·1
·9	—·0
29·2	-8·9
·5	—·8
·9	—·7
30·2	—·6
·6	—·5
31·0	—·4

TABLE

TABLE III.

Th.	Cor.	Th	Cor	Th.	Cor.
82	126	59	68	45	32
72	101	58	65	44	29
71	98	57	62	43	27
70	96	56	60	42	24
69	93	55	57	41	22
68	91	54	54	40	19
67	88	53	52	39	17
66	86	52	49	38	14
65	83	51	47	37	12
64	80	50	44	36	10
63	78	49	42	35	7
62	75	48	39	34	5
61	73	47	37	83	2
60	70	46	34	32	—

XIV.

On the Glauberite. By ALEXANDER BROGNIART*.

Figure of the crystals of glauberite.

THE form of the glauberite is that of an oblique prism, greatly depressed, and with a rhombic base. The angles of the parallelogram of the base are 76° and 104°. The angles of incidence between the parallelogram of the base and the adjacent sides are 142°. That between the base and the edge contiguous to the acute angle of the base is 154°. The faces of the base are generally plane, smooth, and even shining: those of the sides on the contrary are full of striæ, parallel to the edges of the base. Very evident junctures parallel to the base are discoverable by cleaving; as are others not so well defined, parallel to the edges of the base, and inclined to the former in angles of about 104°.

Primitive form.

These observations give as the primitive form of this crystal an oblique prism with a rhombic base.

Colour.

The crystals are nearly limpid, or of a topaz yellow, and retain their solidity and transparency in the air, if they have not been wetted.

* Journal de Physique, vol. LXVI, p. 235.

Hardness. Their hardness exceeds that of sulphate of lime, but is inferior to that of carbonate of lime.

Action of fire. Exposed to the fire, the glauberite splits, decrepitates, and melts into a white enamel.

Singular action of water. Immersed in water, its surface becomes of a milky white, and in a little time the whole of the crystal grows completely white and opake. Taken out of the water and dried, it does not resume its transparency, but the white coating falls to powder; and, if it be entirely removed, the nucleus is discovered remaining unaltered. It is the only mineral substance that possesses this property.

Spec. grav. The specific gravity of the glauberite is 2·73.

Crystals resembling it. This salt, the crystals of which at first sight bear some resemblance to those of axinite, and the fragments of which are a little like those of sulphate of lime, differs essentially from the latter, whether anhydrous or possessing its water of crystallization, in its primitive form, and in the secondary forms derived from it.

Its component parts. It is composed of anhydrous sulphate of lime ···· 49
anhydrous sulphate of soda ···· 51

100

No water. Mr. Brongniart satisfied himself, that it contained no water, not only by several calcinations at the temperature nearly of melting silver, but also by distilling it after Mr. Berthollet's manner with iron filings, when he could obtain no hidrogen gas.

Sulphate of soda, He ascertained the presence of the sulphate of soda by solution and crystallization, which afforded him well defined crystals of this sulphate.

and of lime. The sulphate of lime he found by decomposing this salt both by carbonate of ammonia and oxalate of ammonia.

No loss. As he had no loss, but what cannot be avoided in chemical operations conducted with the greatest care, and this loss did not amount to one per cent, he presumes, that this stone contains no other ponderable matter essential to it but the two salts mentioned above: and to be more certain of this, he examined carefully, whether it contained no phosphates,

phosphates, borates, or muriates, which might have been suspected from the situation where it was found.

Where found. The glauberite was brought from Spain by Mr. Dumeril. It has hitherto been found only at Villarubia, near Ocanna, in new Castile. Its crystals are sometimes solitary, sometimes in clusters, and disseminated in masses of sal gem. Mr. Brongniart has not been able to find any mention of this mineral, either in the works of mineralogists, or in those of travellers in Spain, that he could consult.

XV.

An excellent colourless Copal Varnish. By Mr. LENORMAND, *late Professor of Natural Philosophy**.

Copal difficult of solution. EVERY one knows the difficulty of dissolving copal completely, when we attempt to make a varnish, I hasten therefore to communicate a method, that has succeeded perfectly with me; and which will be found, to produce a very fine varnish with this substance.

Method of selecting it, All copal is not fit for making this varnish, it must therefore be selected with care, and the following method will show what is good. Take each piece of copal separately, and let fall on it a single drop of very pure essential oil of rosemary, not altered by keeping. Those pieces on which the oil makes a certain impression, that is to say, which soften at the part that imbibes the oil, are good, and should be reserved for making varnish. The others are to be rejected.

and of making the varnish. Powder the pieces of copal thus selected, sift the powder through a very fine hair sieve, and put it into a glass, on the bottom of which it must not lie more than a finger's breadth thick. On it pour essence of rosemary to a similar height, stir the whole together with a stick for a few minutes, the copal will dissolve into a viscous substance, and the whole will form a very thick fluid. Let it stand for a couple of hours, after which pour on gently two or three drops of

* Sonnini's Bibliothèque Physico-économique for 1808, Vol. II, p. 133.

very

very pure alcohol, which you will distribute over the oily mass by inclining the glass in different directions with a very gentle motion. In this way you will effect their incorporation. Repeat this operation by little and little, till the varnish is reduced to a proper degree of fluidity. Remember, the first drops of alcohol are the most difficult, and require the longest time to incorporate; and that the difficulty diminishes as each successive addition is incorporated, or as the mass approaches the state of saturation.

When the varnish has attained the suitable degree of fluidity, it is to be suffered to stand a few days; and when it has become very clear, the varnish is to be decanted off.

The magma that remains at the bottom may still be rendered useful, by pouring on alcohol in the manner directed above; but care must be taken, to add very little at a time.

Uses of the varnish.

This varnish is made without heat, is very clear and colourless, may be applied with equal success on pasteboard, wood, and metals, and may be worked and polished with ease, indeed better than any known varnish. It may be used on paintings, and singularly heightens their beauty.

SCIENTIFIC NEWS.

French National Institute.

French national Institute. Question for a double prize.

THE public sitting of the Mathematical and Physical Class for 1809 was held on the 2nd of January. A double prize had been offered for a "Theory of the perturbations of the planet Pallas, discovered by Dr. Olbers: or in general the theory of planets, the excentricity and inclination of which are too considerable for their perturbations to be calculated with sufficient precision by the known methods." Not to enter into any thing more than is indispensable, on such a difficult subject, nothing was required farther than algebraic formulæ, but so arranged, that an intelligent calculator might apply them securely, and without mistake, either to the planet Pallas, or to any other already discovered, or that may be discovered hereafter. Notwithstanding these restrictions, no paper having been sent, the subject is still

still left open till the 1st of October, 1810. The prize is a medal of the value of 6000 francs [£250].

Another prize question.

The ordinary prize subject for next year is: "To examine whether there be any circulation in the animals known by the names of asteriæ, echini, and holothuriæ; and, if there be, to describe its course and organs." The description must be accompanied with observations made on living animals, and include the vessels of the respiratory organs, if there be any such, as well as those of the principal circulation. To examine the chemical effect of the respiration on the air and water, would be a desirable adition, but this is not absolutely insisted on. The examination of one species of each family only is required; but it is expected to be by no means superficial, and accompanied with drawings, so that the principal details may easily be verified. The prize is 3000 francs [£125], and the term as above.

Mathematical class.

The history of the mathematical division of the class of physical and mathematical sciences exhibits this year a singular circumstance; one of the most difficult and most important points of the solar system treated with equal success, though after different methods, by two geometricians of the first rank; to both of whom the investigation was suggested by an interesting paper read to the class by a young geometrician. Astronomers had remarked a perceptible acceleration in the course of the moon: consequently other planets, and among them the Earth, must have a similar acceleration. If the motion of the Earth be accelerated, it must be owing to its approaching the centre of motion: and, if it do, will it not ultimately fall into the sun? The danger of this indeed must be infinitely remote, for the acceleration is extemely slow; and it appears from the instance of the moon, that the acceleration continues but for a time, and is afterward changed into retardation. Still however the question is particularly interesting to astronomers, who in all their calculations suppose the unchangeableness of the ellipses described by the planets.

Problem whether the planets have a constant acceleration.

Mr. Laplace first examined this question, and found by a learned but merely approximate calculation, that the mean motions and axes are really invariable; at least taking into consideration only the first powers of the masses, and the second

Laplace showed they have not by an approximate calculation;

which Lagrange extended;

second of the eccentricities and inclinations. Mr. Lagrange, struck with this conclusion, endeavoured to extend it; and proved by a curious theorem, that the proposition was true, considering even all the successive powers of the eccentricities. But what would be the result, were the masses considered in terms of two dimensions? This inquiry demanded great labour, and no less acumen: yet Mr. Poisson undertook it, and demonstrated, that, if any acceleration exist, it can only depend on terms of four, six, or eight dimensions, and of course must be altogether imperceptible. As soon as Mr. Poisson had demonstrated his theorem, Mr. Lagrange and Mr. Laplace perceived, that it naturally flowed from principles and methods they had formerly laid down: in consequence they were both led to demonstrate the proposition more generally, but each in a different way.

and Poisson has carried still farther.

History of the sciences.

The physical division of the class presented to the emperor a sketch of the history of the sciences from the year 1789, which will soon be published.

Discoveries in chemistry.

The principal discoveries in chemical science are those to which Mr. Davy led the way, and which have been pursued in France chiefly by Messrs. Gay-Lussac and Thenard.

Minerals exposed to great heat under pressure

The experiments of Sir James Hall too have been repeated by Mr. Dree. Having exposed to fusion in close vessel., under irresistible pressure, fragments of rocks with trap or chert for their base, he found, that they assumed all the appearance of stony lavas; and that the crystals of feldspar in them were not altered, which explains the singular fact of so many very fusible crystals contained in lavas, that have rendered it questionable whether these lavas had ever been in a state of fusion.

contain crystals not fused.

Lamination of zinc,

The invention of the art of laminating zinc by heating it is claimed for the late Macquer and Mr. Sage, who practised it long ago: and Messrs. Dony and Poncelet, of the department of the Ourthe, have converted calamine simply by subliming it into metal sufficiently pure to be laminable. The ore affords them one third its weight of metal, which is much cheaper than lead.

and its extraction from the ore.

Acetic acid from wood.

Another successful application of chemistry to the arts is that of procuring from wood an acetic acid as pure as radical vinegar, the manufacture of which has been carried on

some

some time by Mr. Mollerat. It answers extremely well for aromatic vinegar; but possesses a little acrimony, on account of which it is not quite so fit for the table. The wood distilled for this purpose yields as much charcoal as in the ordinary way, and a great deal of tar.

Grape sugar.

In consequence of the interruption between France and the West Indies Mr. Proust and Mr. Parmentier have taken great pains to improve the extraction of sugar from grapes*.

Pyrometer.

Mr. Morveau has given a history of attempts to construct instruments to measure high degrees of heat, in which he does Wedgwood more justice, than he has generally received in France. He afterward describes an instrument of his own invention sufficiently delicate to indicate changes in a metallic bar that do not exceed a thirteenth thousandth part of its length. Such a bar of platina is the only thing sufficiently dilatable, and at the same time unalterable by fire, to serve properly for a pyrometer; but the difficulty is to place it on a scale, that will not dilate. This Mr. de Morveau hopes soon to accomplish.

Law respecting the proportion of metal and oxigen in metallic salts.

Mr. Gay-Lussac has just explored a beautiful law of general chemistry on the proportion of metal, that enters into each metallic salt, and that of oxigen necessary for its oxidation. He has shown, that a metal, which precipitates another from an acid solution, finds in the metal precipitated all the oxigen necessary for it to become oxided, and dissolve in such a quantity, that the solution shall be neutralized to the same degree. The quantity of oxigen remains constant, whatever be the proportion necessary to each metal: and the acid in each salt is proportionate to the oxigen of the oxide, and requires so much more metal to saturate it, in proportion as the metal requires less oxigen for its oxidation. This law affords a very simple method of determining the composition of all metallic salts; for it is sufficient to know the proportion of acid in one salt of each genus, to be acquainted with all; and a single analysis will allow us to dispense with the rest.

Soda and potash cannot be freed from water.

Mr. Darcet jun. has shown, that soda and potash, prepared with alcohol and heated to the point at which they

* See Journal, vol. XXI, p. 306, and 341.

begin

begin to evaporate, notwithstanding still retain nearly a third of their weight of water.

Animal mucus and uree.

Messrs. Fourcroy and Vauquelin have presented two important memoirs, one on animal mucus, the other on uree.

Structure of the brain and nervous system.

Among the anatomical subjects, that have engaged the attention of the class, few are so interesting as the memoir on the structure of the brain and nervous system by Drs. Gall and Spurzheim of Vienna. According to these gentlemen, the cinereous or cortical substance is the organ, from which issue the nervous filaments, that constitute the white medullary substance. Wherever the cinereous substance exists, some of these filaments originate; and wherever any of these filaments commence, this substance will be found. The spinal marrow is not a bundle of nerves descending from the brain: on the contrary the nerves termed cerebral may be traced to the medulla oblongata or spinalis; and the brain and cerebellum themselves are but developements of fasciculi from the medulla oblongata, in the same manner as the nerves come from it. The committee have found almost all the anatomical observations of Drs. G. and S. agreeable to nature: but they think it proper to add, that this has no connection whatever with Dr. Gall's theory of the appropriation of different part of the brain to the different functions of the mind.

Analogy of structure in animals.

Prof. Duméril has considered in new points of view the bones and muscles of the trunk in man and various animals. The grand principle he seeks to establish is, that nature is as uniform as possible in her means, continuing the same through numerous varieties, as long as they are effective, and never adding a new organ, unless when new circumstances require greater efforts and more powerful means.

Coats of the nerves composed of nervous filaments.

Mr. Villars of Strasburg, has presented two papers on the structure of the nerves. He thinks he has perceived, by means of the microscope, that the covering of the nerves is itself composed of nervous filaments: but the committee, notwithstanding they have taken great pains to ascertain this, could not satisfy themselves of the fact.

Mirbel,

The anatomy of plants is indebted for many new and important facts to the researches of Mr. Mirbel. The Royal Society of Gottingen, having made this anatomy a sub-

a subject of one of its annual prizes, has occasioned the publication of several tracts, the principal of which are those of Link, Treviranus, and Rudolph, all professors in different German universities, Agreeing in most facts with Mirbel, they not only add some observations to his, but contradict him on certain points; which has induced him to publish a defence of his theory, in which he gives it more precision, exhibiting it in the form of aphorisms; while he endeavours to show, that most of the objections arise from his having been misunderstood, or his observations not having been repeated with sufficient care.

contradicted in some points,

has defended his theory.

Mr. Mirbel has likewise presented to the class two papers, one on the germination of the family of grasses, the other on the distinguishing characteristics of the monocotyledonous and dicotyledonous plants.

In the first he shows, that the stigmata of wheat unite in a small canal, which reaches to the base of the embryo; and that the cotyledon, as Jussieu thought, is a fleshy substance, in which the radicle and plumula are imperceptibly developed, and which opens lengthways to let them pass, so that it performs the office of a vaginating leaf.

Germination of grasses.

From the second it appears, that the cotyledons have great analogy to the leaves, those of the sensitive plant being irritable, of the borages hairy, &c.; in short, they are true *leaves in the seed*. If, when there are two cotyledons, they appear opposite in plants the leaves of which are alternate, it is because the stalk cannot develope itself in the seed, and the interval between the cotyledons is not to be distinguished. From the different perceptible analogies between them, Mr. M. infers, that the number of the cotyledons must refer to some circumstance respecting the leaves; and he imagines, that the monocotyledonous plants are uniformly those, the leaves of which ensheath each other. Proceeding to examine the formation of the wood, Mr. M. shows, that it is always composed of filaments interspersed in a cellular texture resembling the medulla of the dicotyledons; but that in many of the monocotyledons these filaments are formed at the circumference as well as in the centre: the latter in consequence having a double vegetation; one at the circumference, increasing the diameter

Cotyledons bear great analogy to leaves.

Monocotyledonous plants.

Wood.

meter of the trunk; the other at the centre augmenting its density. He considers each of the filaments of the trunk of the monocotyledons as if it answered to an entire trunk of a dicotyledon; and shows, that in each of these filaments a series of operations takes place as complete as in those trunks.

Mirbel elected to the Institute. Mr. Mirbel, in consequence of his various labours toward illustrating the physiology of plants, was elected to the place vacant by the death of Mr. Ventenat.

Decandolle his competitor. The competitor of Mr. Mirbel for the vacancy in the Institute was Mr. Decandolle, who, beside his previous titles to it, had sent the class early in the year a work on plants with compound flowers, in which he makes a separate family of those the florets of which have two unequal lips, and distributes those termed cinerocephalous according to the lateral or terminal insertion of the seed. It was thought however, that his talents would be more useful in the celebrated school, in which he teaches botany, and at the head of the fine garden under his care, in a climate more favourable to the vegetation of foreign plants than the vicinity of Paris.

Plants with compound flowers.

Botany much cultivated in France. This sitting showed in general, that botany is cultivated in France with more ardour than ever. The Memoir on the Family of Orchideæ, by Mr. du Petit Thouars, a specimen of a greater work on the natural families of plants, with those of Mr. de Longchamp on Narcisausses, Mr. Jaume St. Hilaire on the Orobanches, and Mr. de Cubières on the Lote trees, and the Monography of Eringums by Mr. de la Roche, are proofs of this.

Developement of the bud. Mr. du Petit Thouars in particular has determined to publish his Theory of Vegetation, founded on the developement of the bud in two directions, which was noticed in our former report, vol. XXIII, p. 315.

New family of plants. Mr. Ventenat himself terminated his laborious career by a paper on the Genera Samyda and Casearia, of which he makes a new family next to that of the rhamnoides. This piece was intended for the continuation of the *Jardin de Cels*, a work interrupted by his death. He lived long enough to carry to some extent, though not to finish, his Description of the Garden of Malmaison, which no doubt will be continued by some other hand. The

Jardin de Cels.

Garden of Malmaison.

The history of animals has witnessed the completion of Mr. Olivier's grand work on coleopterous insects, and is enriched with a description of all the gelatinous animals included under the name of medusa by Linnæus. Mr. Péron, who collected a great number in his voyage to the south, has increased this family to more than a hundred and fifty species. The following is his account of their singularities. "Their substance seems to be merely a coagulated water, yet the most important functions of life are exercised in it. Their multiplication is prodigious, yet we know nothing of the peculiar mode in which it is effected. They are capable of attaining several feet in diameter, and fifty or sixty pounds in weight, yet their nutritive system escapes our eyes. They execute the most rapid and long continued movements, yet the details of their muscular system are imperceptible. They have a very active species of respiration, the true seat of which is a mystery. They appear extremely feeble, yet fish of considerable size form their daily prey, and dissolve in a few moments in their stomach. Many species of them shine amid the darkness of night like balls of fire; and some sting or benumb the hand that touches them: yet the principles and agents of both these properties remain to be discovered."

Olivier's Coleopteræ finished.

Medusæ.

All the medusas have a gelatinous body, nearly resembling the cap of a mushroom, which Mr. P., after the example of Spallanzani, names *umbrella*; but they differ in wanting or having a mouth; in the mouth being simple or multiplicious; in the presence or absence of a production resembling a pedicle; and in the edges of this pedicle, or of the mouth itself, being furnished with tentacula, or filaments more or less numerous. From these characters Mr. P. forms divisions and subdivisions, under which every possible kind of medusa may be arranged. Very fine paintings by Mr. Lesueur, who accompanied him on the voyage, illustrate the various forms and colours of these animals, many of which are very pleasing to the eye.

Specific characters.

To this examination of their external characters, Mr. P. has added very interesting remarks on the interior structure of these animals; and in particular of that genus, which Mr. Cuvier named *rhizostome*, because he supposed, that the

Their interior structure.

the filaments bordering its tentacula were so many suckers; and that the nourishment drawn in by them was received into a central cavity, whence it was distributed to the whole body by an infinite number of vessels disposed with great regularity, and particularly numerous about the edges of the umbrella. The four apertures at the sides of the base of the pedicle appeared to Mr. Cuvier to be the organs of respiration. Mr. P. on the contrary, having seen many living rhizostomes take in small animals by these four apertures, and digest them in the four cavities to which they lead, presumes that they are four mouths, and as many stomachs; while the great vascular apparatus, that fills the pedicle and the borders of the umbrella, is more probably appropriated to respiration, as it is almost always found full of air.

Skeletons of animals found in the earth.

Mr. Cuvier read a paper on certain reptiles, the skeletons of which are found in strata of our globe. These had all been taken for crocodiles, and even for the species common in the Ganges, the *gavial;* but the lacerta monitor is also among them, and those that most resemble the *gavial* have striking characteristics to distinguish them. All of them are found in strata much deeper, and consequently more ancient, than those that contain bones of land quadrupeds. The environs of Maestricht conceal the bones of a large animal of this family, which some have taken for a crocodile, others for a fish. Mr. C. attempted to show, that this also was a lacerta monitor, but it is the giant of its kind. It measures in length upward of eight metres [26 feet]. Its tail, much shorter in proportion, but broader, than that of other species, formed a powerful oar; and every thing renders it probable, that it had sufficient strength, and was so good a swimmer, as to live amid the waves of the ocean. Its bones too are found with those of large sea turtle, and among thousands of sea shells.

Bones of a large monitor lizard, 26 feet long.

An inhabitant of the sea.

Fossile bones from America.

Mr. Jefferson, President of the United States, has sent the class a fine collection of fossil bones dug up on the banks of the Ohio. The greater number belong to the large animal improperly called mammoth by the Americans, and to which Mr. Cuvier has given the name of mastodonte: but there are likewise some belonging to the true mammoth of

of the Russians, or the other large animal, much resembling the Indian elephant, the remains of which are so common in Siberia. These two gigantic creatures therefore formerly inhabited together all the northern cap of our globe. The destruction of these enormous races, and of so many others, victims of the same catastrophe, cannot be explained, till we are well acquainted with the strata in which they are buried, as well as their nature and succession.

Strata in the vicinity of Paris.

Mr. Cuvier and Mr. Brongniart have endeavoured to study these in the neighbourhood of Paris. As far as they have been able to penetrate into the earth round that capital, they have found it composed of various strata evidently of different origin. The lowest part is a vast mass of chalk, that reaches to England, and contains nothing but unknown shells, several of which belong even to unknown genera. On this chalk rests a bed of potter's clay, containing no organized body. This in several places is covered by limestone, the hardest of which is used for building, and which is full of shells, most of them of unknown species, but of known genera, or approaching nearer than the preceding to those that live in our present seas. Hills of gypsum are scattered as if by accident sometimes on the clay, at others on the limestone, and contain thousands of bones of land animals entirely unknown, of which Mr. Cuvier has put together the skeletons, and established the characters. In this gypsum, and the clay intermixed with it, or immediately covering it, there are no shells but fresh water ones: but these are afterward covered with thick strata of sea shells. A vast bed of sand, without any organized bodies, crowns all our heights; and, what is most remarkable of all, the most superficial stratum, that which covers the whole, is mixed with fresh water shells alone. It is only in the bottoms of valleys, or in cavities hollowed out of this superficial stratum, that are found the bones of elephants and other animals, the *genus* of which is known, but not the species.

The land there long covered by the sea, afterward with

From the observations of these gentlemen it appears, that the sea, having long covered this country, and several times changed its nature and inhabitants, gave place to fresh

fresh water, and once or twice again by the sea.

fresh water, in which these gypsums were deposited; but that it returned at least once to cover the land it had abandoned, and destroy the beings that had lived on it. On this occasion perished the palæotheria and the anoplotheria. Every thing renders it probable however, that it returned a second time, and that the elephants disappeared in this second catastrophe.

Petrifaction.

Mr. Sage presented to the class a ferruginous petrifaction, having some appearance of a bundle of tobacco leaves tied round with threads, but probably part of a stalk of bamboo, or some other jointed plant. He likewise gave descriptions and analyses of a few ſtones; and communicated some experiments on the cohesion lime contracts with various substances.

Transition strata.

Mr. Brochant, mine engineer, presented some observations on strata much more ancient than those in the vicinity of Paris, which Werner has called transition strata, because they are placed between the primitive mountains, anterior to all organization, and the secondary strata, that abound with remains of animals. Most of them are composed of fragments of the primitive rocks, united into breccias by cements of various kinds, in which we begin to perceive occasionally remains of organized substances, either vegetable or animal. Saussure had already noticed these in the

Alps.

Alps, but Mr. B. has traced them to much greater extent, principally along that side of the Alps which looks toward France.

Climate of Genoa.

Mr. Lescallier has shown, that the climate of Liguria is more favourable to the plants of hot countries, than any other in the same latitude: the winter, though longer, not being so cold, because the Apennines shelter it from the north wind; while the summer is less scorching, from the vicinity of the sea on one hand, and the snows on the other.

Department of the Doubs.

Mr. Girod-Chantrans has given the natural history of the department of the Doubs.

Albumen a remedy against intermittents.

Mr. Seguin, who formerly found gelatine the true remedy against intermittent fevers*, has this year tried albumen with good success. He has already cured forty-one patients, by giving them the whites of three eggs diluted with warm

* See Journal, vol. VI, p. 158, and XIII, p. 205.

water,

water, and sweetened with sugar, just before the fit comes on. He says this convenience attends both these remedies, if the fit that follows the first dose be not mitigated, you must not expect a cure from them; if it be, perseverance in them will succeed†.

Messrs. Cels, Tessier, and Huzard, have drawn up a scheme for a Code of Rural Law, the object of which is to protect landed property from every imaginable injury. It is transmitted to a select committee in every department for examination. Code of rural laws.

Mr. Tessier has drawn up, by order of government, popular instructions for the cultivation of cotton in France. Cultivation of cotton in France.

Mr. Bosc has described twenty-eight species of the ash, half of which, though cultivated in the gardens and nurseries round Paris, have not been noticed by naturalists. Some of them are large trees, superior in elasticity and flexibility to the common ash. Superior species of ash.

St. Thomas's and Guy's Hospitals.

The Winter Courses of Lectures at these adjoining Hospitals will commence as usual, the beginning of October. Viz. Lectures

At St. Thomas's.

Anatomy and the Operations of Surgery. By Mr. CLINE and Mr. COOPER. at St. Thomas's,

Principles and Practice of Surgery. By Mr. COOPER.

At Guy's.

Practice of Medicine. By Dr. BABINGTON and Dr. CURRY. Guy's,

Chemistry. By Dr. BABINGTON, Dr. MARCET, and Mr. ALLEN.

Experimental Philosophy. By Mr. ALLEN.

Theory of Medicine, and Materia Medica. By Dr. CURRY and Dr. CHOLMELEY.

Midwifery, and Diseases of Women and Children. By Dr. HAIGHTON.

Physiology, or Laws of the Animal Œconomy. By Dr. HAIGHTON.

Structure and Diseases of the Teeth. By Mr. FOX.

N.B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a *Complete Course of Medical and Chirurgical Instructions.* Terms and other particulars may be learnt at the respective Hospitals.

London Hospital.

Dr. BUXTON's Autumnal course of Lectures on the Theory and Practice of Medicine will commence on the 2d of October at the Medical Theatre. and the London Hospital.

† Have we not here a clew to the presumed success of such apparently inert remedies? C.

METEOROLOGICAL JOURNAL,

For AUGUST, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker in the STRAND, LONDON.

JULY Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
26	62	64	70	61	29·75	Rain	Cloudy*
27	63	66	69	61	29·73	Fair	Ditto
28	62	59	65	55	29·64	Rain	Fair
29	59	58	64	58	29·73	Ditto	Rain
30	60	59	64	56	29·60	Ditto	Fair
31	58	58	66	57	29·55	Fair	Ditto
AUG.							
1	61	62	66	57	29·66	Ditto	Cloudy
2	60	62	66	60	29·72	Ditto	Ditto
3	60	57	62	51	29·40	Rain	Fair
4	53	55	59	51	29·41	Ditto †	Cloudy
5	53	58	61	57	29·76	Ditto	Rain
6	61	59	63	55	29·38	Ditto	Ditto
7	58	59	64	56	29·65	Ditto	Fair
8	63	64	68	58	29·94	Fair	Ditto
9	64	65	71	58	29·94	Ditto	Ditto
10	62	70	74	60	29·89	Ditto	Cloudy ‡
11	61	61	73	60	29·68	Ditto	Fair
12	63	57	68	59	29·67	Rain	Ditto
13	64	63	68	60	29·79	Ditto	Ditto
14	64	63	69	58	29·80	Ditto	Ditto
15	61	61	63	58	29·75	Ditto	Cloudy
16	61	63	67	60	29·88	Ditto	Fair
17	63	64	76	58	29·84	Fair §	Ditto ‖
18	60	61	71	59	29·78	Rain ¶	Ditto
19	63	61	68	57	29·80	Ditto	Ditto
20	61	61	66	56	29·96	Ditto	Ditto
21	60	58	64	52	29·84	Ditto	Ditto
22	58	57	67	52	29·84	Ditto	Ditto
23	59	57	67	50	29·62	Fair	Ditto
24	58	56	64	48	29·49	Rain	Ditto
25	55	50	63	48	29·43	Ditto *	Ditto

* Thunder, lightning, and rain in the evening, the moon bright at intervals.
† Rainy and cold, almost the whole day.
‡ Thunder, lightning, and rain in the night.
§ Sultry morning.
‖ Lightning in the East, at 11 P.M. very dark and appearance of rain.
¶ Heavy rain in the morning.
* Thunder, at 1 and 3 P.M.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1809.

ARTICLE I.

Further Application of a Series to the Correction of the Height of the Barometer.

To Mr. NICHOLSON.

SIR,

IN calculating a table of the depression of the mercury in the tube of a barometer, produced by the effect of capillary attraction, I have found it necessary to determine a greater number of the coefficients of the series published in your twenty-second volume, p. 213. The value of f is found $14\,b^9 + \frac{21\,b^7}{5\,m} + \frac{529\,b^5}{1920\,m^2} + \frac{163\,b^3}{57600\,m^3} + \frac{b}{3686400\,m^4}$; and g, if I have computed correctly, is equal to $42\,b^{11} + \frac{385\,b^9}{24\,m}$ $+ \frac{129721\,b^7}{69120\,m^2} + \frac{983\,b^5}{23040\,m^3} + \frac{5197\,b^3}{33177600\,m^4} + \frac{b}{530841600\,m^5}$. Here it may be observed, that the numerical coefficients of the

Continuation of a former series.

the highest powers of b form this series, $\frac{2}{2}$, $\frac{6}{3}$, $\frac{6 \times 10}{3 \times 4}$, $\frac{6 \,.\, 10 \,.\, 14}{3 \,.\, 4 \,.\, 5}$, which may be continued at pleasure, and which must obviously express the versed sine of a circular arc, since, when b becomes infinite, the curve coincides with a circle, and the highest powers of b must in this case be infinitely greater than all the rest: the coefficients of the powers of b next in order form this series, $\frac{1}{4 \,.\, 4}$, $\frac{10}{6 \,.\, 6}$, $\frac{10}{8 \,.\, 8} \,.\, \frac{14}{2}$, $\frac{10}{10 \,.\, 10} \,.\, \frac{14}{2} \,.\, \frac{18}{3}$, $\frac{10}{12 \,.\, 12} \,.\, \frac{14}{2} \,.\, \frac{18}{3} \,.\, \frac{22}{4}$, and those of the other orders of terms seem to follow a law nearly similar, but which I have not fully ascertained. The coefficients of the terms including the lowest powers of b are however of more consequence; the progression of the coefficients of b is sufficiently obvious: those of b^3 have their denominators increased according to the same law, and the ratio of the numerators approximates to 8, and differs so little from it, that this number may be employed as a multiplier without sensible errour: and in a similar manner if the denominators of the coefficients of b^5 are made to increase in the same ratio, the numerators approach nearer and nearer to the ratio of 16 to 1, which is probably their ultimate proportion: and in some instances the continuation of the progressions on these principles is required for a sufficiently accurate determination of the quantities concerned.

Arrangement of the series.

For calculating the depression or elevation of a fluid in a tube of a given diameter, it is convenient to arrange the series according to the powers of b; so that the whole assumes this form, $a =$

(2 M 2

$$
\begin{aligned}
&(2\,m\,x \\
&+ \cdot 25\,x^3 \\
&+ \cdot 010\,4167\,\frac{x^5}{m} \\
&+ \cdot 000\,217\,014\,\frac{x^7}{m^2} \\
&+ \cdot 000\,002\,712\,67\,\frac{x^9}{m^3} \\
&+ \cdot 000\,000\,022\,605\,6\,\frac{x^{11}}{m^4} \\
&+ \cdot 000\,000\,000\,134\,557\,\frac{x^{13}}{m^5} \\
&+ \cdot 000\,000\,000\,000\,600\,8\,\frac{x^{15}}{m^6} \\
&+ \cdot 000\,000\,000\,000\,002\,086\,\frac{x^{17}}{m^7} \\
&+ \cdot 000\,000\,000\,000\,000\,0058\,\frac{x^{19}}{m^8} \\
&+ \ldots)\,b
\end{aligned}
$$

$$
\begin{aligned}
&+(\cdot 166\,667\,x^5 \\
&+ \cdot 034\,722\,2\,\frac{x^7}{m} \\
&+ \cdot 003\,559\,03\,\frac{x^9}{m^2} \\
&+ \cdot 000\,235\,822\,\frac{x^{11}}{m^3} \\
&+ \cdot 000\,011\,887\,\frac{x^{13}}{m^4} \\
&+ \cdot 000\,000\,399\,6\,\frac{x^{15}}{m^5} \\
&+ \cdot 000\,000\,011\,1\,\frac{x^{17}}{m^6} \\
&+ \cdot 000\,000\,000\,247\,\frac{x^{19}}{m^7} \\
&+ \ldots)\,b^3
\end{aligned}
$$

$$
\begin{aligned}
&+(\cdot 25\,x^7 \\
&+ \cdot 109\,375\,\frac{x^9}{m} \\
&+ \cdot 022\,960\,1\,\frac{x^{11}}{m^2} \\
&+ \cdot 003\,047\,49\,\frac{x^{13}}{m^3} \\
&+ \cdot 000\,245\,00?\,\frac{x^{15}}{m^4} \\
&+ \cdot 000\,014\,40?\,\frac{x^{17}}{m^5} \\
&+ \cdot 000\,000\,69?\,\frac{x^{19}}{m^7} \\
&+ \ldots)\,b^5
\end{aligned}
$$

$$
\begin{aligned}
&+(\cdot 5\,x^9 \\
&+ \cdot 35\,\frac{x^{11}}{m} \\
&+ \cdot 134\,\frac{x^{13}}{m^2} \\
&+ \ldots)\,b^7 \\
&+(\cdot 1 \cdot 167\,x^{11} \\
&+ 1 \cdot 146\,\frac{x^{13}}{m} \\
&+ \ldots)\,b^9
\end{aligned}
$$

$$+ 3\,x^{13}\,b^{11} + \ldots$$

The value of b being determined by the solution of this equation for any given tube, of which the semidiameter is Expansion.

G 2 x,

x, that of y may be found from the original series, which may be thus expanded,

$$
\begin{aligned}
y = (&4\,m \\
&+ x^2 \\
&+ \cdot 0625\,\frac{x^4}{m} \\
&+ \cdot 001736\;11\,\frac{x^6}{m^2} \\
&+ \cdot 000\;027\;1267\,\frac{x^8}{m^3} \\
&+ \cdot 000\;000\;271\;267\,\frac{x^{10}}{m^4} \\
&+ \cdot 000\;000\;001\;88380\,\frac{x^{12}}{m^5} \\
&+ \cdot 000\;000\;000\;009\;611\;21\,\frac{x^{14}}{m^6} \\
&+ \cdot 000\;000\;000\;000\;037\;54\,\frac{x^{16}}{m^7} \\
&+ \cdot 000\;000\;000\;000\;000\;115\;9\,\frac{x^{18}}{m^8} \\
&+ \cdot 000\;000\;000\;000\;000\;000\;29\,\frac{x^{20}}{m^9} \\
&+ \cdot 000\;000\;000\;000\;000\;000\;000\;6\,\frac{x^{22}}{m^{10}} \\
&+ \ldots)\,b
\end{aligned}
$$

$$
\begin{aligned}
+ (&x^4 \\
&+ \cdot 277\;778\,\frac{x^6}{m} \\
&+ \cdot 035\;590\;3\,\frac{x^8}{m^2} \\
&+ \cdot 002\;829\;86\,\frac{x^{10}}{m^3} \\
&+ \cdot 000\;156\;642\,\frac{x^{12}}{m^4} \\
&+ \cdot 000\;006\;393\;4\,\frac{x^{14}}{m^5}
\end{aligned}
$$

$+ \cdot 000$

$$+ \cdot 000\,000\,200 \frac{x^{16}}{m^6}$$
$$+ \cdot 000\,000\,004\,94 \frac{x^{18}}{m^7}$$
$$+ \cdot 000\,000\,000\,10 \frac{x^{20}}{m^8}$$
$$+ \cdot 000\,000\,000\,0016 \frac{x^{22}}{m^9}$$
$$+ \ldots)\, b^3$$

$$+ (2\, x^6$$
$$+ 1\cdot 09\,375 \frac{x^8}{m}$$
$$+ \cdot 275\,514 \frac{x^{10}}{m^2}$$
$$+ \cdot 042\,665 \frac{x^{12}}{m^3}$$
$$+ \cdot 003\,920\,? \frac{x^{14}}{m^4}$$
$$+ \cdot 000\,261\,? \frac{x^{16}}{m^5}$$
$$+ \cdot 000\,0137\,? \frac{x^{18}}{m^6}$$
$$+ \cdot 000\,000\,55\,? \frac{x^{20}}{m^7}$$
$$+ \cdot 000\,000\,018\,2\,? \frac{x^{22}}{m^8}$$
$$+ \ldots)\, b^5$$

$$+ (5\, x^8$$
$$+ 4\cdot 2 \frac{x^{10}}{m}$$
$$+ 1\cdot 87675 \frac{x^{12}}{m^2}$$
$$+ \ldots)\, b^7$$
$$+ (14\, x^{10}$$
$$+ 16\cdot 04167 \frac{x^{12}}{m}$$
$$+ \ldots)\, b^9$$
$$+ 42\, x^{12}\, b^{11} + \ldots$$

Table of the depression of mercury.

In this manner the following table has been calculated, m being still made ·005, and $n = \cdot 00375$; and it may in general be considered as accurate to the last place of the decimals laid down: the depression is also determined according to the experiments of Mr. Gay Lussac, in which m appeared to be ·0051, n being still ·00375. In applying the correction to a given barometer, the bore might be ascertained by measuring the difference of the central and marginal depressions with a micrometer, and comparing it with this table, without the trouble of emptying the tube.

Diameter

Diameter.	Central depression. $m=\cdot005$	Central depression. $m=\cdot0051$	Central depression. Observed by Ld. C. C.	Marginal depression. $m=\cdot005$	Difference.
1·00	·00031	·00032			
·90	·00060	·00062			
·80	·00115	·00118			
·70	·00220	·00224			
·60	·00411	·00416	·005	·0637	·0596
·50	·00799	·00805	·007	·0676	·0596
·45	·01100	·01106		·0690	·0580
·40	·01516	·01522	·015	·0714	·0562
·35	·02093	·02098	·025	·0745	·0536
·30	·02902	·02906	·036	·0787	·0497
·25	·04064	·04067	·050	·0850	·0444
·20	·05800	·05802	·067	·0966	·0386
·15	·08620	·08621	·092	·1171	·0309
·10	·14027	·14027	·140	·1619	·0216
·05	·29497	·29497		·3060	·0110

Elevation of water.

When it is required to continue the curve till it becomes perpendicular to the absciss, it is evident that the series cannot be sufficiently accurate, since in this case the least imaginable increase of the absciss would afford an impossible value for the ordinate. It is therefore convenient to compute the value of b and y for a portion of the curve a little less than that which is required, and to determine the length of the remainder from its mean curvature, deduced from the magnitude of the ordinate, together with that of the absciss. For example, if it be required to find the central and marginal elevation of the surface of water contained in tubes 1 inch, $\frac{1}{2}$, and $\frac{1}{4}$ of an inch in diameter, taking $m = \cdot01$; we may continue the curve till its inclination to the horizon becomes 60°, and $\frac{n}{m} = \cdot866$; but we must first determine the corresponding diminution of the diameter, in order to obtain the value of x. For this purpose the part of the curve which is nearly vertical may be compared with a cubical parabola, the distance of which from its tangent is to the versed sine of the osculating circle, as the distance from the vertex, diminished by one third of the tangent, to the whole distance. In the first example, taking the marginal elevation by conjecture ·15, we must deduct ·02, the height corresponding to the horizontal curvature, of which the

the radius is ·5, and to find the mean radius of the arc of 30°, we have $2s(\cdot 13 - \frac{1}{2}s) = \cdot 01$, $ss - \cdot 26s = -\cdot 01$ $s = \cdot 13 - \sqrt{\cdot 0069} = \cdot 0463$: but the versed sine of the arc of which this is the sine, in the circle of curvature at the vertical point, is ·01552, which is to be diminished in the ratio of ·13 — ·0154 to ·13, and becomes ·01371; and deducting this from ·5, we have ·4863 for the value of x, when $n = \cdot 00866$. Hence we find $b = \cdot 0948$, $a = \cdot 0038$, and the marginal elevation ·151, which is so near the assumed value, that no further correction is required. In the same manner, for tubes of $\frac{1}{2}$ and $\frac{1}{4}$ inch in diameter, we find $a = \cdot 0374$, and ·130, and the marginal elevation ·162 and ·220 respectively. But it would be rather more accurate to compute the extent of a portion of the curve, somewhat greater than 60°, by means of the series.

We may also obtain a series, in a manner nearly similar, for determining the relation of the arc to the absciss and the ordinate; and such a series must represent the properties of the curve in a more general manner, and may, in some cases, be more convenient for calculation, at the same time that it affords a mode of verifying the results which we have already obtained. Taking the expression $\int xy\dot{x}\sqrt{(\dot{x}^2+\dot{y}^2)} = mx\dot{y}$, we may put $\dot{x}^2+\dot{y}^2=\dot{z}^2$, $x = z + Az^3 + Bz^5 + Cz^7 + \ldots$, and $y = a + bz^2 + cz^4 + dz^6 + \ldots$; then $\frac{\dot{x}^2}{\dot{z}^2} + \frac{\dot{y}^2}{\dot{z}^2} = 1$; but $\frac{\dot{x}^2}{\dot{z}^2} = 1 + 6Az^2 + (10B + 9A^2)z^4 + (14C + 30AB)z^6 + (18D + 42AC + 25B^2)z^8 + \ldots$, and $\frac{\dot{y}^2}{\dot{z}^2} = 4b^2z^2 + 16bcz^4 + (24bd + 16c^2)z^6 + (32be + 48cd)z^8 + \ldots$; whence, by comparing the homologous terms, $A = -\frac{2}{3}b^2$, $B = \frac{-16bc - 9AA}{10}$, $C = \frac{-24bd - 16cc - 30AB}{14}$, and $D = \frac{-32be - 48cd - 42AC - 25BB}{18}$. Again, for the fluent $\int xy\dot{x}$, we have $xy = az + (aA + b)z^3 + (aB + bA + c)z^5 + \ldots$, $\dot{x} = \dot{z} + 3Az^2\dot{z} + 5Bz^4\dot{z} + \ldots$, and $\int xy\dot{x} = a\frac{z^2}{2} + (aA + b + 3Aa)$

z^4

Series in terms of the arc.

$\frac{z^4}{4} + (a\,B + b\,A + c + 3\,A\,(a\,A + b) + 5\,B\,a)\,\frac{z^6}{6} +$ ..., which must be equal to $m\,x\,\frac{\dot{y}}{\dot{z}}$, or to $2\,m\,b\,z^3 +$ $(4\,m\,c + 2\,m\,b\,A)\,z^4 + (6\,m\,d + 4\,m\,c\,A + 2\,m\,b\,B)\,z^6$ $+ \ldots$; whence $b = \frac{a}{4\,m}$, $c = \frac{a\,A + b + 3\,A\,a - 8\,m\,b\,A}{16\,m}$ and $d = \frac{a\,B + b\,A + c + 3\,A\,(a\,A + b) + 5\,B\,a - 24\,m\,c\,A - 12\,m\,b\,B}{36\,m}$: and, by reduction, $c = \frac{b}{16\,m} - \frac{b^3}{3}$, $d = \frac{b}{576\,m^2} - \frac{4\,b^3}{45\,m} + \frac{2\,b^5}{45}$, $e = \frac{b}{36864\,m^3} - \frac{302\,b^3}{26880\,m^2}$ $+ \frac{122\,b^5}{4032\,m} - \frac{b^7}{315}$, and $A = -\frac{2}{3}\,b^2$, $B = -\frac{b^2}{10\,m} + \frac{2\,b^4}{15}$, $C = -\frac{5\,b^2}{672\,m^2} + \frac{2\,b^4}{35\,m} - \frac{4\,b^6}{315}$, and $D = \frac{-7\,b^2}{20736\,m^3} +$ $\frac{493\,b^4}{45360\,m^2} - \frac{b^6}{70\,m} + \frac{2\,b^8}{2835}$. Here we may observe, that in the series for finding the value of y, the coefficients of the terms involving the lowest powers of b are the same as in the former case, and that there is a similar approximation to the ratios of 8 and 16 in the neighbouring terms, so that we may safely continue the series on these foundations: the coefficients of the highest powers may be found by this progression, $1, \frac{4}{3\,.\,4}, \frac{4}{3\,.\,4}\,.\,\frac{4}{5\,.\,6}, \frac{4}{3\,.\,4}\,.\,\frac{4}{5\,.\,6}\,.\,\frac{4}{7\,.\,8}$, which, for the reason already mentioned, must represent the versed sine of a circular arc. In the series for x, the coefficients of the first terms form this progression; $\frac{2}{3}, \frac{2}{5}\,.\,\frac{3}{12}, \frac{2}{7}\,.\,\frac{3}{12^2}\,.\,\frac{5}{4}, \frac{2}{9}\,.\,\frac{3}{12^3}\,.\,\frac{5}{4^2}\,.\,\frac{7}{5}\,.\,2, \frac{2}{11}\,.\,\frac{3}{12^4}\,.\,\frac{5}{4^3}\,.\,\frac{7}{5^2}\,.\,\frac{9}{6}\,.\,2^2\,.\,3,$ $\frac{2}{13}\,.\,\frac{3}{12^5}\,.\,\frac{5}{4^4}\,.\,\frac{7}{5^3}\,.\,\frac{9}{6^2}\,.\,\frac{11}{7}\,.\,2^3\,.\,3^2\,.\,4$, or $\frac{2}{3}\,.\,\frac{1}{1}, \frac{2}{5}\,.$ $\frac{3}{2\,.\,2\,.\,3}, \frac{2}{7}\,.\,\frac{3\,.\,5}{2\,.\,2\,.\,3\,.\,2\,.\,2\,.\,3\,.\,4}, \frac{2}{9}\,.\,\frac{3\,.\,5\,.}{2\,.\,2\,.\,3\,.\,2\,.\,2\,.\,3}$

7 . 2

$$\frac{7.2}{.4.2.2.3.4.5}, \quad \frac{2}{11}.\frac{3.5.7.2.2.3}{2.2.3.2.2.3.4.2.2.3.4.5}$$

$\overline{.2.2.3.4.5.6}$: and those of the last terms are $\frac{2}{3}$, $\frac{2}{3}.\frac{4}{4.5}$, $\frac{2}{3}.\frac{4}{4.5}.\frac{4}{6.7}$, $\frac{2}{3}.\frac{4}{4.5}.\frac{4}{6.7}.\frac{4}{8.9}$, or $\frac{2}{3}$, $\frac{2^3}{3..5}$, $\frac{2^5}{3..7}$, $\frac{2^7}{3..9}$: and this series must obviously represent the sine of a circular arc, since all the other terms vanish in comparison with these, when b becomes infinite.

These series however have not the convenience of affording a fluent divisible by x the absciss, as in the former case, and the expression for the inclination of the curve is much less convergent: it may however be employed where great accuracy is not required. Since $\int x y \dot{x} = n x$, we find, from the first equation, $n x \dot{z} = m x \dot{y}$, and $\frac{\dot{y}}{\dot{z}} = \frac{n}{m}$, consequently $\frac{\dot{x}}{\dot{z}} = \sqrt{(1 - \frac{nn}{mm})}$, and the relation of z and b may be determined from either of the series, when n and x are given. The series themselves may be thus expanded. Inconveniences.

Expansion of the series.

$$x = z - (\cdot 666667\, z^3 + \cdot 10 \frac{z^5}{m} + \cdot 00744048 \frac{z^7}{m^2} + \cdot 000\,337\,577 \frac{z^9}{m^3} + \cdot 000\,010\,357\,5 \frac{z^{11}}{m^4} + \ldots)\, b^2$$
$$+ (\cdot 133333\, z^5 + \cdot 057\,1429 \frac{z^7}{m} + \cdot 010868\,5 \frac{z^9}{m^2} + \ldots)\, b^4 - (\cdot 012\,698\,4\, z^7 + \cdot 014\,285\,7 \frac{z^9}{m} + \ldots)\, b^6$$
$$+ \cdot 007\,054\,67\, z^9\, b^8 + \ldots - \cdot 000\,256\,533\, z^{11}\, b^{10} - \ldots + \cdot 000\,006\,577\,77\, z^{13}\, b^{12} + \ldots - \ldots$$

$y = (4\,m$

$$y = \left(4m + z^2 + {\cdot}0625\,\frac{z^4}{m} + {\cdot}001736\,11\,\frac{z^6}{m^2} + {\cdot}000\,027\,1267\,\frac{z^8}{m^3} + {\cdot}000\,000\,271\,267\,\frac{z^{10}}{m^4} + \ldots \text{ as above}\right) b$$

$$- \left({\cdot}33333\,z^4 + {\cdot}088\,889\,\frac{z^6}{m} + {\cdot}011\,235\,\frac{z^8}{m^2} + {\cdot}000\,891\,74\,\frac{z^{10}}{m^3} + {\cdot}000\,049\,48\,\frac{z^{12}}{m^4} + {\cdot}000\,002\,02\,\frac{z^{14}}{m^5} + {\cdot}000\,000\,063\,\frac{z^{16}}{m^6} + {\cdot}000\,000\,001\,6\,\frac{z^{18}}{m^7} + {\cdot}000\,000\,000\,031\,\frac{z^{20}}{m^8} + \ldots\right) b^3$$

$$+ \left({\cdot}044\,444\,z^6 + {\cdot}030\,258\,\frac{z^8}{m} + {\cdot}008\,4528\,\frac{z^{10}}{m^2} + {\cdot}001\,30?\,\frac{z^{12}}{m^3} + {\cdot}000\,130?\,\frac{z^{14}}{m^4} + {\cdot}000\,009?\,\frac{z^{16}}{m^5} + \ldots\right) b^5$$

$$+ \left({\cdot}003\,174\,6\,z^8 + {\cdot}006\,031\,7\,\frac{z^{10}}{m} + \ldots\right) b^7$$

$$+ {\cdot}0014109\,z^{10} + \ldots)\,b^9 + {\cdot}000\,042\,755\,z^{12} + \ldots)\,b^{11}$$

$$+ {\cdot}000\,000\,09344\,z^{14} + \ldots)\,b^{13} + {\cdot}000\,000\,015\,57\,z^{16} + \ldots)\,b^{15} + \ldots$$

Other parts of the curve.

The same mode of investigation may be applied to the more accurate determination of the properties of the curve at any other point of its extent; substituting $r - x$ for x, $q - y$ for y, and $p - f[(r - x) . (q - y)\,\dot{x}]$ for the fluent: but the calculations become considerably complicated. Thus, if we suppose z to begin where the curve is vertical, we have $x = b z^2 - \frac{z^3}{6\,m} - \frac{36\,m\,q\,b^2 - 9\,r + q\,r + 144\,m^2\,r\,b^4 - 144\,m^2\,b^3}{576\,m\,r\,b - 72\,m}\,z^4 + \ldots$, and $y = z - \frac{1}{3}\,b^2\,z^3 + \frac{b\,z^4}{4m} + \ldots$:

$+ \ldots$: or, if we express x in the powers of y, $x = b y^2 + \frac{2 b y^3}{3 q + 24 m b} + \ldots$. For example, in the case of water rising in a tube an inch in diameter, q being $\cdot 151$, and $b = \frac{q}{2 m} - \frac{1}{2 r} = 6\cdot 55$, we have, for an arc of 30°, Sin. 30° $= \cdot 5 = 13\cdot 1 z - 50 z^2 - 145\cdot 6 z^3$, whence $z = \cdot 05$, or perhaps $\cdot 0505$, $x = \cdot 0161$, and $y = \cdot 0483$. But for this value of y, x ought to be but about $\cdot 0153$: and this difference, as well as the numbers obtained from the properties of the cubic parabola, shows only that it would be better to extend the calculation to an arc of 70° or 80° by the first series, if great accuracy were required. According to Mr. Gay-Lussac's experiments, m is more correctly, in the case of water $\cdot 0115$; in that of pure alcohol $\cdot 0047$.

For a surface of simple curvature, the primary equation is $\int y \dot{x} \surd(\dot{x}^2 + \dot{y}^2) = m \dot{y}$, and the coefficients of the first series become $b = \frac{a}{2 m}$, $c = b^3 + \frac{b}{12 m}$, and $d = 2 b^5 + \frac{11 b^3}{30 m} + \frac{b}{360 m^2}$: the ratios of the last terms being $\frac{1}{5.6 m}$, $\frac{1}{7.8 m}$, and so forth: and $n = \frac{1}{3} b x^3 + \frac{1}{5} c x^5 + \frac{1}{7} d x + \ldots$: and from this series we may calculate the elevation of a fluid between two plane surfaces. Surface of simple curvature.

I am, Sir,

Your very obedient servant,

3 *Sept.* 1809. E. F. G. H.

II.

II.

On the Action of the Metal of Potash on Metallic Salts and Oxides, and on Alkaline and Earthy Salts. By Messrs. THENARD *and* GAY-LUSSAC*.

Muriatic acid not obtainable separate.

CONVINCED by a number of experiments, that it was not possible to obtain muriatic acid free from every other substance, we attempted to make the metal of potash act directly on muriates, in order to ascertain whether this acid would not by these means undergo some alteration.

Muriate of barytes exposed to the action of potassium.

No action took place.

For this purpose we took muriate of barytes fused at a red heat. We had powdered it, and introduced it into a tube of glass blown by the lamp, into which we had previously put a small ball of the metal; but no action took place, either cold or at a red heat; the metal passed through the salt without any perceptible alteration, and on throwing it into water, after the refrigeration of the matter, it inflamed very vividly. Other alkaline muriates did not afford us more satisfactory results. We then subjected to the same trial, in the same way, insoluble metallic muriates, as the muriate of silver, and mild muriate of mercury. Scarcely was the heat greater than sufficed to fuse the metal, when a very vivid inflammation was excited, and these two salts were reduced. In both reductions the tube was broken; and in that of the muriate of mercury, there was something like a slight detonation owing to the mercurial vapour. In both cases nothing was formed but muriate of potash, and no sign of the muriatic acid being decomposed was observed.

Other alkaline muriates.

Muriates of silver and mercury acted upon by potassium,

but the acid not decomposed.

Examination of the action of potassium on other salts and metallic oxides.

Having no farther hope of finding a mean of decomposing muriatic acid in experiments of this kind, we attempted to ascertain the action of the metal of potash on other salts, and on the metallic oxides, continuing to employ the same method of operating as before. In all our experiments the heat was constantly a little higher than was necessary to fuse the metal. Sometimes, as far as the decomposition of phos-

* Journal de Physique, January, 1809, p. 103.

phate

phate of lime, sulphate of barytes, oxide of zinc, &c. it was carried to near 300° of the centigrade thermometer [572° F.]. The tubes we used were always broken, when the inflammation was very vivid. To avoid minutiæ, we ſhall confine ourselves to the results we observed.

Sulphate of barytes. Decomposed without any inflammation, and sulphate of barytes obtained. On sulphate of barytes;

Sulphite of barytes. Vivid inflammation, and sulphuret of barytes formed. sulphite of barytes,

From these two experiments we ſhould infer, that the oxigen is much less condensed in the sulphite, than in the sulphate of barytes; and very probably also less condensed in sulphurous acid, than in sulphuric.

Sulphite of lime. Slight inflammation: formation of a very yellow sulphuret. and of lime;

Sulphate of lead. Very vivid inflammation. sulphate of lead,

Sulphate of mercury but little oxided. Inflammation as with the mild muriate. and of mercury;

Nitrate of barytes. Very vivid inflammation, and projection of the matter. nitrate of barytes,

Nitrate of potash. Destruction of the metal without inflammation; which is owing, no doubt, to the nitre containing water. and of potash;

Superoxigenized muriates. Very vivid inflammation. oximuriates;

Phosphate of lime. Decomposition without appearance of inflammation: production of phosphuret of lime. phosphate of lime,

Carbonate of lime. Decomposition without inflammation: charcoal set free. and carbonate;

Chromate of lead. Vivid inflammation. chromate of lead,

Chromate of mercury. Became slightly redhot: the mass changed green. and of mercury;

Arseniate of cobalt. Vivid inflammation. arseniate of cobalt;

Green and yellow tungstic acid. Vivid inflammation. oxides of tungsten,

Red oxide of mercury. Very vivid inflammation: slight detonation owing to mercurial vapour. mercury,

Oxide of silver. Very vivid inflammation. silver,

Brown oxide of lead. Like the preceding. lead,

Red oxide of lead. Vivid inflammation.

Yellow oxide of lead. The same.

Yellow

copper, Yellow and brown oxides of copper. Vivid inflammation.

arsenic, White oxide of arsenic. Inflammation.

cobalt, Black oxide of cobalt. Like the preceding.

antimony, Volatile oxide of antimony. Inflammation, but less vivid than with the oxides of copper.

Oxide of antimony at a maximum. Very vivid inflammation.

tin, Oxide of tin at a maximum. Very vivid inflammation.

Putty of tin. Inflammation, but less vivid than the preceding.

iron, Red oxide of iron. Very slight inflammation.

Black oxide. No inflammation, but reduction of the iron.

manganese, Oxides of manganese at a maximum. Very vivid inflammetion.

Oxide at a minimum. No inflammation.

bismuth, Yellow oxide of bismuth. Vivid inflammation.

zinc, White oxide of zinc. Reduction without inflammation.

nickel, Gray oxide of nickel. Pretty vivid inflammation.

and chrome. Green oxide of chrome. No inflammation: production of a blackish matter, which, when completely cooled, and afterward exposed to the air, takes fire as an excellent pyrophorus, and becomes yellow. This matter is a combination of potash and oxide of chrome, which in the air changes to chromate of potash.

Pyrophorus.

Action of potassium on earths.

We likewise tried the action of the metal of potash on earths, and particularly on zircon, silex, yttria, and barytes, and found, that it was evidently altered by all these; but as we do not yet well know the cause of this alteration, we shall not here enter into any particulars respecting it. We shall only say, it appears to us very probable, that the phenomena observed in burning the metal of potash in silicious fluoric gas are in no respect owing to the silex.

Siliceous fluoric gas.

Decomposes all substances containing oxigen,

Be this as it may, it follows from all the preceding facts, that every substance, in which the presence of oxigen is hitherto known, is decomposed by the metal of potash: that almost all these decompositions take place with extrication of light and heat: that more is disengaged in proportion as the oxigen is less condensed: and that consequently they afford

and indicates its condensation.

afford means of estimating the degree of condensation of oxigen in each substance.

These experiments, having occupied a great deal of time, have prevented us from continuing those we had begun on boracic acid. Yet we had already learned, that this acid is capable of being decomposed at a very high temperature by a mixture of charcoal with iron or platina, and forming borurets: for Mr. Descotils, on exposing such mixtures to a forge fire, has obtained metallic buttons, which, treated with nitromuriatic acid, yielded him very evident quantities of boracic acid; and which, from our experiments on the nature of the boracic acid, could be nothing but a combination of bore, platina, and iron.

Boracic acid decomposable by a mixture of charcoal and metal.

III.

The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, &c. By HUMPHRY DAVY, *Esq., Sec. R. S., F. R. S. Ed., and M. R. I. A.*

(Concluded from page 24.)

8. *Analytical Experiments on Muriatic Acid.*

I Have made a greater number of experiments upon this substance, than upon any of the other subjects of research that have been mentioned; it will be impossible to give any more than a general view of them within the limits of the Bakerian lecture.

Numerous experiments made on muriatic acid.

Researches carried on some years ago, and which are detailed in the Journals of the Royal Institution, showed, that there were little hopes of decomposing muriatic acid, in its common form, by Voltaic electricity. When aqueous solution of muriatic acid is acted upon, the water alone is decomposed; and the Voltaic electrization of the gas affords no indications of its decomposition; and merely seems to show, that this elastic fluid contains much more water than has been usually suspected *.

Muriatic acid gas contains much water.

I have already laid before the Society an account of some experiments made on the action of potassium on muriatic

* See p. 31.

acid.

acid. I have since carried on the same processes on a larger scale, but with precisely similar results.

Its action on potassium.

When potassium is introduced into muriatic acid gas, procured from muriate of ammonia and concentrated sulphuric acid, and freed from as much moisture as muriate of lime is capable of attracting from it, it immediately becomes covered with a white crust, it heats spontaneously, and by the assistance of a lamp acquires in some parts the temperature of ignition, but does not inflame. When the potassium and the gas are in proper proportions, they both entirely disappear; a white salt is formed, and a quantity of pure hidrogen gas evolved, which equals about one third of the original volume of the gas.

8 grs. of potassium absorb 22 cub. inches of the gas.

By eight grains of potassium employed in this way, I effected the absorption of nearly twenty-two cubical inches of muriatic acid gas; and the quantity of hidrogen gas produced was equal to more than eight cubical inches.

Hidrogen evolved in the same proportion as if water had been used.

The correspondence between the quantity of hidrogen generated in cases of this kind, and by the action of potassium upon water; combined with the effects of ignited charcoal upon muriatic acid gas, by which a quantity of inflammable gas is produced equal to more than one third of its volume; seemed to show, that the phenomena merely depended upon moisture combined with the muriatic acid gas*.

Farther proof, that nothing but water was decomposed.

To determine this point with more certainty however, and to ascertain whether or no the appearance of the hidrogen was wholly unconnected with the decomposition of the acid, I made two comparative experiments on the quantity of muriate of silver furnished by two equal quantities of muriatic acid, one of which had been converted into muriate of potash by the action of potassium, and the other of which had been absorbed by water; every care was taken to avoid

Spark taken in muriatic gas over mercury.

* When the Voltaic spark is taken continuously, by means of points of charcoal in muriatic acid gas over mercury, muriate of mercury is rapidly formed, a volume of inflammable gas, equal to one third of the original volume of the muriatic acid gas appears, and the acid gas enters into combination with the oxide of mercury, so that water enough is present in the experiment to form oxide sufficient to absorb the whole of the acid.

sources

sources of errour; and it was found, that there was no notable difference in the weight of the results.

There was no proof then, that the muriatic acid had been decompounded in these experiments; and there was every reason to consider it as containing in its common aeriform state at leaſt one third of its weight in water; and this conclusion we ſhall find warranted by facts, which are immediately to follow.

Muriatic acid gas contains one third of its weight of water.

I now made a number of experiments, with the hopes of obtaining the muriatic acid free from water.

Attempts to obtain it free from this.

I first heated to whiteness, in a well luted porcelain retort, a mixture of dry sulphate of iron, and muriate of lime which had been previouſly ignited; but a few cubic inches of gas only were obtained, though the mixture was in the quantity of several ounces; and this gas contained sulphureous acid. I heated dry muriate of lime, mixed both with phosphoric glass and dry boracic acid, in tubes of porcelain, and of iron, and employed the blaſt of an excellent forge; but by neither of these methods was any gas obtained, though when a little moisture was added to the mixtures, muriatic acid was developed in such quantities, as almoſt to produce exploſions.

Muriate of lime distilled with dry sulphate of iron, phosphoric glass, and dry boracic acid.

The fuming muriate of tin, *the liquor of Libavius*, is known to contain dry muriatic acid. I attempted to separate the acid from this ſubſtance, by diſtilling it with sulphur and with phosphorus; but without success. I obtained only triple compounds, in phyſical characters something like the solutions of phosphorus and sulphur in oil, which were nonconductors of electricity, which did not redden dry litmus paper, and which evolved muriatic acid gas with great violence, heat, and ebullition, on the contact of water.

Muriate of tin distilled with sulphur and phosphorus.

I distilled mixtures of corrosive sublimate and sulphur, and of calomel and sulphur. When these were used in their common states, muriatic acid gas was evolved; but when they were dried by a gentle heat, the quantity was exceedingly diminished, and the little gas that was generated gave hidrogen by the action of potassium. During the distillation of corrosive sublimate and sulphur, a very small quantity of a limpid fluid passed over. When examined by transmitted light, it appeared yellowish green. It emitted fumes of muriatic acid, did not redden dry litmus paper,

Muriates of mercury distilled with sulphur,

 and

and deposited sulphur by the action of water. I am inclined to consider it as a modification of the substance discovered by Dr. Thomson, in his experiments on the action of oximuriatic acid on sulphur.

and with phosphorus.

Messrs. Gay-Lussac and Thenard have mentioned*, that they endeavoured to procure dry muriatic acid by distilling a mixture of calomel and phosphorus, and that they obtained a fluid, which they consider as a compound of muriatic acid, phosphorus, and oxigen. In distilling corrosive sublimate with phosphorus, I had a similar result, and I obtained the substance in much larger quantities than by the distillation of phosphorus with calomel.

Phosphorus burned in oximuriatic acid gas.

As oximuriatic acid is slightly soluble in water, there was reason to suppose reciprocally, that water muſt be slightly soluble in this gas; I endeavoured therefore to procure dry muriatic acid, by absorbing the oxigen from oximuriatic acid gas by substances, which, when oxigenated, produce compounds possessing a ſtrong affinity for water. Phosphorus, it is well known, burns in oximuriatic acid gas: though the results of this combuſtion, I believe, have never been minutely examined. With the hopes of procuring muriatic acid gas free from moisture, I made the experiment. I introduced phosphorus into a receiver having a stop-cock, which had been exhauſted, and admitted oximuriatic acid gas. As soon as the retort was full, the phosphorus entered into combustion, throwing forth pale white flames. A white sublimate collected in the top of the retort, and a fluid as limpid as water trickled down the sides of the neck. The gas seemed to be entirely absorbed, for when the stop-cock was opened, a freſh quantity of oximuriatic acid, nearly as much as would have filled the retort, entered.

A white substance sublimed, and a fluid formed.

The same phenomenon of inflammation again took place, with similar results. Oximuriatic acid gas was admitted till the whole of the phosphorus was consumed.

No muriatic acid gas formed.

Minute experiments proved, that no gaseous muriatic acid had been evolved in this operation, and the muriatic acid was consequently to be looked for either in the white subli-

* The Moniteur before quoted.

mate,

mate, or in the fluid which had formed in the neck of the retort.

The sublimate was in large portions, the fluid only in the quantity of a few drops. I collected by different processes sufficient of both for examination.

Properties of the sublimate.

The sublimate emitted fumes of muriatic acid when exposed to air. When brought into contact with water, it evolved muriatic acid gas, and left phosphoric acid, and muriatic acid, dissolved in the water. It was a nonconductor of electricity, and did not burn when heated; but sublimed when its temperature was about that of boiling water, leaving not the slightest residuum. I am inclined to regard it as a combination of phosphoric and muriatic acid in their dry states.

A compound of dry phosphoric and muriatic acids.

Properties of the fluid.

The fluid was of a pale greenish yellow tint, and very limpid; when exposed to air, it rapidly disappeared, emitting dense white fumes, which had a strong smell differing a little from that of muriatic acid.

It reddened litmus paper in its common state, but had no effect upon litmus paper which had been well dried, and which was immediately dipped into it. It was a nonconductor of electricity. It heated when mixed with water, and evolved muriatic acid gas. I consider it as a compound of phosphorous acid, and muriatic acid, both free from water*.

A compound of phosphorous and muriatic acids free from water.

Sulphur heated in oximuriatic acid gas.

Having failed in obtaining uncombined muriatic acid in this way, I performed a similar process with sulphur, but I was unable to cause it to inflame in oximuriatic acid gas. When it was heated in it, it produced an orange coloured liquid, and yellow fumes passed into the neck of the retort, which condensed into a greenish yellow fluid. By repeatedly passing oximuriatic acid through this fluid, and distilling it several times in the gas, I rendered it of a bright olive colour,

Phosphuretted muriatic acid distilled in oxigen and oximuriatic gas.

* I attempted to obtain dry muriatic acid likewise from the phosphuretted muriatic acid of Mess. Gay-Lussac and Thenard, by distilling it in retorts containing oxigen gas, and oximuriatic acid gas. In the first case, the retort was shattered by the combustion of the phosphorus, with a violent explosion. In the second, compounds, similar to those described above, were formed.

colour, and in this case it seemed to be a compound of dry sulphuric and muriatic acid, holding in solution a very little sulphur. When it was heated in contact with sulphur, it rapidly dissolved it, and then became of a bright red colour, and when saturated with sulphur, of a pale golden colour*. No permanent aeriform fluid was evolved in any of these operations, and no muriatic gas appeared, unless moisture was introduced.

As there seemed little chance of procuring uncombined muriatic acid, it was desirable to ascertain what would be the effects of potassium upon it in these singular compounds.

Potassium introduced into the fluid from muriate of mercury.

When potassium was introduced into the fluid generated by the action of phosphorus on corrosive sublimate, at first it slightly effervesced, from the action of the liquid on the moist crust of potash surrounding it; but the metal soon appeared perfectly splendid, and swimming on the surface. I attempted to fuse it by heating the fluid, but it entered into ebullition at a temperature below that of the fusion of the potassium; indeed the mere heat of the hand was sufficient for the effect. On examining the potassium, I found that it was combined at the surface with phosphorus, and gave phosphuretted hidrogen by its operation upon water.

The fluid deprived of a considerable quantity of phosphorus,

I endeavoured, by repeatedly distilling the fluid from potassium in a close vessel, to free it from phosphorus, and in this way I succeeded in depriving it of a considerable quantity of this substance.

and heated with potassium.

I introduced ten or twelve drops of the liquid, which had been thus treated, into a small plate glass retort, containing six grains of potassium. The retort was exhausted after having been twice filled with hidrogen, the liquid was made to boil, and the retort kept warm till the whole had disappeared as elastic vapour. The potassium was then heated by the point of a spirit lamp; it had scarcely melted, when it burst into a most brilliant flame, as splendid as that of phosphorus in oxigen gas, and the retort was destroyed by the rapidity of combustion.

* All these substances seem to be of the same nature as the singular compound, the sulphuretted muriatic acid discovered by Dr. Thomson, noticed in page 98.

In

Results.

In other trials made upon smaller quantities after various failures, I was at last able to obtain the results; there was no proof of the evolution of any permanent elastic fluid during the operation. A solid mass remained of a greenish colour at the surface, but dark gray in the interior. It was extremely inflammable, and often burnt spontaneoufly when exposed to air; when thrown upon water, it produced a violent explosion, with a smell like that of phosphuretted hidrogen. In the residuum of its combustion there was found muriate of potash, and phosphate of potash.

An iron tube destroyed in the experiment.

I endeavoured to perform this experiment in an iron tube, hoping, that, if the muriatic acid was decomposed in the process, its inflammable element, potassium, and phosphorus, might be separated from each other by a high degree of heat; but in the first part of the operation the action was so intense, as to produce a destruction of the apparatus, and the stop-cock was separated from the tube with a loud detonation.

Potassium heated in the vapour of the compound with phosphoric acid.

I heated potassium in the vapour of the compound of muriatic and phosphoric acid; but in this case the inflammation was still more intense, and in all the experiments, that I have hitherto tried, the glass vessels have been either fused or broken; the solid residuum has however appeared to be of the same kind as that I have just described.

Action of potassium on the sulphuretted compounds.

The results of the operation of the sulphuretted compounds, containing muriatic acid free from water, upon potassium are still more extraordinary than those of the phosphuretted compounds.

When a piece of potassium is introduced into the substance that distils over during the action of heated sulphur upon oximuriatic acid, it at first produces a slight effervescence, and if the volume of the potassium considerably exceeds that of the liquid, it soon explodes with a violent report, and a most intense light.

Violence of the explosion.

I have endeavoured to collect the results of this operation, by causing the explosion to take place in large exhausted plate glass retorts; but, except in a case in which I used only about a quarter of a grain, I never succeeded. Generally the retort, though connected with the air pump at the time, was broken into atoms; and the explosion produced

by

by a grain of potassium, and an equal quantity of the fluid, has appeared to me considerably louder than that of a musket.

Solid compound formed.

In the case in which I succeeded in exploding a quarter of a grain, it was not possible for me to ascertain if any gaseous matter was evolved; but a solid compound was formed of a very deep gray tint, which burnt, throwing off bright scintillations, when gently heated, which inflamed when touched with water, and gave the most brilliant sparks, like those thrown off by iron in oxigen gas.

Its properties certainly differed from those of any compound of sulphur and potassium that I have seen: whether it contains the muriatic basis must however be still a matter of inquiry.

The highly inflammable nature of the compounds probably depends on the muriatic acid.

There is, however, much reason for supposing, that, in the singular phenomena of inflammation and detonation that have been described, the muriatic acid cannot be entirely passive: and it does not seem unfair to infer, that the transfer of its oxigen, and the production of a novel substance, are connected with such effects; and that the highly inflammable nature of the new compounds partly depends upon this circumstance. I am still pursuing the inquiry, and I shall not fail immediately to communicate to the Society such results as may appear to me worthy of their attention.

9. *Some general Observations, with Experiments.*

An experiment has been lately published, which appeared so immediately connected with the discussion entered into in the second section of this paper, that I repeated it with much earnestness.

Experiment of Dr. Woodhouse.

In Mr. Nicholson's Journal for December, Dr. Woodhouse has given an account of a process, in which the action of water caused the inflammation of a mixture of four parts of charcoal and one of pea lash, that had been strongly ignited together, and the emission of ammonia from them. I thought it possible, that in this case a substance might be formed similar to the residuum described in page 50*; but

* See Journal, vol. XXIII, p. 250.

by

by cooling the mixture out of the contact of nitrogen, I found that no ammonia was formed; and this substance evidently owed its existence to the absorption of atmospherical air by the charcoal *.

New views of the nature of acidity.

The experiments that I have detailed on the acids offer some new views with respect to the nature of acidity. That a compound of muriatic acid with oxide of tin or phosphorus should not redden vegetable blues, might be ascribed to a species of neutralization by the oxide or inflammable body; but the same reasoning will not apply to the dry compounds, which contain acid matter only, and which are precisely similar as to this quality. Let a piece of dry and warm litmus paper be moistened with the compound of muriatic and phosphorous acid, it perfectly retains its colour. Let it then be placed upon a piece of moistened litmus paper, it instantly becomes of a bright red, heats, and developes muriatic acid gas.

Fluid acids containing water are conductors of electricity.

All the fluid acids that contain water are excellent conductors of electricity, in the class called that of imperfect conductors; but the compounds to which I have just alluded are nonconductors in the same degree as oils, with which they are perfectly miscible. When I first examined muriatic acid, in its combinations free from moisture, I had great hopes of decomposing them by electricity; but there was no action without contact of the wires, and the spark seemed to separate no one of their constituents, but only to render them gaseous. The circumstance likewise applies

Potash decomposed by the combined affinities of two combustibles.

* Potash or pearlash is easily decomposed by the combined attractions of charcoal and iron; but it is not decomposable by charcoal, or, when perfectly dry, by iron alone. Two combustible bodies seem to be required by their combined affinities for the effect; thus in the experiment with the gun barrel, iron and hidrogen are concerned. I consider Homberg's pyrophorus as a triple compound of potassium, sulphur, and charcoal; and in this ancient process, the potash is probably decomposed by two affinities. The substance is perfectly imitated by heating together ten parts of charcoal, two of potassium, and one of sulphur.

When I first showed the production of potassium to Dr. Wollaston in October 1807, he stated, that this new fact induced him to conceive, that the action of potash upon platina was owing to the formation of potassium, and proposed it as a matter of research, whether the alkali might not be decomposed by the joint action of platina and charcoal.

to

to the boracic acid, which is a good conductor as long as it contains water; but which, when freed from water and made flu d by heat, is then a noncond ctor.

Alkalis &c. nonconductors when solid, but conductors when fused.

The alkalis, and the earthy compounds, and the oxides, as dry as we can obtain them, though nonconductors when solid, are on the contrary, all conductors when rendered fluid by heat.

Water in muriatic acid gas.

When muriatic acid, existing in combination with phosphorous or phosphoric acid, is rendered gaseous by the action of water, the quantity of this fluid that disappears at least equals from one third to two fifths of the weight of the acid gas produced; a circumstance that agrees with the indications given by the action of potassium*.

Muriate of mercury in vapour passed through ignited charcoal.

I attempted to procure a compound of dry muriatic and carbonic acids, hoping that it might be gaseous, and that the two acids might be decomposable at the same time by potassium. The process that I employed was by passing corrosive sublimate in vapour through charcoal ignited to whiteness; but I obtained a very small quantity of gas, which seemed to be a mixture of common muriatic acid gas and carbonic acid gas; a very minute portion of running mercury only was obtained, by a long continuation of the process; and the slight decomposition, that did take place, I am inclined to attribute to the production of water by the action of the hidrogen of the charcoal upon the oxigen of the oxide of mercury†.

Muriatic acid gas attracts water from some other gasses,

In mixing muriatic acid gas with carbonic acid, or oxigen, or hidrogen, the gasses being in their common states as to moisture, there was always a cloudiness produced; doubtless owing to the attraction of their water to form liquid muriatic acid.

* Page 101.

† These facts, and the other facts of the same kind, explain the difficulty of the decomposition of the metallic muriates in common processes of metallurgy. They likewise explain other phenomena in the agencies of muriatic salts. In all cases when a muriatic salt is decomposed by an acid, and muriatic acid gas set free, there appears to be a double affinity, that of the acid for the basis, and of the muriatic acid for water; pure muriatic acid does not seem capable of being displaced by any other acid.

On

On fluoric acid gas no such effect was occasioned. This fact, at first view, might be supposed to show that the hidrogen evolved by the action of potassium upon fluoric acid gas is owing to water in actual combination with it, like that in muriatic acid gas, and which may be essential to its elastic state; but it is more probable, from the smallness of the quantity, and from the difference of the quantity in different cases, that the moisture is merely in that state of diffusion or solution in which it exists in gasses in general; though from the disposition of water to be deposited in this acid gas in the form of an acid solution, it must be either less in quantity, or in a less free state, so as to require for its exhibition much more delicate hygrometrical tests.

but not from fluoric acid gas.

The facts advanced in this Lecture afford no new arguments in favour of an idea, to which I referred in my last communication to the Society, that of hidrogen being a common principle in all inflammable bodies; and except in instances which are still under investigation, and concerning which no precise conclusions can as yet be drawn, the generalization of Lavoisier happily applies to the explanation of all the new phenomena.

No farther proof of hidrogen being a common principle of inflammables.

In proportion as progress is made towards the knowledge of pure combustible bases, so in proportion is the number of metallic substances increased; and it is probable, that sulphur and phosphorus, could they be perfectly deprived of oxigen, would belong to this class of bodies. Possibly their pure elementary matter may be procured by distillation, at a high heat, from metallic alloys, in which they have been acted upon by sodium or potassium. I hope soon to be able to try this experiment.

Sulphur and phosphorus probably contain metallic bases.

As our inquiries at present stand, the great general division of natural bodies is into matter which is, or may be supposed to be, metallic, and oxigen; but till the problem concerning the nature of nitrogen is fully solved, all sytematic arrangements made upon this idea must be regarded as premature.

Perhaps all bodies, except oxigen metallic.

IV.

IV.

Extract of a Letter from Mr. J. B. VAN MONS, *Member of the Institutes of France and Holland, to the Editor, on Atmospheric Phenomena.*

SIR,

Formation of thunder storms.

IN a paper which I laid before the Batavian Society of Experimental Philosophy at Rotterdam, I showed, that thunder storms form in the atmosphere spontaneously, and wholly. The diminution of sidereal, and particularly of lunar attraction, suffers the air to sink down, by depriving it of the additional elasticity this attraction imparted to it; this sinking loosens the union between the air and water; the temperature is raised by the separation of the caloric, that served as the medium of this union; and the water separates in some part or other of the atmosphere, forming a cloud. This cloud soon enlarges by the continuation of the same cause, the caloric separates from it in great abundance, and, as the air is a very bad conductor of heat, this can neither diffuse itself, nor be dissipated in the form of light, a modification of caloric into which it is not sufficiently concentrated to transform itself, adopts the state of electric fluid, and decomposes the water of the cloud.

Clouds.

Water converted into a permanent gas by caloric in a state approaching to that of electric fluid,

It is probable, that this effect happens only to a very slight quantity of caloric; and that the portion of this principle, which in combination with air serves to convert water into a permanent gas, is contained in this union in the state of electric fluid, or at least in a state intermediate either to that of heat and electricity, or of electricity and light; which fourth state being incapable of subsisting except in combination, will never be known to us separately, or otherwise than by its effects. This state is the agent, by means of which permanent gasses retain their state. With the bases of these gasses it enters into a chemical union, which can be broken only by an affinity of the same nature.

which is the cause of permanent gasses.

Different states of caloric.

Caloric alone cannot convert these bases into gas, before it

it is sufficiently concentrated to assume the requisite elasticity, and then it is in the state of light. Light, though little concentrated, produces this effect, because it has only to lose a little of its elasticity to become electricity, or subelectricity, the fourth modification of caloric; which excess of elasticity it transmits however to the caloric, with which the bases abovementioned are fixed, and which has lost much of its natural elasticity in that fixation. Thus more or less elasticity constitutes all the difference between light, the electric fluid, sublight, and subelectricity, if indeed this exist, and heat. We cannot take a single step in natural philosophy or chemistry, without perceiving the facility with which these agents are metamorphosed one into another; a metamorphosis on which depends a very great number of phenomena.

Their ready conversion into each other the cause of many phenomena.

Heat partially and copiously separated only becomes electricity.

It is the heat alone that separates in great abundance, and in a distinct part of the atmosphere, which can thus transform itself into electric fluid. That which is produced by the general loosening [*relachement*] of the air, or a certain decomposition of this fluid in its aqueous combination, and which heats the atmosphere, has no occasion to diffuse itself, being generally separated, and it remains heat. Every increase or diminution of the temperature of the air is spontaneous, and not communicated, or conducted by the winds, which are themselves the effects, and not the causes, of changes of temperature, and other alterations that take place in the atmosphere.

Heat of the air not communicated but spontaneous.

With the rise of the thermometer the barometer falls, and vice versa.

At every increase of the temperature of the air, the barometer sinks, because the precipitation of water diminishes the elasticity of this fluid: as every diminution of temperature, which always results from the combination of water with air, with fixation of caloric, and the transformation of heat into electricity, causes the barometer to rise by the increase of elasticity which the air acquires. This last effect frequently takes place during rain, when this rain is the excess of water which the air deposits, to be enabled to resume, constantly under the influence of some sidereal cause, that state of serenity, which constitutes fair weather. This rain, or that which falls with a rising of the barometer and a falling of the thermometer, is a rain of recomposition

of the air in its aqueous combination; and that which falls with a sinking of the barometer and a rise of the thermometer is rain of decomposition of the air with respect to that combination.

The barometer rises from the increased elasticity of the air, not from its weight.

I cannot easily conceive, how people continue to ascribe to the weight of the air that pressure, which this fluid exerts on the mercury in the barometer; while we see it almost always increases this pressure, when it loses part of its matter, or gravitating power; and diminish it when the air is at a *maximum* of aqueous saturation, or just before rain; and when the bulb manometer, or true aerostatic balance, indicates the ultimate degree of density in the air; and that all other phenomena, both those that occur in nature, and those that present themselves in experiments with the mercurial pump, prove to a demonstration, that the air presses chiefly by virtue of its elastic power, which is increased by condensation, and by the addition of caloric, the matter remaining the same in a closed space; and diminished by rarefaction, and the subtraction of caloric, the matter remaining equally the same, and in the same space; but which is neither increased nor diminished in the open air, but by the association, more or less elastic, more or less solid, of water with air.

Experiments in confined air not applicable to meteorology.

This proves how little applicable to atmospheric phenomena are the results we obtain under our glasses, in which the air is deprived of its free motion, and where this fluid is withdrawn from the effects of rarefaction and condensation produced by sidereal influences; which effects, added to the more or less permanent or solid gassification of water, and the transformation of light and of heat into electric fluid, give rise to all meteoric phenomena, and occasion by their frequent variations the great variableness of the state of the atmosphere.

Formation of clouds.

The first portion of water decomposed into gas, while it changes the composition of the air, and increases the density of this fluid at the point where this decomposition takes place, determines the formation of other clouds, which depose also electric fluid, and are in part decomposed, and so on.

Charged with electricity.

The electric fluid, that does not combine to gassify the principles of water, charges these clouds by strata of opposite

opposite zones, in the same manner as it charges semiconductors, and as their natural fluid is distributed in the state of charge in nonconductors; and the gasses of water, notwithstanding the lightness of one of them, dissolve in the air as spirit of wine dissolves in water, and remain diffused in the matter of the cloud. Soon, by the condensation of the fluid, or the intensity of the charge, this state destroys itself; the fluid bursts from stratum to stratum, and the water is recomposed by the inflammation of its gasses. The fulguration or flashes of lightning without or almost without noise, and the light of which perfectly resembles that of the electric spark, are the effects of the explosion of the fluid of that spark; and the flashes accompanied with thunder, or true lightning, which diffuse the same light as the combustion of hidrogen and oxigen gas, are those of the combustion of the gasses of water. These two sorts of lightning alternate with each other, because the decomposition and recomposition of water take place alternately. The rolling of thunder arises from a succession of partial inflammations, in proportion as the strata oppositely electrified confound their two states. The sounds too are different; that of the fulgurations being acute, quick, snapping; and that of the fulminations heavy, dull, rolling; and, from their analogy to the sounds produced by electric sparks and the combustion of hidrogen and oxigen gas in our experiments, may easily be referred to the phenomena, to which they belong.

The principles of water diffused in them.

Discharge of the clouds.

Lightning of two kinds.

Thunder of two kinds.

The sound of the combustion is more intense, because a vacuum is formed, which is instantly filled, and more than filled, by the vapour of water, that acquires a state of considerable expansion. When once the rain has begun to fall, and the work of the storm is set a going, it proceeds of itself, or has no longer occasion for the formation of fresh clouds to keep it up; the caloric that separates from the combined gasses transforming itself into electricity, which in its turn decomposes a portion of water; so that the work of the successive decompositions and compositions continues by the effect of its alterations, and is kept up of itself, till all the water diffused through the surrounding air by vaporization is condensed there, and resolved into rain; or till, by

Cause of the intensity of the sound.

Progress of the storm.

by the separation of the fluid, and its conveyance to the Earth, in consequence of its great condensation, as well as of the water of the cloud being again taken up in solution by the air, the storm ceases before this has happened. The water of clouds being again taken into solution by the air occasions a cooling of the air, and presages a definitive cessation of its stormy state; while the heating of the air, or continuation of its high temperature, denotes the continuation of the decomposition, and is always followed by a recommencement of the storm.

The air growing cool indicates its cessation.

Hail.

Hail arises from a strong fixation of caloric, which transforms itself into electricity, to gassify the principles of water; and sometimes from a too copious combination of the same caloric converted into electric fluid to reunite the water with the air; or from the same conversion of caloric to reinforce the thunder, which endeavours to explode toward the Earth. This explosion of the thunder takes place either after a considerable recomposition of water, or when, the greater part of the water of the storm being dispersed, the electric fluid remaining no longer finds any thing to which it can adhere, concentrates itself in a point, and acquires elasticity enough to overcome the opposition of the air, and rush toward the Earth, or some prominent points on the globe. As this passage of the thunder toward the Earth is not solicited by a state of subtraction, opposite, or negative charge, the course it follows is neither direct, or the shortest possible, nor determined to a given point; but its course is uncertain, irregular, and in some measure vague, bursting from one substance to another, even striking the ground and separating from it anew, without any other cause than the difficulty of diffusing or decomposing itself.

Lightning darting toward the ground.

Cause of the great mischief done by lightning.

To this difficulty of resuming an equilibrium, which it finds no where broken, or of diffusing itself in a point of subtraction which no where exists for it, are owing the extraordinary effects of the explosion of thunder, and the incalculable means of destruction, with which we see it act; and that even when it has already arrived at the ground, where it ought to be able to diffuse itself, it still vaporizes water with great force, splits stones, &c. To the same cause is owing, that it proceeds so slowly, that it so long retains

retains its state of sparkforming concentration, and that it so easily fuses and inflames substances, staying long at each point of its course, and transforming itself easily into light and heat. One portion of the electric fluid separated during a thunderstorm transforms itself into light, and is dissipated in space, at every explosion of a spark, or of a fulmination of combustion. The sound of the thunder that bursts toward the Earth is very different too from that of rolling thunder, and perfectly resembles that of the discharge of our electrical batteries. The common people readily distinguish it, and denote it by the name of falling thunder. The opposite winds that blow during a thunderstorm, and are even contrary to its direction, are the natural effect of a strong condensation of the aqueous part of the atmosphere.

Two sounds of thunder.

Opposite winds during a thunder storm.

A thunderstorm then does not arise from an accumulation of hidrogen gas extricated from the Earth, from which none is extricated, and rising to the superior regions of the atmosphere, whither it does not ascend; this gas never being extricated in its pure state; and that which is extricated in combination with a combustible substance, whether phosphorus, sulphur, or carbon, being burned by a concurrence of action on the part of these combustibles as soon as it comes into contact with the air, and no experiment having ever demonstrated the exi tence of the least bubble of hidrogen gas in the air at any elevation whatever. Besides, the hidrogen gas we set free in the air does not ascend in it in consequence of its greater lightness, or less specific gravity, but becomes incorporated with the air with which it is in contact, remains adherent to it by an affinity of penetration; and even does not diffuse itself in it without difficulty, and in some time, when the air is perfectly at rest. Nay more, I have strong reasons for believing, that, at the time of great assimilations of water, the affinity of the air for this fluid determines the direct combustion of hidrogen gas by the air, without the intervention of any other inflammable substance.

Hidrogen gas does not ascend from the earth into the upper regions of the air.

Hidrogen gas alone burned in the air.

The rain too is not the consequence of the condensation of aqueous vapour by cold, since the fall of rain always precedes the cooling of the air, while an increase of the temperature

Rain not aqueous vapour condensed by cold.

temperature of the air always precedes rain: water then is dissolved by the air, or rather associated with the composition of the air by the intervention of caloric in the state of electricity, and this in so large a quantity, that it forms almost a fourth of the weight of the atmosphere. I give in the paper, to which I have alluded above, the facts and experiments on which this calculation is founded; but these facts are not very numerous, and almost all synthetical, that is of addition, or composition, and but few analytical, or of subtraction or decomposition; the air being of all known bodies that which has the greatest affinity for water to a certain point of saturation, of which there are very many degrees, and very distinct, from the nature of the affinity that limits them; so that, without decomposing it, we can scarcely separate the water from it, partly, no doubt, on account of the form of the air, which it faithfully preserves, and which prevents us from retaining it to separate it. And the difficulty of synthetical experiments depends on this, that, in removing the water by decomposing it, we cannot prevent the air itself from being decomposed with respect to its oxigen, all the processes we must employ for this purpose being of the disoxigenizing kind, not excepting the electric fluid, which determines the condensation of oxigen by azote. Nothing then is more difficult, than to obtain, for the purpose of synthetical experiments, air deprived of its water to a certain point; and the method, that has succeeded best for this purpose, is the disengagement of muriatic gas from a very dry muriate, by means of highly concentrated sulphuric acid, in confined air.

A fourth of the weight of the atmosphere owing to water.

Difficulty of the synthetical experiments.

Best method of depriving air of water.

Causes of mistakes in determining the proportion of oxigen in substances.

I need not observe to you, how many mistakes in determining the proportions of oxigen in burned substances must have arisen from the great quantity of water, that makes part of the air, which becomes solidly fixed in these substances, and serves as an indispensable medium of the combination of oxigen with the bodies it burns. To this large quantity of water in the air are owing those spontaneous and heavy rains, which frequently fall in an atmosphere, that was perfectly serene and tranquil a moment before.

Caloric proper to the solar

The caloric, that under its different forms is incessantly ascending in the air, without ever returning to the Earth, being

being a substance that belongs to the atmosphere of the Sun, and is foreign to ours and those of other planets; at which it arrives only by virtue of the great elasticity it possesses when in the state of light, and where it is retained only by its adhesion to substances that belong to these planets; must resume the state of light, the moment when, having arrived at the utmost limits of these foreign atmospheres, and being disengaged from the substances that can no longer follow it, it returns to that which is proper to it, and there takes a centripetal motion, or movement of approximation to the Sun; which, being a perfectly transparent and elastic substance, occasions it to take an opposite course with the same velocity, with which it rushed upon it, which must occasion a perpetual circulation of light between the Sun and those globes, that make part of its system.

atmosphere alone.

Perpetual circulation of light.

If this were not the true state of things, there would be an incessant accumulation of caloric, that would soon change the face of these globes; while in this hypothesis the equilibrium is scarcely ever interrupted. These globes then would not be visible but from the extreme limits of their atmospheres, and where the caloric, separated from its combinations, is transformed into light: and the opacity of a globe would not at all prevent this effect, in which the globe itself would not interfere; which would make a wonderful difference in the calculations, from which we have determined the apparent magnitudes of the celestial bodies; as in this case their magnitudes would have been calculated from the extent of their atmospheres, and by no means from that of the globes or celestial bodies themselves; and the light, which renders these bodies visible to us, would not be reflected light, but light extricated from them, or returning toward the Sun. It is to be understood, that this extrication cannot take place, except as far as the atmosphere faces the Sun, and is under the direct influence of its attractive power; otherwise the light extricated would diffuse itself through space, take a course different from that to the Sun, and not reach the atmosphere of that celestial body, where alone it can resume its character of light. Nothing prevents the light in this return from traversing

Otherwise an accumulation of caloric would change the face of things;

and our astronomical calculations would be erroneous.

The presence of the Sun necessary to attract this light.

other atmospheres. It is by the light refracted in this passage, that we see the globes from which it emanates.

I am, Sir, with great esteem,

Yours, &c.

J. B. VAN MONS.

V.

Remaining Proof of the Cause of Motion in Plants explained; and what is called the Sleep of Plants shown to be Relaxation only. By Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Cause of motion in plants.

ANXIOUS to complete the proofs of that idea suggested in my last paper, concerning the motion of plants; and to show, that I should not have endeavoured to call the attention of the public to this subject, had I not possessed what appeared to me to be the most incontrovertible arguments in its favour, with the most solid reasons for believing, not only "that this leatherlike substance and the spiral wire are the cause of motion in plants," and of every degree of irritability (which I was at first fearful of advancing), but that "they are also the cause of what has been mistaken for the sleep of plants." In short, this appears to me perfectly to explain all that has hitherto been considered as *feeling* or *volition* in plants, and to resolve it into mechanical power; and the complete management of the spiral wire. The interior formation of plants, when duly magnified by the solar microscope, proves the vegetable world to be composed of machines governed wholly by light and moisture; and dependant on these causes for motion.

No feeling or volition in plants.

Spiral wire.

The spiral wire may be considered as a *secondary cause*, acted upon by the *two first*; and by its means all the movements of the plant are made, the flower opens and shuts in the morning and evening, the leaves turn, or the creeping plants

plants wind in their regular order. Nor can the flower opening at a *different time* of *the day*, or *turning* in a *different manner*, militate against the argument; as the constant effect of strong light and dry weather is to *contract* the wire; that of darkness and moisture, to *dilute* it; and it depends wholly on *which way* the spiral wire is placed, whether its dilating shall *open* or *shut* the flowers: as in mechanics, the same spring may be made to turn to the right or to the left, to open or shut a box. Most of the flowers I have observed, that close at noon, are extremely limber in the corolla, which is formed only of a double cuticle, *without pabulum*, and soon overcome by heat; and when this is the case, relaxation directly takes place. Such is the convolvulus *nil*, the hesperantha cinamomea, the tiger plant, the evening primrose, &c. The great Author of nature shows in all his plans a simplicity, that must hourly strike the dissecter of plants, and prove how much more ability is necessary to produce such *simple mechanism*, than to invent our more *cumbrous machines*.

Spiral wire regulates the opening of the flower.

Mirbel, one of the latest and best of the French botanical writers, though his work is one of the first compendiums of the science, has greatly mistaken the subject, merely from not having *sufficiently magnified* his specimens. He says, "if the spiral wires were common to most plants (which he does not believe) they could not in any way promote the motion of plants, because they are confined in a case which cannot stretch." I shall give an exact drawing of the *case*, which will I think plainly prove it was made for no *other purpose*: but to see this it is necessary to place it in the solar microscope, and Mirbel did not use one. I contrived to measure its increase by taking it out of a leaf stalk, and placing it between a double pair of pincers, which were laid in a groove, moving them by means of a thread over a very little wheel. They were drawn with a delicacy no hand could imitate, and it stretched without breaking (by moistening it) from 1 inch and a quarter, to 2 inches wanting 2 tenths, but it was after being apparently much contracted by light in the microscope. As to the spiral wire it is apparent, that it may be drawn to any length. A larger magnifier would also have convinced Mirbel, that the case

Case of the spiral wire

made to stretch.

is of so thin a substance, or rather I believe I should say *of so loose a one*, as plainly to be intended to dilate and contract; a few very thin vessels, interlaced with an extremely fine spiral wire, composes it, as will be seen in the plate: while the larger spiral vessels fill up the case in an irregular manner, the nourishing vessels forming a regular circle of tubes round it, and, to complete the contrivance, the midrib of the leaf is formed also to contract and dilate a little with perfect ease, even in the hardest leaves, as the *laurel*, &c. But in some leaves there is a curious contrivance to lengthen it in the bosom of the leaves just where the bud is *concealed* in its *first birth*, as in the *ash*, *plane*, &c. The peach, and most of that order, will stretch the midrib of the leaf far more than is necessary for any succeeding motion: and if any person could doubt the power of the spiral wire to draw up, and of course turn the leaf, he has but to take a leaf so drawn from a nectarine, or peach; for it seems impossible that it should not be seen, that it is drawn from the inside of the midrib, and not from the gathering up of the cuticle of the leaf, which has been suggested. I have seen in the geranium the spiral wire to have stretched the case with such violence, it could not return to its usual size, but has remained in a spiral form: which shows however how easily the spiral wire acts on the case.

Spiral wire contracting or dilating.

Mid rib of the peach leaf stretches.

It may be thought, that it was not necessary the *stalk* should dilate itself, provided the part within did; but nature finds its account in this arrangement, the leaf stalk could not turn with ease, did not one side of it contract, to wind it round. They have all in this respect their *appropriate proportions*, the spiral wire stretches to any degree wanted, the case a great deal less than the former, and the outward cuticle has only flexibility enough not to impede the continual irritability of its inward spiral wire.

Different parts stretch in different degrees.

In my first letter on this subject, I showed the effect of the spiral wire on plants in general, selecting those that were only *commonly affected* by it, that I might not be accused of favouring my subject, in order to conceal any weakness the argument might possess. It is truth alone I seek, and I can have no other attachment to it but supposing it so; my eyes and my microscope must grossly de-

Extreme effect of the spiral wire.

ceive

ceive me, before I deceive others. I have now to present three of the most sensitive plants I am acquainted with, in order to show the *full strength* of the spiral wire, and of that sort of leathery substance of which it is composed. The first is the Indian grass that conducts the hygrometer made by Captain Kater; the second is the thorn of the nettle, which is certainly made of the same leatherlike substance; and the third is the mimosa sensitiva, the bag of which plant is also of the same nature, though infinitely thinner. They are all governed by this substance turned into a spiral wire, and the same substance in another form. When thicker it is certainly infinitely stronger, as it proves by the awn of the grass, which is so powerful, though much of it has lost its spiral wire. There is a water plant that sends up its flower by the same substance, and which I have not been able to procure; but I shall be satisfied with showing the dissection of these plants, perfectly convinced they will be thought sufficient to prove, that this *substance* is the *cause of motion in plants*.

Formation of the awn of the Indian grass.

The first is an Indian grass, but the only part sensitive is the awn, which is formed of this leatherlike substance, infinitely thicker and stronger than the usual spiral wire, and its untwisting would be certainly capable of regulating a much more powerful instrument. The awn is formed of two apparently flat pieces, with a cylindric hollow running through the middle, which is filled with a thick spiral wire, but which I have found in only two pieces: the rest (I suppose from long keeping) must have decayed. Each side is bristled as the awns of grass generally are, but I never could perceive, that these bristles added any sensitive power to the awn, though from their resembling those in the sensitive plant I expected to find that they did. I therefore deprived both plants of this ornament; but I could not perceive any difference in their sensibility, and in neither specimen are they twisted. It is quite wonderful to see the strength with which this wire twists and untwists. It is only the very finest part, that can be placed in the solar microscope, without breaking the glasses between which it is laid, though not three tenths of an inch in length. What is extraordinary is, *that so made*, it will continue to *untwist* into two

The untwist-

different

ing the awn of the grass.

different threads, and I doubt not at last separate into as fine a wire as runs in the mimosa. The hygrometer made of it is the best that was ever invented, and indeed it is difficult to conceive one more sensible, since at nearly an inch distance the moisture of a finger will cause it to make one or two revolutions of 100 parts each. The figure of the instrument was given in your Journal for last July.

Formation of the sting of the nettle.

The next plant is the nettle, irritable only at the *awn* or *sting* of *the plant*. It is a long pipe with a bag at the end, divided into two parts; the smaller, in which is enclosed the bag of the poison; and the larger, which is below it. The whole bag appears to be formed of the same leather-like substance as the awn of the grass, and to be, in proportion to its size, equally affected by light and moisture. The moment the upper part of the pipe is touched, the under part of *the bag whirls up*, breaks the poison bladder, and throws its contents violently up the pipe, burning the person who touches it. This is instantaneous; and so susceptible is it, that the light thrown on it in the solar microscope has exactly the same effect as the touch. The liquor is protruded with a force quite wonderful up the pipe, till it issues at the minute aperture of the point; but before it does so, the pipe is bent down with a jerk by the spiral wire, exactly as the leaf stalk of the mimosa sensitiva bends to the touch. They are managed exactly by the same force, and governed by the same powers, *light* and *moisture*. This description of the nettle will account for its not stinging when pressed hard; the pipes being then broke, and the liquor of the nettle, mixing with the poison, dilutes it so completely, that it has no longer any effect. It will be seen, that the spiral wire is carried round the bag, and that it is drawn together by the contraction of the wire: and to complete the likeness of the three plants I must mention, that the nettle lays down its large stings every evening just as the sensitive plant does its branches, and that the awn of the grass also untwists towards evening.

Effect of light on the sting of the nettle.

Lays down its stings at night.

Sensitive plant.

I shall now turn to the mimosa sensitiva, which has less of real strength, but more mechanism than the other two. Its motions proceed from the same cause, which is not only the spiral wire, but a bag of the same leathery kind, that contracts

contracts and dilates: but the plant seems much more to depend on the spiral wire. It is impossible not to be struck with astonishment and admiration at the beauty and delicacy of the contrivance, which is far more artificial than the works of nature generally are; and I know not a plant, that has taken me so much time, and given me so much trouble, to develope the different joints, pulleys, knots, and bolts. I have long perceived, that a plant is sensitive in proportion to the *tight manner* in which it is twisted, and the ſhort distance between the *knots:* now there is scarcely in this plant $\frac{1}{10}$ of an inch between the knots; and the spiral is twisted as tight as *possible.* I could not persuade myself the sketch was exact, till I had from different specimens drawn it twelve or fourteen times; but I have exposed the greatest part to the view of so many friends, that I think I may truly answer for its being exactly sketched after nature.

Mechanism of the leaf.

At D. fig. 1, Pl. IV, are the springs that govern each leaf, *d d* is the stalk. Each leaf has a base *c c*, which serves to concentrate the spiral wires. These passing over in every direction, being drawn *through the narrow part of the stem* by the strings *b b b b*, press the stem together; and, when touched, lay the leaves one on the other the whole way down the leaf stalk. But before the ſtimulus is applied; the stem is flattened in a contrary direction. The ball of the leaf is hollow, and filled with oil. The parts *e e* and *p p*, Pl. III, fig. 8, are made of that leathery substance, which forms the cuticle, and is contracted by the light in the solar microscrope, just as the bag of the nettle is acted upon, or the twisting part of the grass. The parts *e e* contain the oil, which serves to lubricate the knots (I suppose) and enable them to slip over each other; beside probably acting some important part in the formation of the various gasses and juices in the composition of the plant. When touched the whole string relaxes at *o o*, and lets the branch fall. This it would also do at *m*, if it was not supported by the wood vessels turning into the leaf. Fig. 2, Pl. IV, is the part *e e p p*, uncut, and in its natural state. The sort of bolts are retained in their places by the wood vessels which cross them in every direction; as in trailling plants they do, to defend the *bud from accident.* I should have placed them

them in the sketch, had I not been fearful, by mixing them with the spiral wire, to make such confusion, they would not be known one from the other. But it is easy to understand, that by crossing vessels they are retained in their present situation.

Seminal leaves have no spiral wire.

I must now mention a circumstance, which helps greatly in my mind to prove, that the spiral is the cause of motion. It is that in the *seminal leaves* there is no spiral wire; and in the seminal leaves there is no *motion* whatever. In describing the spiral wire I did not mention the case, in which it is confined, because I wished at the same time to give a sketch of it to avoid confusion. It will now be found in Plate IV, fig. 4.

Drosera acaûlis very curious.

I might add to the three plants I have given many others, in which the spiral wire distinguishes itself. The drosera acaulis is entirely governed by the spiral wire, which enters the hairs, or rather *arms* of the leaf, and the moment a fly touches it, it collapses, confining it within its circle; or should the fly escape the first arm, the point leaves so viscid a humour, that the next is sure to be caught. This is infinitely more curious in its formation than the dionea muscipula; which, is also governed by the spiral wire turning over a ball like the mimosa, and drawing the leaves together in the same manner. But drosera is managed in a more peculiar way, and well worthy a drawing, which I will give in my next.

Leatherlike substance the same matter with the spiral wire.

I hope to be perfectly understood, in giving an account of that which regulates the *motion of plants*, that the *leatherlike substance*, and the *spiral wire*, are the *same matter*. The thickness of the first balancing the force the second gains by its spiral form; and the latter gains much strength also from the case that encloses it.

Recapitulation of the proofs.

Before I close this letter you will excuse my recapitulating the proofs brought forward in it and the former. The spiral wire is found in every leaf that has *motion;* in no leaf that does not move; in no *firs*, *grasses*, *sea-weed*, except *confervas*, and the confervas alone of all the tribe have *motion*. It is found in no chenopodiums, salsolas, or ice plants. In leaves that have no other motion than toward the *stem* and back again, the spiral wire occurs only in the midrib of the leaf;

leaf; in all others, it is found even to the smallest spire. The spiral wire is made to stretch, and so does its case. They both contract and dilate in the solar microscope. The spiral wire is found in all the sensitive plants in great quantities, and in every part except the *seminal leaves;* and it is the seminal leaves *alone* that have no motion. I may add, that, when the spiral wires were divided, the leaf would not turn. I think it is hardly possible, where positive evidence is not to be had, to prove a fact in a more direct manner; and that I may say, that plants have spiral wires, which, contracted and dilated by light and moisture, are the cause of all motion in plants.

Sleep of plants.

The sleep of plants is nothing more than the dilating and lengthening of the spiral wire from the evening moisture; and I think the very appearance of it proves it to be so. Does not every violent rain, long continued, produce the same effect? Does not the common acacia, when wetted by continued rain, drop her leaves as at night? So does the gleditsia tricanthos also with even less moisture. The æsculus hippocastanum droops as soon as the leaves are old, and begin to decay. Every plant drops its leaves before the leaves go off. It appears to me, that there is no expression in the human countenance more easy to be understood, than the expression of strength and debility in the appearance of plants. Nor did I ever see a plant close its leaves without showing even an excess of debility in every other part.

Strength and debility in plants.

Plants have no volition.

In one of my two former letters I mentioned two instances of apparent volition in plants, to show how many things of that sort happen, to mislead the judgment: but I have now too often pursued them, till undeceived, through such a course of experiments, perpetually renewed to gain nothing but disappointment, that I am now most absolutely convinced that all plants are merely machines, governed by light and moisture; and that every idea of their sensibility, or of their volition, is only a proof, that we too often let imagination run away with our judgment. Mechanical power is sometimes so delicately managed, that it is difficult to trace it even with the solar microscope; especially as that is of no use till the specimen is most delicately dissected, and placed

placed in order for this purpose. Such fine instruments are required, that a surgeon's collection has scarce need of more variety than the botanical dissecter.

Sensitive plant does not give out much oxigen.

On immersing a specimen of the mimosa in a cylinder of water, to see whether it produced much oxigen, I found what I expected, that the oil in the plant would not permit the water to approach or touch it. It lay quite hollow from the plant, which closed the moment I placed it there; but the next morning, when the sun shone full on it, it opened, and remained thus till night, when it again closed. Again the sun opened it, but from that time for near a fortnight it remained open, and when I took it out of the water it had lost all power of motion, and had I suppose dilated the wire till no longer capable of stretching farther. It gave not much oxigen.

I should apologize for the extreme dryness of this letter; but to explain the formation of any sort of a machine can only be done by the most simple and clear method, and nothing is less entertaining than such a discourse. To advance a few steps nearer to truth is however a certain gain, and if I have made out my proposition to the conviction of those who study the subject, I am satisfied. Those who possess a solar microscope I can only advise to follow me in my experiments, for without seeing it, it is impossible to conceive the amazing effect of light on plants, or almost to imagine what are its powers.

No perspiration in plants.

I will not finish this letter without adding a few words on the perspiration of plants; a subject I have so repeatedly brought forward. I mentioned in one of my last papers, that, on placing a plant in a growing state, under a glass, I put under the glass a paper doubled a few times so as to raise the glass $\frac{1}{20}$ of an inch from the stand; to introduce under the glass the *smallest* quantity of air possible; just enough to prevent the air from *stagnating*, and the plant from becoming *sick* or *discomposed*, and the plant gave out no moisture. This gave me the idea, that it really was the sickness of the plant, which caused the degree of moisture *Duhamel* talks of, but which is certainly *excessively exaggerated*. But for a farther proof I enclosed a large plant in a silver paper case, with a hoop very thin that would preserve it

it from pressing on the plant, and weighing the whole, covering the plant, and tying it close at bottom, I left it six hours. The difference of the weight, when I took it out, was *half a grain;* nor could I feel the least moisture on the paper. On breaking off different branches, and blinding my eyes, I was always able to discover the branch dismembered from the other, provided it had been so for half an hour, from the moist feel of the leaves, that can be compared only to the death-like touch of a dying person. Most plants have this when in sickness, and I am persuaded when confined in stagnant air. I think I may therefore finish *this subject also*, and say, that there is *no sensible perspiration in plants*, and that I *very much doubt* whether there is even *insensible perspiration: if there is, it is most trifling.* Moisture caused by sickness.

I am, Sir,

Your obliged servant,

Cowley Cott. AGNES IBBETSON.

Aug. 20.

Explanation of the Plates.

Plate III, figs. 1, 2, 3, a sting of the nettle in its different states. Fig. 1, its perfect state, when placed in the solar microscope, and before it stings: *z* the bag of poison: *x* the spiral wire. Explanation of the plates.

Fig. 2, the sting after the poison has been thrown to the point: *x* the spiral wire contracted. This is not drawn up at night, when the stings bend, but only when it is touched. If however a mirror be held over them, the poison is thrown up directly.

Fig. 3, the sting of the nettle very much broken.

Fig. 4, untwisted Indian grass greatly magnified, showing the manner in which it is formed. This is the first grass foreign or English, in which I ever found the spiral wire; but I doubt whether it runs through the whole of the grass.

Fig. 5, the awn of the grass.

Figs. 6 and 7, the grass twisted.

Fig. 8, a longitudinal section of the leaf stalk of the mimosa sensitiva, the middle part containing five cases full of spiral wire, and each extremity containing only three.

Pl. IV, fig. 1, a leaf of the mimosa.

Fig.

Fig. 2, the extremity of the leaf stalk, at *p p*, Pl. III, fig. 8, undivided.

Fig. 3, horizontal section of the stem of the sensitive plant.

Fig. 4, part of a case full of the spiral wire much more magnified than in fig. 8 of Pl. III. In many plants it is much thicker, but always loose: that is; it is formed exactly like this, but doubled, or trebled, I imagine to preserve it from the effect the moisture of the nourishing vessels might have on it.

Fig. 5, the spiral wire still more magnified.

VI.

A curious Property of Single Repetends. In a Letter from W. SAINT, *Esq.*

Cromer, Norfolk, Aug. 10th. 1809.

To Mr. NICHOLSON,

SIR,

Single repetends divisible by any number, except 5 and its multiples.

A Friend of mine, some time since, in the process of an arithmetical operation, observed, that any repetend digit, as 111111 &c., or 777777 &c., would divide by the odd numbers 3, 7, 9, and 11; and supposing it probable, that such repetends would divide by *any* odd number, 5 and its multiples excepted, he had the patience to try all such divisors from 1 to 151, and found them to succeed, by taking a sufficient number of digits for the dividend, which, he observed, never *exceeded* the number denoted by the divisor. This property of numbers my friend submitted to me for demonstration, and as it is certainly a very curious one, I thought it probable, that it might not be unacceptable to many of your readers. I have accordingly sent it herewith, in the form of a proposition, accompanied with a demonstration.

I am, Sir,

Your obliged and humble servant,

W. SAINT.

Proposition.

PROPOSITION.

Proposition.

EVERY odd number, except 5 and its multiples, is a divisor of a repetend of any of the nine digits; and the number of digits necessary to form the dividend will never exceed the number expressed by the divisor.

Demonstration.

Demonstration.

First it is evident, that, if we can prove the truth of this proposition for a repetend of units, it must necessarily be true also for a repetend of any other digit, since such repetend would be a multiple of a repetend of units.

Again if the former part of the proposition be true, the truth of the latter part also easily follows; for if 111111, &c. be divisible by any number D, no remainder can *recur*, till after the remainder 0 has occurred; since, if any remainder recurred before the division terminated, the operation would proceed with *precisely the ſame figures* as when that remainder first occurred; and thus this remainder would recur again, and so on ad infinitum; and hence the division would never terminate, or 111111 &c. would not be divisible by D, contrary to hypothesis. Now since all the possible remainders, which can occur in dividing 111111 &c. by D, will be between D and 0 inclusive, therefore there can be but D different remainders; and since, in the operation of division, each figure in the dividend will give one remainder, therefore D figures in the dividend will give D remainders. Hence in dividing 111111 &c. to D places of digits by D, all the different remainders, which can take place, will occur; and therefore, if the dividend were to consist of more digits than are denoted by the *divisor*, some one or more of the remainders would recur; and hence if the division did not terminate *previous to this recurrence*, it would never terminate, but would go on ad infinitum. Consequently, if 111111 &c. be ever divisible by D, it must be so when or *before* the dividend consists of D digits. We say *before*, because, though the remainder 0 might not occur precisely at the *end* of D digits, yet, from what has been shown above, if that remainder

remainder ever occur, it must necessarily do so before the dividend *exceeds* D digits.

Let therefore 111111 &c. to D digits, when divided by D, give q for a quotient and r for a remainder, now since this remainder will recur again, whether 111111 &c. *be or be not* divisible by D, if the number of digits D in the dividend be increased, let therefore 111111 &c. to $D + d$ digits, when divided by D, give Q for a quotient and r for a remainder.— In the first case we have 111111 &c. to D digits $= Dq + r$, and in the second case we have 111111 &c. to $D + d$ digits $= DQ + r$, the difference of these equations gives 111111 &c. 000000, &c. $= DQ - Dq$, where it is evident the units will consist of d digits and the ciphers of D places, whence

$$Q - q = \frac{111111 \text{ \&c.... to } d \text{ digits } 000000 \text{ \&c. D places}}{D}$$

= *a whole number*, where the divisor D is *any number even or odd*. Now the 111111 &c.. to d digits must, *without the ciphers*, be divisible by every *odd* number not greater than the divisor D (5 and its multiples excepted) for if it were *not*, whatever were the remainder, suppose R for instance, we should have R 0000 &c.. to D ciphers divisible by an *odd number not a multiple of* 5, which is impossible; moreover 111111 &c.. to d digits, will not divide by 2, or by 5, or by any multiple of these numbers, since no multiple of 2 or 5 can terminate with 1: hence 111111 &c. to d digits is divisible by any odd number, except 5 and its multiples, where d the number of digits in the dividend can never be *greater* than D the divisor, since the recurrence of any remainder must take place in D digits of the dividend.

Q. E. D.

VII.

On the Use of Iron for Stairs, and instead of the Timbers of Houses, as a Security against Fire. In a Letter from Mr. BENJAMIN COOK.

To Mr. NICHOLSON.

SIR,

Iron recommended for stairs.

IN a former paper I threw out some loose hints on the advantage of employing iron in various articles of furniture, as

as a substitute for mahogany and other expensive woods. I will now add to it a mode of substituting it in the place of oak, and other less expensive woods.

The chief use I would recommend it for is in stairs, and stair cases, but especially in the metropolis, where so many fires are constantly happening, and where so many lives are annually lost by them; where fo many plans have been devised for fire-escapes, and so few, if any, that have ever answered the end.

Security against fires should be provided within doors.

I have long wondered some plan has not been thought of, which provided security within doors, instead of waiting for precarious assistance from without. It is not so easy to introduce a remedy, such a remedy I am now proposing, into houses already built; either from a parsimoniousness of the owners, or from a fancied security in the idea, that with them there is no danger, and therefore they will not go to the expense of adding a new flight of stairs; which beside the expense, will be attended with much trouble and confusion. The other class, that are likely to hinder the adoption of the remedy, are those that are not able to go to the expense of the alteration. But those persons that could afford it, and wished to provide for the danger of fire, if a probable remedy was shown them, might certainly do it; and as houses are continually altering, and new ones constantly being erected, certainly it would decrease the evil, and be introducing, if but slowly, a system that in years would increase, and be of essential utility.

This would be had from iron stairs.

The remedy I mean is stairs and stair cases made either of cast and sheet iron combined, or cast iron only. The framing for the stairs, to which the boards are nailed in the present mode, might all be cast, and screwed together. Of course this framing would be considerably lighter in appearance, than if made of wood. The front and top of the step, if made of sheet iron, might be attached with six or eight screws, to the cast iron framing; and in order to give it a neat finish, a light bevelled moulding might run all round the front of every step, and the jointings be neatly screwed to it with small screws, with heads countersunk into the mouldings.

Mode of constructing them.

But if the front and top of the steps were cast in plates, which

which I think the cheapest and easiest way, the framing might be cast with sunk edges, so that the front and top of the steps would just fit into the groved framing, and four or six screws would fasten them in a few moments. All the tops and fronts, when cast in a mould, would fit in the framing; and all the framing being so cast to fit, a flight of stairs would soon be put together; the plates might all be cast light, and, when all screwed together, would appear a handsome mass of iron.

May be made very handsome.

They who are unaquainted with the method of casting may suppose, that the work would leave the sand rough and uneven; but, if it is cast in fine sand, it will be level and uniform, and be ready for screwing together, the surfaces will be as regular as stone, when put together, and not so liable to wear smooth, and endanger a person to slip off, in coming down stairs. Such stairs will certainly be much handsomer than stone, and of half the price, or less: with this advantage, the railing may match, and be made of cast iron also.

They would appear very beautiful, if well painted, to imitate mahogany, as also the railing, which might be cast in very handsome and various fanciful patterns. There would be much scope for genius and fancy in devising and executing the staircases and railings, as almost any device, almost any antique figure, or gothic scroll, might be tastefully introduced, forming an elegant, indeed I might venture to say, if expense was not the object, the most beautiful, and certainly the most durable, staircases, that can possibly be formed.

Common staircases.

Common staircases of iron would certainly be made as cheap, or cheaper than of oak; and I think, if a manufactory was to be established, and a regular trade made of it, they might afford them as cheap as any kind of wood, and a great deal more work might be put in them, as far as concerns the ornamental part: for the same cast, that formed only straigh tlines, would, by varying the mould, at the same expense, form the most beautiful specimens of antiquity. Therefore wood cannot be brought into comparison with it on the score of taste, nor can price be admitted as an objection to its introduction. Besides, if painting was looked upon

upon as an expense, they would always look well if brushed with black lead; and, as all houses, except the houses of the lower orders, have carpets up the stairs, the tread would be quite as pleasant as on stairs made of mahogany; and in case of fire, a safe escape would always be ready. Dreadful must be the situation of those persons, who, waked by the cry of fire, rush to the landings, find the lower rooms are burning; the staircase blazing and falling; and no escape left but the dreadful one of precipitating themselves from a window, running the risk of being dashed to pieces, or of remaining in the house, to perish in the flames; when, if the stair case had been of iron, all might have escaped with little or no injury.

Iron beams and rafters, & fire proof floors.

If iron was introduced for joists, rafters, and beams, they might all be cast hollow, they might all be screwed and pinned together, and have a very light appearance, at the same time possessing much more strength than wood. If the spars, on which the floor is laid, were made tight and laid near each other; and cast with a small projecting edge on each side at the bottom of each spar, so that, when laid down, to form the floor, a flat tile, or thin quarry, would just fit in between two spars; when all the interstices of the floor were filled up with cheap tiles or quarries made on purpose, the floor would be fire proof, and made so at a very little expense; as the spars might be cast light, there being more in number, and would be nearly if not quite as cheap as wood spars; and all the additional expense would be the common flat tiles, which would not be of much extra value, nor give much trouble in the laying; on which fire proof floor, the boards might be laid.

Communication from room to room.

By the introducing of iron for timber, the danger of fire would be much less to be dreaded; for, if a room took fire, its contents and floor could only be destroyed; and the fire could not easily be communicated from room to room. Indeed I do not see how it is possible for it to extend. The large timbers, that now connect rooms together, would be taken away, which timbers being burnt through, the floor falls, and overwhelms in destruction the rooms and furniture below.

Floors could not fall in, if laid on iron. As only the

boards on them could be burnt, roofs could not fall in, if the beams, rafters, &c. were iron. In fact, a fire could not make its way and spread, if iron was substituted for the timbers now used in building, and few if any lives would ever be lost, if the staircases were made of iron also.

I am, your obedient servant,

Caroline Street, Aug. 22d, 1809, B. COOK.

VIII.

On Respiration. By Mr. J. Acton. *In a Letter from the Author.*

Dear Sir, *Ipswich*, 22d *Aug*. 1809.

Respiration a subject of importance.

AGREEABLY to the conclusion of my last letter, I enter now upon respiration. No subject can be more important, more deserving investigation and serious reflection, than that on which animal life so essentially depends. Whether its utility be referred to the medical and chemical philosopher, as enabling him to take more comprehensive views of the cases submitted to his decision, or as generally increasing the sum of human knowledge, it is still the same. The consequences resulting from a thorough insight into this most important function of vitality exceed all calculation.

Its interruption always produces death:

Some other of the animal functions may be arrested by disease, and their action altogether cease for a time, and the animal shall still continue to live; but in the instance under consideration there cannot be a complete interruption with impunity, whether it take place by immersion in water, or in noxious air, or by a ligature tied round the trachea. Cut off by any means the communication between the lungs and atmospheric air, and the animal dies; the obvious effect is instantaneous, and nearly similar.

but resuscitation may sometimes be affected.

It must be admitted however, that resuscitation by timely interference may frequenty be brought about, after animal life has been for some time apparently extinct; but in many instances a few minutes deprivation are sufficient to destroy the vital spark, beyond the possibility of revival. I do not flatter myself with being

being able to throw much additional light on a subject, which has been already so extensively discussed by men of the highest attainments: the principal end I have in view is merely to endeavour to establish, as in germination, the simple phenomenon of the absorption of oxigen gas by the blood in the lungs, in contradiction to the theory which supposes the emission of solid carbon, and its subsequent union with the oxigen gas of the air to form carbonic acid gas, as stated in my former paper. For this purpose I have confined my experiments to the greatest simplicity, conscious how important it must be to demonstrate this one circumstance beyond any doubt, and thereby afford the means of unerring data for constituting a more distinct theory of respiration: for I believe it is an axiom in natural philosophy, as well as mathematics, that, if the data be founded in errour, the conclusions derived from them must be false. In order therefore to establish any doctrine upon a secure foundation, it would appear very desirable in the first place, to endeavour to remove existing doubts, which perhaps cannot be better done than by the institution and arrangement of a certain number of facts, placed in so appropriate and lucid a point of view, as shall be calculated to carry conviction to the inquiring mind. These only ought to be considered as the pillars and support of every rational theory; and the decay and failure of them from after experiment and improvement should be the signal for its vanishing away, to be replaced by more accurate results. For my own part, in pursuing these investigations I put in no claim for novelty, and my direct object is confined within very narrow bounds. I must confess, that my greatest pleasurable feeling arises principally from the contemplation of the probability of future benefit being derived from them by their stimulating others, who have more energy of mind, with better opportunities and advantages, to resume them with additional ardour, so as to carry them to the greatest perfection they may be capable of. Could I be convinced, that any experiment I shall perform, or any sentence flowing from my pen, may have so desirable an effect, my highest ambition would be satisfied, and I should console myself with the reflection, that I have not lived in vain.

Object of the present paper.

Facts the only secure foundation of any doctrine.

Cruelty to animals.

A great deal has been lately said respecting cruelty to animals. It has necessarily occurred, that, in the following experiments, it has been found impossible to avoid putting them to pain. I can only say, that every wanton infliction of it has been studiously avoided. Those animals which are considered as most noxious and insignificant have been preferred for the purpose. And I have no doubt, if the part of natural history treating of the economy of domestic vermin were more diffused and understood, they would be yielded up without reluctance by the most humane and tender hearted for experimental purposes, where the intention is evidently for instruction and improvement, and not to satisfy mere idle curiosity.

Mice increase very rapidly.

Mice, for instance, it is well known, under favourable circumstances, such as plenty of food and the absence of their natural enemies, increase with the most astonishing rapidity, the female often producing nine young ones at a single parturition. The consequence is, the numbers sooner or later begin to exceed the means of subsistence.

When their food begins to be exhausted, they swallow dirt and sand.

When this first happens, and the food is nearly or quite exhausted, they endeavour to repel the first attacks of hunger by picking up dirt and sand in sufficient quantity to have the mechanical effect of distending the stomach and intestines.

This the stomach soon rejects,

But the delicate coats of the stomach will not long bear the repetition of this artificial food, the sharp angles of the particles of sand at length irritate and inflame that organ, weaken its powers, and compel it to reject it altogether.

and then they prey on each other.

No other resource is then left to the animal, but to try to sustain its existence by feeding on those of its own species it can overcome, thus impelled by the corroding sensations of hunger to recur to this unnatural but only method left them to satisfy the most voracious appetites. And this will proceed till nearly or quite the whole community be extinct, if no opportunities of emigration, or supply of food, present itself. I make these remarks from actual observation: and I am sure it need not be suggested, that often the individual sufferings of these little animals must be very great in this way, without noticing the length of time they are frequently tormented when in the power of their worst enemy the cat; so that it is merciful to increase the means of

Not so cruelly

destroying them. I believe also it will be found that the manner

manner in which they have been treated in these experiments must not be compared in severity to the common mode of exterminating them by poison. It is on the first transient view that humanity shudders at the infliction of pain; could she have patience to listen to adequate reasons for what is done, conviction of utility would often take place of censure. Far, very far be it from me by the least word or action to advocate the cause of cruelty: should I be suspected of so great depravity, I can only say, I feel conscious of deserving no such accusation; it is foreign to my nature; not the poles of the Earth are more distant from each other than is inhumanity in any shape from my genuine feelings. It must however be allowed, and with concern I mention it, that philosophers have sometimes in recording their experiments particularized the most painful operations on animals with an indifference not very characteristic of a tender nature, and sufficient almost to induce a suspicion of a deficiency of the finer traits of sensibility, particularly of that species so masterly pourtrayed and inculcated in the instructive and pleasing writings of Mr. Pratt.

treated in the following experiments as if merely killed by poison.

In the following experiments it will be seen there is a sameness and want of variety bordering upon tediousness, which the simplicity of the fact sought to be demonstrated can alone excuse. I was desirous not to lose any thing for want of repetition; and if by this means a sufficiently strict analogy be apparent in them, the end will be as well answered, as in all probability it would have been by extending them in a more complicated form, which might only have had the effect of rendering the deductions less plain and easy.

Repetition of the experiments in order to establish the fact.

I must premise, without farther apology, that in these as well as my former experiments, it has not been possible to avoid the introduction of small quantities of atmospheric air; but it will appear, that, so far from having any tendency to vitiate these results, they become in most instances a farther confirmation of them.

18 Oct. 1808, Temp. 45°, Press. 29·20.

Exp. 1. An accurately graduated jar being filled with quicksilver and inverted, 17·50 cub. inches of atmospheric

Mouse kept in atmospheric

air

air over mercury 50'. air were passed up (the air of the laboratory being previously examined and ascertained by the average of several trials at a medium temperature and pressure to be composed of 20 parts oxigen and 80 parts nitrogen). A mouse was put through the mercury into the jar, and suffered to remain 50 minutes. When withdrawn the air was found to have diminished 1·25 c. in. On exposure to lime water, a farther absorption was observed of 100 c. in. The remaining 15·25 c. in. being exposed to liquid sulphuret of potash, 00·46 c. in. were absorbed, leaving a residue of 14·79 c. in., which was nitrogen.

Liquid sulphate of iron impregnated with nitrous gas a better test of oxigen than sulphuretted alkali.

It is evident the accuracy of this experiment may be called in question; for, according to the analysis of the air of the laboratory, the whole diminution should have been 3·50 c. in.; but it was only 2·71 c. in., making a difference of 00·79 c. in., which I attribute to the attempt at operating with the whole quantity of gas, instead of taking an aliquot part of it, which I have since done, and always found to be more easy and true. Neither do I think the sulphuretted alkalis so good and rapid tests of oxigen gas as the liquid sulphate of iron impregnated with nitrous gas.

30 January, 1809. Temp. 44°, P. 28·94.

Mouse killed in oxigen gas.

Exp. 2. Into an inverted Jar in the same manner as the above were passed up 13 cubic inches of oxigen gas nearly pure. A mouse was then conveyed through the mercury into the jar, where it was suffered to remain an hour and a quarter, when it was quite dead, and the gas had in that time diminished 1·00 cubic inch. An accident prevented the farther prosecution of this experiment.

Another.

Exp. 3. At the same time another mouse was placed in a like quantity of oxigen nearly two hours. When taken out it was quite dead. The gas had diminished as before 1·00 cubic inch, and being then examined by lime water, 89·50 per cent disappeared, showing, that the animal had absorbed nearly the whole of the oxigen, and given out a considerable quantity of carbonic acid gas.

19 Feb. Temp. 63°, P. 30·10.

Two mice kil-

Exp. 4. Two mice were suffered to die in one cubic inch of

of atmospheric air over mercury. Upon trying the residue with lime water, 12·90 per cent only were absorbed, evidently showing the oxigen was not all consumed; and consequently the animal dies while yet a portion of oxigen remains; most probably from the combined effect of the carbonic acid gas produced, and the effluvia transpiring from the animal's body, and which from this species in particular is most highly offensive and disgusting. ed in 1 c. inch of atmospheric air:

Exp. 5. The above two mice, when taken out of the jar while yet warm, were passed up another inverted jar containing 4 cubic inches of atmospheric air. After remaining four days no material diminution could be perceived. A portion of the air was then tried with lime water, and 18 per cent absorbed. I did not proceed farther with this experiment; for, being convinced how different must be the chemical action of bodies possessing vitality, and those undergoing decomposition, I did not see the utility of endeavouring to trace any analogy between them: but to satisfy myself what were the aerial products arising from the putrefaction of animal bodies cut off from contact with the surrounding air, I instituted the following experiment. and afterward kept four days in 4 c. inches.

Exp. 6. Into an inverted jar filled with mercury I passed up a mouse so recently dead as to be quite warm. The next day a little gas had been produced, which continued to increase, and in seven days amounted to about 2·50 cubic inches. A liquor of a pale red colour had oozed from the body, amounting to about 0·15 parts of a cubic inch of a most fetid and disgusting smell. 100 parts of the gas being exposed to lime water, 81 parts disappeared. The remaining 19 parts being submitted to the test for oxigen, 3 parts were absorbed. The residual gas appeared to be nitrogen. If any ammonia had been formed, it must have been contained in the liquor, and the fetor arising from that was so very powerful, as to prevent my distinguishing or entering into any nice examination respecting it; but I have some reason to believe its formation is considerably facilitated by the presence of atmospheric air. Mouse recently killed in a jar filled with mercury.

19 Feb. Temp. 63°, P. 30·10.

Exp. 7. A mouse being passed up an inverted jar over Mouse in oxigen gas.

quicksilver,

quicksilver, containing 1·62 cubic inch of oxigen nearly pure, after 50 minutes it had diminished 0·19 of a cubic inch. The gas being then exposed to caustic potash only 0·31 of a cubic inch were absorbed.

14 April, Temp. 46°, P. 29·90.

Another.

Exp. 8. Another was placed in the same situation in 1·10 cubic inch of oxigen gas. When nearly dead it was withdrawn, and the air found to have decreased 0·40 of a cubic inch. The remainder being submitted to caustic potash, 0·30 of a cubic inch more were taken up, leaving a residue of only 0·40 of a cubic inch.

15 April, Temp. 45°, P. 29·30.

Another.

Exp. 9. Another was placed in the same situation for 50 minutes in 2·40 cubic inches of oxigen. When the mouse was in, the scale indicated 3·05. At the expiration of the above time it was reduced to 2·80. The decrease would doubtless have been more, but for the casual introduction of some atmospheric air.

19 April, Temp. 42°, P. 29·80.

Another.

Exp. 10. Another placed in the same situation in 1 cubic inch, or 100 parts of oxigen, 29 parts were absorbed. The remaining 71 parts exposed to lime water lost 20 parts more, leaving only 51 parts.

20 April, Temp. 48°, P. 29·60.

Another.

Exp. 11. A mouse was passed up a jar in the same manner into 3·10 cubic inches of oxigen gas. At the expiration of 50 minutes the air had decreased to 2·30, which being exposed to caustic potash, 1·60 were absorbed.

Several mice suffocated in nitrogen.

Exp. 12. Several mice having been suffocated in 4 cubic inches of nitrogen gas, upon trial with lime water it became turbid, and 4 per cent were absorbed.

Mouse suffocated in hidrogen.

Exp. 13. A mouse having been suffocated in hidrogen gas, on examination by lime water a trace also of carbonic acid gas could be perceived.

24 June,

24 June, Temp. 65°, P. 29·95.

Exp. 14. A mouse being placed in the usual manner in a jar, containing 3 cubic inches of oxigen of the purity of 98, in 30 minutes it was quite dead. The air had then decreased by observation 1·00 cubic inch. The residuum being treated with lime water, 33·67 per cent disappeared. This was the average of two trials, and 100 parts taken of the remainder, and submitted to impregnated sulphate of iron, 84 per cent were absorbed, making the whole by calculation stand thus. Mouse in oxigen.

Statement of the gas.

3·00	cubic inches of oxigen gas
1·00	diminished in respiration
2·00	
0·67	absorbed by lime water
1·33	
1·11	absorbed by test for oxigen
·22	Residue, being nitrogen.

At the same time another was passed up into 3·50 parts of oxigen gas, and at the expiration of 40 minutes 0·90 parts were diminished.

10 July, Temp. 62°, P. 29·70.

Exp. 15. In the same manner a mouse was passed up into 1·70 cubic inch of oxigen gas. After it was in, the scale indicated 2·80 cubic inches. When withdrawn in ten minutes, 1·50 cubic inch. 100 parts being then examined with lime water, 21 parts were taken up, and the remaining 79 parts being exposed to the test for oxigen, 67 parts disappeared, leaving a residue of 12 parts, which appeared to be nitrogen. Mouse in oxigen gas.

11 July, Temp. 60°, P. 29·80.

Exp. 16. Another being passed up into 1·60 cubic inches of oxigen gas, the scale then indicated 2·40 cubic inches. When the mouse was quite dead, it had diminished to 2·05; and after it was taken out to 1·25; showing an absorption Another.

of

of 0·35 cubic inches. 100 parts being exposed to lime water, 46 parts were taken up.

14 July, Temp. 65°, P. 29·93.

Another.

Exp. 17. Another being in the same situation in 1·14 cubic inches of oxigen gas, the scale indicated 1·81. In 20 minutes it had diminished to 1·30, an absorption of 0·51 of a cubic inch having taken place. The jar being by accident overset, it could be proceeded with no farther.

Same Time.

Another.

Exp. 18. Another being placed in 1·30 cubic inch of oxigen gas, by the scale it was then 2·18. In 20 minutes it had diminished to 1·70 cubic inch. 100 parts by lime water were reduced to 32 parts, which, exposed to the test for oxigen, left only 13 parts of nitrogen.

Same Day.

Another.

Exp. 19. A mouse being put into 1·15 cubic inches of oxigen gas in the same way, the scale, after it was in, indicated 2·00 cubic inches. At the expiration of 20 minutes it was reduced to 1·47 cubic inch. 100 parts by lime water were reduced to 43 parts; and these, exposed to the test for oxigen, lost 22 parts, leaving 21 of nitrogen.

Same Day.

Another.

Exp. 20. Another being put into 1·10 cubic inch of oxigen gas, the scale indicated 1·80 cubic inch; and at the end of 20 minutes 1·50, there being an absorption of 0·50 of a cubic inch. Out of 100 parts lime water took up 51 parts: the remaining 49 parts being submitted to the test for oxigen, 31 parts disappeared, leaving 18 parts of nitrogen.

Same Day.

Mouse in atmospheric air.

Exp. 21. Another being placed in 4 cubic inches of atmospheric air, the scale then indicated 4·70. In 20 minutes it had decreased to 4·35 cubic inches. 100 parts being then tried with lime water, 13 parts were taken up; and of the remaining 87 parts exposed to the test for oxigen, 6 parts

6 parts were absorbed; leaving a residue of 81 parts of nitrogen.

Same Day.

Exp. 22. Another being placed in 4 cubic inches of atmospheric air, the scale then indicated 5 cubic inches. In 20 minutes it had decreased to 4·80 cubic inches. 100 parts being then tried with lime water, 15 parts were taken up; and of the remaining 85 parts, exposed to the test for oxigen gas, 5 parts were absorbed; leaving a residue of 80 parts of nitrogen. Another.

From the general complexion of these experiments it must be obvious, that, although for reasons easily to be assigned they are not always to the same extent in the decrease of air from respiration, it is still sufficiently demonstrated, there is an absorption of oxigen in every case more or less. Certainly they must be considerably influenced by the state of the animals at the time of the experiment, some of them being more recently caught, and more healthy than others, as well as by the difference in the capacity of the lungs. The noisome effluvia continually emitted from their bodies by transpiration must have its effect, as it appears in no case can the whole of the oxigen gas be absorbed in respiration. Therefore the carbonic acid gas formed, and these effluvia together, terminate their existence, when there sometimes remains even more oxigen than in common air. When animals die in confined portions of atmospheric air, it is also true a portion of the oxigen remains unconsumed, but in much less proportional quantity. It will be seen in the experiments with oxigen gas nearly or quite pure, a proportion of nitrogen has been left, in some instances more than a fifth of an aliquot part of the whole gas tried, which doubtless must have arisen from the introduction of some atmospheric air, when the animals were passed through the quicksilver into the jars, and I have no doubt a little is given out from the lungs. More or less oxigen absorbed in every case by respiration: but not the whole. Less oxigen left in atmospheric air. Nitrogen left after respiration of oxigen.

I should have been content with giving these experiments as they are, and suffering such inferences to be drawn from them as their tendency may warrant, being perfectly satisfied in my own mind the absorption of oxigen is fully proved; Oxigen absorbed in respiration,

proved; particularly from having always observed, when mice are put into this gas, the greatest decrease is always in the first few minutes. And that neither by analogy nor experiment have we any right to assume, that the decrease takes place from the condensation occasioned by the conversion of the oxigen and solid carbon into carbonic acid gas, as supposed by Mr. Ellis. Indeed I do not think I should again have commented on this subject, but by being forcibly struck with the most singular perversion (I dare not say intended) of one of the celebrated Mons. Bichat's experiments quoted from his work, "Recherches sur la Vie et la Mort," the original of which I had an opportunity of seeing for a short time only, in support of this new theory, and which was intended and does absolutely go to demonstrate the absorption of the oxigen gas by the blood. It was my intention to have repeated this very interesting experiment; but being about to institute others, having some relation to it, when opportunity will permit, I have preferred delaying it till that time. Thus stands the quotation in Mr. Ellis's work on Germination, &c. p. 128. "Air, says "Mr. Bichat, thrown into the vascular system, quickly "brings on agitation, convulsions, and death. (P. 179 of "Mr. B.'s work). By forcing air through the windpipe "into the lungs with a syringe, and confining it there, he "has made it to enter into the blood vessels, which imme-"diately brings on agitation and exertion in the animal. "And if an artery in the leg or foot be now opened, the "blood will spring out frothy and full of bubbles of air. "If hidrogen gas has been used, the bubbles may be in-"flamed, and when this frothy blood has flowed thirty se-"conds, the actions of life cease, and cannot be again re-"stored, even although fresh air be applied. (P. 303 of "Mr. B.'s work)."

not converted into carbonic acid.

Oxigen absorbed by the blood.

I regret I have not now by me Mr. B.'s work, and I have not heard of either that or his Anatomie Générale being yet translated. I have however now before me a very copious analysis of it, which will be quite sufficient to enable me to point out the application Mons. Bichat intended by the above passages. But first I hope it will not be deemed improper, if I depart from the subject for a moment. As an enthusiastic

enthusiastic admirer of physiological pursuits, I have experienced the greatest delight in reading an account of Mr. Bichat's labours. I was astonished at the irrisistible manner in which his experiments and demonstrations carry conviction to the mind; and I cannot but deeply lament, in common with every lover of science, that so sublime and ardent a genius should be suddenly cut off in the midst of his useful and instructive career. I believe none of his works have yet been translated into English, nor the originals much diffused through this country. I hope however, if not already done, no long period will elapse before so desirable an object is accomplished. His physiological and anatomical writings deserve to be most carefully studied, particularly by those of the medical profession, ere they can be duly appreciated. I am by no means competent to decide upon the merits of performances executed by so extraordinary a man. I can only say, if upon a careful perusal they shall leave upon the minds of others the same strong impressions, that a partial knowledge of them has left upon mine, they can never be obliterated; and I have no doubt of their occasioning so much ardour and discussion in the progress of these pursuits, as will eventually be productive of the most beneficial consequences to mankind, by fixing the structure of medical science upon the immovable basis resulting from the combination of the most liberal and enlightened theories with the most decisive facts and practical experience. Hence shall arise out of the ashes of unstable and departed hypothesis a permanent superstructure, invulnerable to the attacks of mere speculatists, and which, though solid and immovable in itself, shall still admit of being improved and beautified by the labours of present and future artists. I trust I may be excused for thus taking the liberty of paying this trifling tribute of admiration and esteem to the memory of the ever to be lamented Bichat, though he was never known to me but through the medium of his writings. From the sketch of his life, which I have read, I may be confident in stating, that my veneration cannot be misplaced on him as a man, however my ability to understand and value him as a writer may be readily called in question—that he was one of those, who will ever have a

Bichat.

first

first rank among the illustrious dead, will not I believe, be disputed.

Mr. Ellis asserts,

But to return to the purpose, for which the above quotation was made. It is necessary, for the illustration of my proposition, I should make a short extract from Mr. Ellis's work. At page 198 he says: "We have endeavoured to "prove, that no gasses either exist in the blood, or can be "transmitted through the vascular and cellular structure in-"terposed between the air and that fluid in the lungs: con-"sequently no oxigen can enter into the blood, to unite "with its supposed carbon; nor, if such union did take "place, could the carbonic acid be afterward expelled from "that fluid."

that no oxigen enters into the blood.

Bichat found hidrogen gas enter the blood through the lungs.

Now is it not wonderful, that Mr. Ellis, writing thus, should make the before mentioned extract from Mr. Bichat in proof of it; which, instead of being so, goes directly to contradict it? for in the analysis of his work before me, after stating the injection of hidrogen gas into the lungs, and keeping it there by a ligature on the trachea; and demonstrating its passage into the blood by opening an artery, and presenting a lighted taper to the air bubbles formed on the surface the blood which issues out; he thus continues: "*This affords a proof of the passage of air into the blood* "THROUGH THE LUNGS, *in* ADDITION *to that of healthy re-*"*spiration*, &c.

Air injected into the blood vessels kills by its mechanical action.

The injection of air into the veins or arteries occasioning the destruction of animal life can be no proof, that oxigen gas is not chemically absorbed by the blood in the lungs in healthy respiration; for in the blood vessels it evidently kills by its mechanical action only. Neither does the deleterious effect take place in the heart, as has been supposed; for Bichat has shown, and indeed I myself have often seen, that the heart beats long after the signs of animal life are extinct. Air injected into the carotid artery has the same effect as in the veins, with the addition of agitating the heart by contact as a mechanical body. And also if injected into the vena portæ, but taking a much longer time before the animal is affected, as the capillary circulation of the liver prevents its arrival so soon at the brain. And hence it has been concluded, that the death of the heart is the effect, and not the

The deadly effect does not take place in the heart.

Death of the heart the effect

ause of the death of the brain. But the injection of to the crural artery never proves mortal, though it oc- is a paralysis of the muscles.

of that of the brain.

'. Ellis could not surely for a moment suppose, that ise the absorption of oxigen gas by the blood in the in healthy respiration is contended for, therefore the must take place with respect to hidrogen, or any other itic gas; or else with neither: and yet such is the in- ce naturally presenting itself upon comparison of the two quotations. As an attentive observer of the chan- nsuing in blood by its conversion from venous to arte- I am firmly persuaded, it is by chemical affinity alone, not a mere mechanical absorption, such as would take with water and oxigen or carbonic acid gas. It is only essure that hidrogen gas can enter the blood vessels, natural inspirations of that air no such effect can be vered as that by the lighted taper; and judging from gy we may conclude the same of the rest of the mephi- rs. Besides, it has been ascertained, that the whole is thrown out of the lungs unaltered in the next expira- ; and, as we have already seen, when oxigen gas is hed, it is far otherwise.

Nonabsorption of mephitic gasses by the blood in the lungs no proof, that it does not absorb oxigen.

e blood, in circulating through the pulmonary vessels, nts itself to the air cells to receive its accustomed sup- f oxigen; and when noxious airs are respired, or respi- n suspended, being continually disappointed it still towards all the organs of the body, and their arteries me filled with black blood, till at length the animal be- s asphixated: that is, the volume of the blood returned e veins is increased; the venous blood not having the r to stimulate the organs of secretion, their functions in unperformed; the matter that should be secreted re- therefore with the mass; while the venous blood, cir- ing in the bronchial arterie produces the same delete- effect on the lungs, as on the other organs; and from leficiency of oxigen, which is to the air cells what food the stomach, the cells cease to be expanded; and at h, by producing a similar effect on the walls of the t, so enfeebles its contractions, it cannot surmount the tance set up by the lungs. And, as Bichat energeti-

Manner in which death is brought on from a defect of oxigen in the lungs.

cally

cally expresses it, when once the black blood, (that is the blood which has absorbed no oxigen) has penetrated the heart's tissue, it is dead to sympathy, as well as direct stimuli. Hence it may be inferred, that, in suspended animation from drowning or otherwise, till this fatal effect takes place upon the heart, it is capable of sympathizing in the excitement of the lungs by inflation: that is, it continues susceptible of the impression or action of the oxigenated blood. This knowledge ought to be a still more powerful inducement with us, to persevere in our efforts to restore our fellow beings when they have been by any means accidentally suffocated: and perhaps one of the most powerful auxiliaries we could make use of for this purpose would be the judicious application of pure oxigen gas for the inflation of the lungs, which must evidently be more effectual than common air, and might always be kept in readiness over water in the usual places appropriated for containing the proper resuscitative apparatus, and highly creditable to the philanthropy of some of the inhabitants is it to say, that in this town there are several.

Resuscitation.

Oxigen gas a powerful auxiliary of resuscitation.

Upon examining the lungs of the mice which died in oxigen, their vascular substance appeared to be engorged with dark red blood; and on observing the liver I found it to be much lighter coloured than usual, no doubt from deficiency of blood. It would naturally have been expected, that, from the action of the oxigen gas on the blood in the lungs, it would have appeared of a florid colour: but in consequence of the great excitement, occasioned by the rapid absorption of this gas in respiration, to the lungs, intercostals, and diaphragm, the mechanical action of the lungs must have first ceased, and several rounds of circulation must afterward have gone on from the continued action of the heart, which I have sometimes known to beat nearly an hour after the discontinuance of the animal functions.

State of the sanguineous system in mice killed in oxigen,

In those which died in atmospheric air, nitrogen, hidrogen, and carbonic acid gasses, I invariably found the lungs collapsed and empty, and the liver full of blood. The circulation in these cases being more suddenly interrupted, time enough was not allowed to fill the vessels of the lungs.

and of those killed in other gasses.

The effect of oxigen on animals placed in it appears to be

An atmos-

be similar to what occurs when they are exposed in a room of atmospheric air heated many degrees beyond the temperature of the body: the excitement in each case is equally great; and, if it continue, death will be eventually occasioned in both from the same cause, the too rapid absorption of oxigen. In heated air the circulation is proportionably quickened, and a larger surface of blood is of course presented to its influence. And the action by chemical affinity between the blood and the oxigen is no doubt in such circumstances considerably increased. I have sometimes seen dogs sleeping by a large fire excited to such a degree, as at length to respire with great difficulty; the action of the diaphragm has been very violent; and they have in consequence awoke, and been compelled though reluctantly to move.

phere highly heated acts like oxigen gas.

But the most deleterious and noxious of all the gasses to animal life is the oxigenized muriatic acid. If it be pure and recent, animals die the instant they are put into it, and the effects upon the thoracic viscera are most dreadful.

Oximuriatic acid most deleterious.

In examining some mice suffocated in it, I found the lungs converted into a dark purplish brown pulp; the heart, auricles, and vessels, become black, dry, and corrugated; the membranes and other parts nearly destroyed; and the blood coagulated into a mass like an electuary. The brain too appeared much inflamed when compared with some that had died in a different manner. But in so small an animal the symptoms cannot be very accurately traced; nor can any other than general observations be made, without considerable difficulty and patience. Death by this means seems to be synchronous; that is, the action of the acid gas is not referrible to any particular organ, but kills the lungs, heart, and brain, all at the same time.

Its effects on mice.

I have often inspected animals suffocated by drowning, and have always found the lungs distended by a quantity of air remaining in the cells; and as this must consist of carbonic acid and nitrogen, might it not be proper, in cases of suspended animation, to draw it out of the lungs by an exhausting syringe, previously to the inflation of them with the oxigen gas?

Drowned animals.

By the politeness of Mr. Stebbing I was permitted to examine

Lungs of persons stran-

examine the lungs of one of the two criminals, who were executed here on the 31st ult. for murder, and delivered to him for dissection. The cells contained a considerable quantity of air; and from the room the lungs appeared to take up in the thorax, I should imagine, they must have been nearly distended to their usual size, as in a living state. Undoubtedly there must be some variation in the appearance of different subjects; and, whether vitality cease suddenly, or in a more gradual manner, the effect on the lungs will in a great measure be determined by it.

Having extended my remarks on this subject farther than I at first intended, I shall defer entering upon vegetation &c. till a future opportunity.

IX.

On the Camera Lucida. In a Letter from Mr. R. B. Bate.

Superiority of the camera lucida.

" The camera lucida is portable in a very small compass; " it represents objects with more brilliancy and distinct- " ness than the camera obscura; and it represents them " either singly or in combination with perfect truth and " correctness of perspective. What disadvantages has " it then to counterbalance these particulars, in which " it is evidently superior in a very great degree to the " camera obscura?"

See Supplement to Vol. XXIII of Nicholson's Journal, page 373.

To Mr. NICHOLSON.

SIR,

Mr. Sheldrake's statement too unfavourable to it.

YOUR correspondent, Mr. Sheldrake, after passing the above encomium on the camera lucida, has put the query which follows, and answered it; but in a manner ill calculated to lead to a fair conclusion upon the subject of his investigation. And, as I have found the camera lucida not only less deficient in the points to which he refers, but to posses many advantages which he appears to have overlooked, I feel induced to state them in a more familiar manner than

than has hitherto been done; being persuaded, that some of these advantages are not generally known, and likewise influenced by a wish to see justice done to the merit of an invention, which deserves to be better understood, and which is peculiarly admirable for its correctness and simplicity.

It has advantages not generally known.

You very justly remark, that, in using the camera lucida, it is certainly intended "the tracing should be made upon that part of the paper, where the picture and point of the pencil can both be seen coincident; and not that a copy should be taken in the manner described by Mr. Sheldrake." But it is matter of regret, that you should not have enlarged upon the effect of varying the position of the eye; in describing which the ingenious inventor has not been sufficiently minute, as is strongly instanced by the misconception manifested in the case before us.

Method of using it.

The management of the eye has not been described minutely enough.

Mr. Sheldrake evidently confines the camera lucida to the purpose of bringing the reflection of some of the objects upon the upper part of the paper, for the approximated convenience of copying them upon the lower part; instead of placing his peneil among the images themselves, and "rendering them permanent," by tracing their outline at once, as himself states to be done in the camera obscura. As Mr. Sheldrake seems sensible of the advantage of moving his eye to the right and left, it is the more extraordinary, that he should confine himself to that motion, when the transverse motion of the eye is the most obviously important.

Mr. Sheldrake's method of using the instrument erroneous.

In copying a landscape the instrument is to be fixed upon a steady table or board, on which a sheet of paper is stretched, and the prism brought over the middle of it: the open face of the prism is to be placed opposite the centre of the view; the black eye piece, or stop, being in a horizontal position, is to be moved till the lucid edge of the prism intersects the eye hole. The eye should now be brought close to this opening, and, upon looking through it vertically towards the paper, a perfect copy of the view will appear reflected upon it, and the reflected images will be large in proportion to the elevation of the prism. The eye hole should now be drawn farther off the prism, so as to

Proper method.

leave a representation of the object barely distinct, for the more complete command of the pencil.

Management of the eye.

The whole apparatus remaining stationary, it will be found, that, by moving the head so as to carry the eye farther over the prism, and looking inwards, the view will be continued upon the lower part of the paper; and by drawing the eye off towards the edge of the prism, and looking the contrary way, the view will be continued upwards: thus the reflection of every object comprised within an angle of 45° in height and depth will in succession be distinctly seen; and by a diagonal inclination of the eye towards the right and left a horizontal compass of the landscape equal to an angle of 80° may also be obtained, and few will be dissatisfied with this field of view.

Field of view.

Method of obtaining a clear sight of the image and pencil at the same time.

The pencil may now be employed in following the outline of the images, and, if their brightness should any where impede distinct vision of the former at the point of coincidence, a slight motion of the eye towards the edge of the prism will obtain it, and vice versa when the image is not sufficiently distinct. It may not be amiss to recommend generally the near edge of the prism to be kept in a line with the pencil and the image; for which purpose it will obviously be necessary to move the head in a direction opposite to the motion of the pencil, that the eye may follow it, and keep it in contact with the lower edge of the picture, or rather, the edge of that part of the reflection which is at the instant visible.

Directions for copying a near or very tall object.

When the instrument is used in copying a near or very tall object it may occasionally be found, that, in following the image towards the upper part of the paper, the eye will be confused with the original reflection, which is coloured and inverted; it will then be necessary to enlarge the field of view, by turning the prism upon its pin, slightly inclining its face upwards, and depressing the near edge of the stop; which may be done without inconvenience, for, while the pin is strictly confined to a motion in that direction*, the

* To ensure the confinement of this motion to the vertical direction, a small clamp for the top of the outer stem will be found useful; this may be tightened as soon as the elevation for the prism is determined on, and will answer to prevent the inner stem from sliding down or turning round.

images

images will not be in the least shifted from their places on the paper, which is a great advantage belonging to this instrument.

A much more important advantage peculiar to the camera lucida is the essential benefit a young artist may derive from a limited use of it. For instance, to have the outline of one or two objects, situate near the middle of the view, as reflected by the prism: and afterwards to look directly at the view itself, using the upper edge of the prism as a guide for the point of observation. His eye and judgment may then be exercised in determining, by this outline, the relative magnitudes and distances of the remaining objects; occasionally referring to the reflection of them in the prism for their true situations in comparison with those his judgment has assigned them: and these corrections, attentively observed, seem capable of affording the most valuable aid in cultivating a delicacy of discrimination. The finished artist will also find a great economy of time, upon extensive and complicated subjects in particular, by using the instrument in determining the situations of so many points as he may deem important; and which the camera lucida is allowed to give "with perfect truth and correctness of perspective."

Peculiar advantage of the camera lucida to the student,

and to the artist.

Though hitherto omitted, it is proper to notice the frequent impediments to an extent of view, arising from the projection of near objects; parts of the head-dress in particular are sometimes unsuspected obstructions, and the brim of the hat the most formidable of all.

View obstructed by the brim of the hat &c.

Dr. Wollaston has briefly adverted to the method of enlarging a drawing, or delineating minute objects as magnified; by bringing the eye piece to a vertical position and looking directly at the object through the eye-hole and the lens, which must be turned up likewise to the same position; the paper and pencil then appear reflected in front of the object, more or less distinctly, according to the quantity of prism exposed to the pupil: and a delineation of the object may be obtained large in proportion to the magnifying power of the glass and the surface of the paper occupied. To this I beg to add, that a compound microscope may be used in the same manner, but more conveniently with the horizontal

An object may be drawn magnified by the camera lucida.

A compound microscope may be used

in the same manner.

horizontal position of the eye hole, by bringing the microscope to the same position, and the face of the prism close to its first eye glass. A telescope may likewise be employed; having previously removed the head or cover, the face of the prism must be brought into contact with the eye glass, to which it serves as a diagonal eye piece; a distant object is then approximated, appears reflected upon the paper as before, and may be delineated in a manner at once pleasing, novel, and correct.

The camera lucida superior to every other contrivance.

These combined advantages, and above all, the truth of the reflected image under every circumstance, give the camera lucida a decided superiority over all other known contrivances for the same purpose. And if the hints I have offered should enable Mr. Sheldrake, or any of your readers, to derive farther gratification in the use of the instrument than they have hitherto received, or call to notice any unexpected purpose, to which it may be applied, I shall be happy in having contributed, however poorly, to that gratification.

I am, Sir,

Poultry, 12*th Sept*. 1809. Your very obedient servant,

R. B. BATE.

X.

An Account of some Experiments, performed with a View to ascertain the most advantageous Method of constructing a Voltaic Apparatus, for the Purposes of Chemical Research. By JOHN GEORGE CHILDREN, *Esq. F. R. S**.

Best construction of a voltaic battery desirable.

THE late interesting discoveries by Mr. Davy having shown the high importance of the voltaic battery, as an instrument of chemical analysis, it became a desirable object to ascertain that mode of constructing it, by which the greatest effect may be produced, with the least waste of power and expense.

Battery after

For this purpose, I made a battery, on the new method, with

* Philos. Transact. for 1809, p. 32.

with plates of copper and zinc, connected together by leaden straps, soldered on the top of each pair of plates; which are twenty in number, and each plate four feet high, by two feet wide: the sum of all the surfaces being 92160 square inches, exclusive of the single plate at each end of the battery. The trough is made of wood, with wooden partitions well covered with cement, to render them perfectly tight, so that no water can flow from one cell to another. The battery was charged with a mixture of three parts fuming nitrous, and one part sulphuric acid, diluted with thirty parts of water, and the quantity used was 120 gallons. the new method. Plates of 4 feet by 2. Surface 92160 square inches.

In the presence, and with the kind assistance of Messrs. Davy, Allen, and Pepys, the following experiments were made.

Experiment 1. Eighteen inches of platina wire, of $\frac{1}{30}$ of an inch diameter, were completely fused in about twenty seconds. Its effects.

Exp. 2. Three feet of the same wire were heated to a bright red, visible by strong day-light.

Exp. 3. Four feet of the same wire were rendered very hot; but not perceptibly red by day-light. In the dark, it would probably have appeared red throughout.

Exp. 4. Charcoal burnt with intense brilliancy.

Exp. 5. On iron wire, of about $\frac{1}{70}$th of an inch diameter, the effect was strikingly feeble. It barely fused ten inches, and had not power to ignite three feet.

Exp. 6. Imperfect conductors were next submitted to the action of the battery, and barytes, mixed with the red oxide of mercury, and made into a paste with pipe-clay and water, was placed in the circuit; but neither on this, nor on any other similar substance, was the slightest effect produced.

Exp. 7. The gold leaves of the electrometer were not affected.

Exp. 8. When the cuticle was dry, no shock was given by this battery, and even though the skin was wet, it was scarcely perceptible.

Before I offer any observations on the inferences to be drawn from these experiments, I shall mention some others, performed, for the sake of comparison with the foregoing, with

means of a fine screw, through a collar of leathers, and the distance between the points was ascertained by a small micrometer attached. This receiver was inverted over well dried potash over mercury, and suffered to stand a couple of days, to deprive the air it contained, as thoroughly as possible, of moisture. The 1250 plates being excited precisely to the same degree as the great battery, mentioned in the beginning of this communication; and the little receiver placed in the circuit, I ascertained its striking distance to be $\frac{1}{50}$ of an inch. That I might be certain, that the air in the apparatus had not become a conductor by increase of temperature, I repeated the experiment several times with fresh cool air, and always with the same result; but perhaps it will be objected, that the striking distance was so small, as not to afford a satisfactory refutation of the argument alluded to, when it is considered to how very great a distance, comparatively, the spark of the common electrical machine can pass through air. The answer to this is obvious: increase the number of the plates, and the striking distance will increase; for we see throughout, the intensity proportioned to the number, and it probably may be carried to such extent, as even to pass through a thicker plate of air, than the common spark. The great similarity of the appearance of the electric light of this battery in vacuo, and that of the common machine, might also be urged as an additional proof of the identity of their nature.

Striking distance ·02 of an inch, with 1250 plates.

This distance might be increased.

Another proof of their identity.

The effect of this large combination on imperfect conductors was, as may be supposed, very great; but of the same platina wire, of which the four-feet plates fused eighteen inches, this battery melted but half an inch, though, had the effect been in the ratio of their surfaces, it should have fused nearly fourteen inches.

Numerous combination fused but little platina.

The absolute effect of a Voltaic apparatus, therefore, seems to be in the compound ratio of the number, and size of the plates: the intensity of the electricity being as the former, the quantity given out as the latter; consequently regard must be had, in its construction, to the purposes for which it is designed. For experiments on perfect conductors, very large plates are to be preferred, a small number of which will probably be sufficient; but where the resistance of

Effect of the apparatus in the compound ratio of the number & size.

of imperfect conductors is to be overcome, the combination must be great, but the size of the plates may be small; but if quantity and intensity be both required, then a large number of large plates will be necessary. For general purposes, four inches square will be found to be the most convenient size.

4 inches square a convenient size.

Of the two methods usually employed, that of having the copper and zinc plates joined together only in one point, and movable, is much better than the old plan of soldering them together, through the whole surface, and cementing them into the troughs: as, by the new construction, the apparatus can be more easily cleaned and repaired, and a double quantity of surface is obtained. For the partitions in the troughs, glass seems the substance best adapted to secure a perfect insulation; but the best of all, will be troughs made entirely of Wedgwood's ware, an idea, I believe, first suggested by Dr. Babington.

Plates joined together in one point only preferable.

Troughs of Wedgwood's ware best.

XI.

Report of a Memoir of Mr. HASSENFRATZ, *respecting the Alterations, that the Light of the Sun undergoes in traversing the Atmosphere. By Mr.* HAUY *.

THE class of physical and mathematical sciences having directed Mr. Laplace and me to examine a paper of Mr. Hassenfratz on the changes that the solar light undergoes in passing through the atmosphere, we shall proceed to give an account of it.

The light of the sun being composed of an infinite number of rays of different tints, the union of which forms white, we should always see it white, if it came to us in the state in which it is emitted from that body. But in passing through the atmosphere it frequently undergoes alterations, that change its appearance, so that there are circumstances in which it appears to us with its natural whiteness, and

The sun would always appear white, if its rays were not affected in passing thro' the atmosphere.

* Journal de Physique, vol. LXVI, p. 356.

others

others in which it appears yellow, orange coloured, or red. According to Mr. Hassenfratz these different effects depend in general on the state of the atmosphere, the difference of the latitude, and the elevation above the sea. As to the ultimate cause of these phenomena, it was natural to ascribe them, as Mr. Hassenfratz does, to the suppression of a part of the rays of the solar light in its passage through the atmosphere. Newton has already announced this property of transparent mediums, to stop certain of the rays that enter them, letting the rest pass on; and this celebrated philosopher even remarks, that they are frequently absorbed one after another at different distances from the surface at which the light entered; and he quotes for example the various tints exhibited in succession by a coloured fluid in a conical glass, which is placed between the eye and the light, and raised so as to have the thickness traversed by the visual ray continually increasing.

Object of the author to determine the kind and quantity of rays intercepted.

Now Mr. Hassenfratz proposes to determine the number and kinds of rays, the suppression of which occasions the various tints, that alter the primitive whiteness of the solar light. The means he has employed are founded on a rule given by Newton, to determine the colour produced by a given mixture of rays of different kinds taken from those that compose the solar spectrum. It follows from this, that, if we can know the sorts of rays that the atmosphere takes away from the solar light, we shall know by necessary consequence the colour produced by the mixture of the species remaining, and we may judge whether this colour be the same as that, under which the disk of the sun presents itself. Here we must observe, that the mixture producing a given colour may be more or less compounded, because a colour does not change, at least with regard to its species, by the addition of parts of the spectrum situate on each side at equal distances from the point considered as the centre of this colour. For instance, if we add to the green its two contiguous colours, blue and yellow, we still have green; and the mixture will remain green, if we farther add indigo and orange, one of which is contiguous to the blue and the other to the yellow. Nothing but direct experiment therefore can indicate the species of rays absorbed in their passage,

sage, when the disk of the sun appears yellow, orange, or red.

Mr. Hassenfratz concluded, that the observation of the solar spectrum produced by the refraction of the prism would lead him to his object, because, the spectrum being necessarily incomplete from the absence of the rays intercepted in their passage, the determination of the deficiencies in the spectrum would indicate these; and it might afterward be ascertained whether the colour resulting from a mixture of the rays remaining would be the same as that of the solar disk. This to be done by observation of the spectrum.

Mr. Hassenfratz cites several results of the experiments he made under the various circumstances of which we are speaking. Thus on the 13th of January, 1807, having observed the spectrum at ten o'clock in the morning, he found the violet wanting, with part of the indigo. Now according to Newton's rule, if the violet be suppressed, with a certain portion of the indigo, the remaining colours are those, which, by their mixture, produce yellow: and the disk of the sun appeared of the latter colour. As a necessary consequence, the yellow of the spectrum was deeper than ordinary. The same day at noon, the sun was white, and the spectrum then had its whole extent. But at four in the evening the violet had disappeared anew, with a greater quantity of indigo, so that the sun appeared of a deeper yellow than at ten in the morning. Lastly, at a quarter after four the spectrum was shortened on the same side, and in consequence the solar disk inclined to orange. Observations made.

Mr. Hassenfratz presented to the class several coloured drawings of the solar spectrum, such as he observed them in circumstances where it had lost more or less of its length. The drawings were made at the moment of the experiment by Mr. Gerard, at the Polytechnic School. Drawings of the spectrum taken.

The author adds, that he has sometimes remarked the effects of the subtraction of several rays in rainbows seen at different hours of the day, which have exhibited varieties in the number or extent of the coloured arcs. Subtraction of rays from the rainbow.

The experiments are interesting in themselves, because they serve to explain in a natural and satisfactory manner some phenomena, on which we had not any thing precise. They

They deserve the attention of the natural philosopher also from the influence these phenomena have in experiments relating to the decomposition of light.

SCIENTIFIC NEWS.

Cultivation of fruit trees.

MR. van Mons, of the Institutes of France and Holland, is publishing a "Theoretical and Practical System of Fructiculture, or Instructions for the Work of the Nursery and Fruitgarden in the Order of the Months." The extensive correspondence of the author having brought him acquainted with all the improvements lately made in this branch of science by a great number of persons distinguished for their education and talents, who have withdrawn from the fatigues of war or the toils of politics, or who, grieving at public and private calamities, or chagrined at the ingratitude and injustice of mankind, have retired to forget their sorrows in the quiet enjoyment of their gardens, he has conceived he should be rendering a service to many, by making them more generally known. The work, which commenced in January last, and will finish with December, is on the principle of a gardener's calendar, and will include every thing relating to the culture of fruit. It will give in detail the whole management of fruit trees in the nursery and in the garden, not from books however, but from the author's own experience, and the communications of his friends.

Middlesex Hospital.

Medical Lectures.

Dr. SATTERLEY's Course of Clinical Instruction at the Middlesex Hospital will begin the first week in November: the attendance on the patients will be continued daily, and Lectures will be given once a week, or oftener, when it may be necessary, at eleven o'clock. Mr. CARTWRIGHT, Assistant Surgeon to the Hospital, will undertake such occasional demonstrations of morbid anatomy, as may be required for the illustration of the respective cases. The objects of the Course will also be extended to such remarkable

peculiarities

peculiarities in the diseases of children, as may occur in the Foundling Hospital.

Dr. YOUNG will begin his Elementary Lectures on Chemistry, Physiology, the Practice of Physic, and the Materia Medica, about the middle of December: he will deliver them on Mondays, Wednesdays, and Fridays, at 7 o'clock in the evening, throughout the season. Chemical and Medical Lectures.

St. George's Hospital, and George Street, Hanover Square.

On Saturday, October the 7th, a Course of Lectures on Physic and Chemistry will recommence in George street, at the usual morning hours, viz. the Therapeutics at eight; the Practice of Physic at half after eight; and the Chemistry at a quarter after nine. By GEORGE PEARSON, M.D. F.R.S. Senior Physician to St. George's Hospital, of the College of Physicians, &c. Medical and Chemical Lectures.

Clinical Lectures are given as usual on the patients in St. George's Hospital, every Saturday morning, at nine o'clock.

Dr. SQUIRE will begin a Course of Lectures on the Theory and Practice of Midwifery, and the Diseases of Women and Children, on the 3d of October, at his house, Ely Place, Holborn. Obstetrical Lectures.

The notice received from Dublin, and given in our Journal, No. 103, p. 239, that Professor Davy intended to give a Course of Lectures on Galvanism in that city, was erroneous; it being incompatible with the situation and duties of that gentleman to lecture in any other place than in the Royal Institution, where the usual Courses must be in progress at the very time mentioned in the said notice.

To CORRESPONDENTS.

Mr. Rootsey's paper is obliged to be set aside, from my printers being unable to procure from the letter-founders the requisite types.

Mr. Singer's communication will appear next month.

J. S. of Hatton-garden, is deferred on account of the engraving. In the mean time I should be glad, if he would favour me with his address.

METEOROLOGICAL JOURNAL,

For SEPTEMBER, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

AUG. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
26	58	60	62	52	29·80	Rain	Cloudy
27	57	61	63	54	29·75	Ditto	Fair
28	59	61	67	55	30,00	Fair	Ditto
29	62	65	71	62	30·08	Ditto	Ditto
30	66	68	73	62	29·89	Rain †	Ditto *
31	63	62	66	58	29·85	Cloudy	Ditto
SEPT.							
1	60	62	64	57	29·80	Ditto	Ditto
2	60	64	69	60	29·68	Rain	Ditto
3	64	65	71	59	29·60	Ditto	Ditto
4	64	64	69	58	29·62	Ditto †	Cloudy
5	63	62	68	58	29·54	Ditto	Ditto
6	64	62	68	57	29·52	Ditto	Ditto
7	62	60	67	54	29·27	Ditto	Ditto
8	57	59	65	52	29·34	Ditto	Ditto
9	59	61	67	53	29·58	Ditto	Fair
10	60	60	64	50	29·67	Rain	Ditto
11	57	56	64	48	29·70	Ditto	Ditto
12	56	56	64	47	29·78	Fair	Ditto
13	54	56	64	53	29·85	Rain	Cloudy
14	58	58	62	54	29·68	Ditto	Fair
15	60	58	64	49	29·98	Fair	Ditto
16	57	59	63	49	30·14	Rain	Rain
17	55	58	64	52	30·00	Fair	Fair
18	58	56	64	5[illegible]	29·90	Rain	Ditto
19	55	55	64	50	29·72	Fair	Ditto
20	55	56	65	49	29·47	Rain	Ditto
21	55	56	65	5[illegible]	29·64	Fair	Cloudy ‡
22	58	62	66	54	29·60	Rain	Fair
23	61	56	65	50	29·64	Ditto	Ditto
24	56	50	58	48	29·85	Ditto	Ditto
25	53	48	53	40	29·70	Ditto §	Ditto
26	43	50	53	47	30·00	Fair	Cloudy ‖

* Lightning in the East.
† Thunder, at 1 P.M.
‡ Hazy moon.
§ The maximum of the thermometer at 9 A.M. gradually subsiding during the day. The morning cold and rainy.
‖ Hazy moon.


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
z
x
x
e
p
p
e
m
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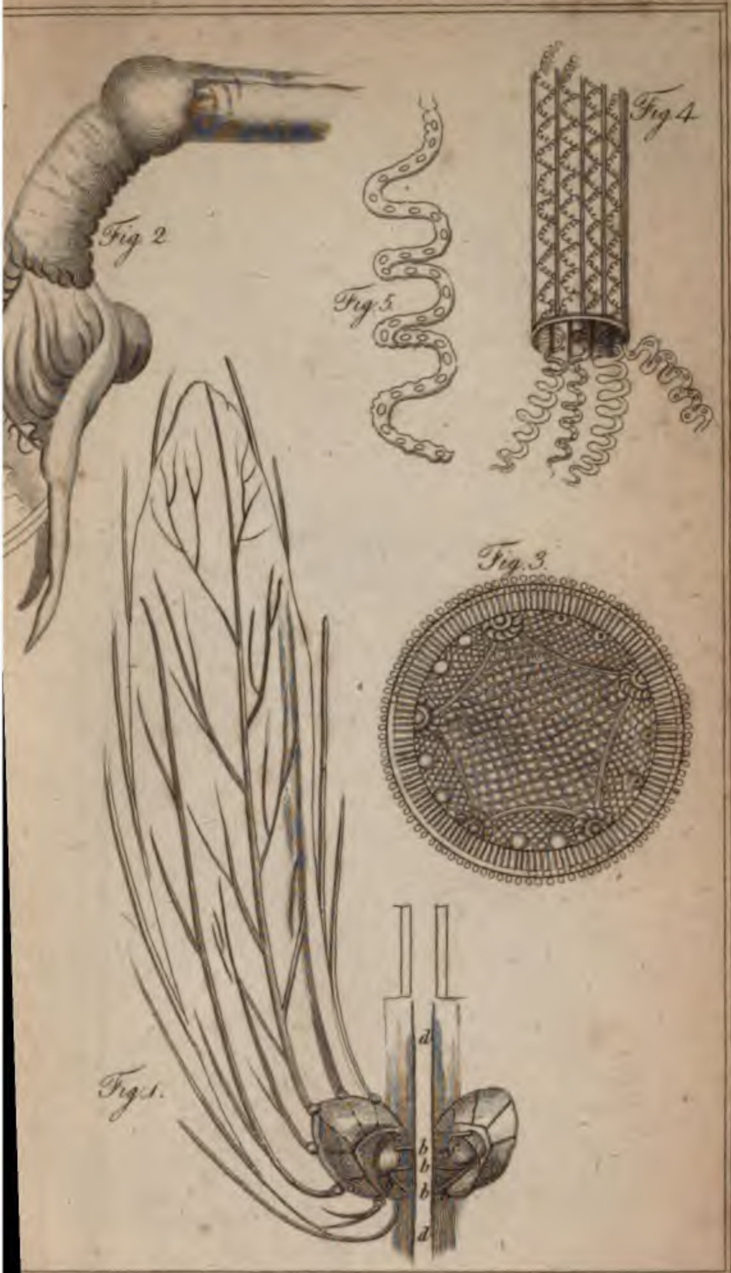
Fig. 2
Fig. 5
Fig. 4
Fig. 3.
Fig. 1.
d
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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1809.

ARTICLE I.

Account of some luminous Meteors seen during a Thunder Storm. In a letter from JAMES STAVELEY, *Esq.*

To Mr. NICHOLSON,

SIR,

RETIRING rather late to bed last night, and throwing up the window to admire the beauty of the lightning, I was struck with the appearance of the sky, the grandeur and singularity of which I never remember to have seen equalled. The time was about half past one o'clock. Considering that facts of this kind are at all times acceptable to the meteorologist, and that this may perhaps serve to elucidate some of the mysteries of that science yet unfathomed, if you have no better account of the phenomenon, may I offer this for a place in your valuable Journal? Appearances of the sky in a thunder storm

The whole surface of the heavens seemed covered with one unbroken mass of black pitchy cloud, in which the very vivid flashes of lightning, that almost instantaneously succeeded each other, showed no break; and from which but described.

but from inferior regions, they did not seem to issue. Over, or rather (to speak more properly) below this apparent surface, were spread light and flocky clouds, broken into large fleeces, white and apparently luminous throughout. I looked round that I might find whence proceeded the light that illuminated them; for they seemed as summer clouds in a bright sun, and as the clouds have appeared to day. I could perceive no light. Every other part of the hemisphere was totally dark.

White clouds apparently full of specks of light.

Looking fixedly at them, I fancied, that they seemed full of little dazzling and dancing specks of light, that sometimes shone as stars peeping through a misty cloud. Some of these increased gradually, and as gradually died away.

A luminous meteor of larger size.

One in particular became more and more distinctly visible, and increased in size, till it reached the brilliancy and magnitude of Venus, as she shines in a clear evening; and yet, there seemed no *body* of the light. At first I thought it must be some star; and it was with difficulty, that I renounced the idea. But such it could not have been: for, when these clouds had passed away, and when the intensity of the black masses above became diminished, when they seemed only concealed by a dark and thick haze, none of them became visible. To be certain that the motion, that, I fancied, I observed it to have, did not proceed from the motion of the cloud, and was not deceitfully produced to me, from the swimming and indistinctness of vision necessarily occasioned in my eyes by the quick and vivid flashes of lightning, that encircled the whole horizon, I brought the meteor to a bearing with the window frame, and by that means distinctly ascertained its movement, and, that it was with considerable rapidity. I observed it coast, if I may use the expression, round the edge of that mass in which it appeared, and, having again become stationary, diminish from its full splendor till it disappeared. Its duration must have been of minutes.

Another meteor of the same kind.

After a short interval I had an opportunity of observing another of these meteors in a similar cloud, though that at a considerable distance; and of which, though it behaved much as the former had done, I was not able so distinctly to mark the motion.

During

During this time, these, and during the space of half an hour at least, similar clouds, were full of these little luminous innumerable points, which, playing incessantly, gave them an appearance similar to that, which is exhibited in a clear sky by the galaxy. Several clouds resembling the milky way.

I have already said, that, when these had passed away, and the pitchy clouds also, which moved in the same direction, though not so rapidly, I could discern no stars whatever, and I took no small pains to spy out any, as they might have furnished me with a solution of the phenomenon. No star visible.

There was no flash of *lightning broke* from these clouds, but they emitted much light of a pale phosphoric colour, and such seemed the kind of light, that formed the body of the meteors. These clouds were at a very considerable distance beneath the higher stratum, and at no great elevation in the atmosphere, and though, after the interval of an hour, some of the most vivid flashes proceeded from this point in the heavens, yet do I conceive no connection between them and the clouds; as the latter had clean passed away, in an easterly direction, or with a few points north. Another thing I must mention, that as they tended to a greater distance, their brilliancy gradually diminished. No lightning from the luminous clouds.

Along with this account I have enclosed a sketch of the phenomena, wherein, though guilty of an anachronism, having in the same moment of time shown the two meteors, I may be pardoned, as I have tolerably preserved the relative bearings of distances, and, as nearly as in such a sketch I could, the respective forms of the masses of cloud. The course of the first I have marked by making it luminous throughout; and note, that its first appearance was to the eastward, which in this sketch being the left hand, the position will be best seen when the sketch is held in the position of observation, above the head. It is on a very proportionally small scale, as at least 35 degrees are included within it, and the spots noted for the meteors proportionally as large, as was the halo that seemed to surround them. I am afraid to have dilated too much; yet, not seeing where I can curtail the description, leave for you, Sir, to lop off any superfluous matter. Explanation of the plate.

No rain at the time.

There was no rain at the time of observation.

I am, Sir,

Hatton Garden, Your humble servant,

11 *Aug*. 1809. J. S.

P. S. As these meteors increased in size, they seemed to *descend*, and had much of that semblance, which the phantasmagorial spectres have, as they seem to approach the spectator.

II.

On Aerial Navigation. By Sir GEORGE CAYLEY, *Bart.*

SIR, *Brompton, Sept.* 6, 1809.

A man raising himself into the air by mechanical means.

I Observed in your Journal for last month, that a watch-maker at Vienna, of the name of Degen, has succeeded in raising himself in the air by mechanical means. I waited to receive your present number, in expectation of seeing some farther account of this experiment, before I commenced transcribing the following essay upon aerial navigation, from a number of memoranda which I have made at various times upon this subject. I am induced to request your publication of this essay, because I conceive, that, in stating the fundamental principles of this art, together with a considerable number of facts and practical observations, that have arisen in the course of much attention to this subject, I may be expediting the attainment of an object, that will in time be found of great importance to mankind; so much so, that a new æra in society will commence, from the moment that aerial navigation is familiarly realized.

The principles may be reduced to practice.

It appears to me, and I am more confirmed by the success of the ingenious Mr. Degen, that nothing more is necessary, in order to bring the following principles into common practical use, than the endeavours of skilful artificers, who may vary the means of execution, till those most convenient are attained.

Since

Since the days of Bishop Wilkins the scheme of flying by artificial wings has been much ridiculed; and indeed the idea of attaching wings to the arms of a man is ridiculous enough, as the pectoral muscles of a bird occupy more than two thirds of its whole muscular strength, whereas in man the muscles, that could operate upon wings thus attached, would probably not exceed one tenth of his whole mass. There is no proof that, weight for weight, a man is comparatively weaker than a bird; it is therefore probable, if he can be made to exert his whole strength advantageously upon a light surface similarly proportioned to his weight as that of the wing to the bird, that he would fly like the bird, and the ascent of Mr. Degen is a sufficient proof of the truth of this statement.

Artificial wings attached to the arms cannot answer.

But the whole strength of a man applied to a machine may.

The flight of a strong man by great muscular exertion, though a curious and interesting circumstance, in as much as it will probably be the first means of ascertaining this power, and supplying the basis whereon to improve it, would be of little use. I feel perfectly confident, however, that this noble art will soon be brought home to man's general convenience, and that we shall be able to transport ourselves and families, and their goods and chattels, more securely by air than by water, and with a velocity of from 20 to 100 miles per hour.

Confident expectation of its accomplishment.

To produce this effect, it is only necessary to have a first mover, which will generate more power in a given time, in proportion to its weight, than the animal system of muscles.

First mover requisite.

The consumption of coal in a Boulton and Watt's steam engine is only about $5\frac{1}{2}$ lbs. per hour for the power of one horse. The heat produced by the combustion of this portion of inflammable matter is the sole cause of the power generated; but it is applied through the intervention of a weight of water expanded into steam, and a still greater weight of cold water to condense it again. The engine itself likewise must be massy enough to resist the whole external pressure of the atmosphere, and therefore is not applicable to the purpose proposed. Steam engines have lately been made to operate by expansion only, and those might be constructed so as to be light enough for this purpose, provided

Steam engine.

provided the usual plan of a large boiler be given up, and the principle of injecting a proper charge of water into a mass of tubes, forming the cavity for the fire, be adopted in lieu of it. The strength of vessels to resist internal pressure being inversely as their diameters, very slight metallic tubes would be abundantly strong, whereas a large boiler must be of great substance to resist a strong pressure. The following estimate will show the probable weight of such an engine with its charge for one hour.

	lb.
The engine itself from 90 to	100
Weight of inflamed cinders in a cavity presenting about 4 feet surface of tube	25
Supply of coal for one hour	6
Water for ditto, allowing steam of one atmosphere to be $\frac{1}{1800}$ the specific gravity of water	32
	163

This statement merely an approximation.

I do not propose this statement in any other light than as a rude approximation to truth, for as the steam is operating under the disadvantage of atmospheric pressure, it must be raised to a higher temperature than in Messrs. Boulton and Watt's engine; and this will require more fuel; but if it take twice as much, still the engine would be sufficiently light, for it would be exerting a force equal to raising 550 lb. one foot high per second, which is equivalent to the labour of six men, whereas the whole weight does not much exceed that of one man.

Another first mover

It may seem superfluous to inquire farther relative to first movers for aerial navigation; but lightness is of so much value in this instance, that it is proper to notice the probability that exists of using the expansion of air by the sudden combustion of inflammable powders or fluids with great advantage. The French have lately shown the great power produced by igniting inflammable powders in close vessels; and several years ago an engine was made to work in this country in a similar manner, by the inflammation of spirit of tar. I am not acquainted with the name of the person who invented and obtained a patent for this engine, but from some minutes with which I was favoured by Mr. William

William Chapman, civil engineer in Newcastle, I find that 30 drops of the oil of tar raised eight hundred weight to the height of 22 inches; hence a one horse power may consume from 10 to 12 pounds per hour, and the engine itself need not exceed 50 pounds weight. I am informed by Mr. Chapman, that this engine was exhibited in a working state to Mr. Rennie, Mr. Edmund Cartwright, and several other gentlemen, capable of appreciating its powers; but that it was given up in consequence of the expense attending its consumption being about 8 times greater than that of a steam engine of the same force.

less weighty, but more expensive.

Probably a much cheaper engine of this sort might be produced by a gas-light apparatus, and by firing the inflammable air generated, with a due portion of common air, under a piston. Upon some of these principles it is perfectly clear, that force can be obtained by a much lighter apparatus than the muscles of animals or birds, and therefore in such proportion may aerial vehicles be loaded with inactive matter. Even the expansion steam engine doing the work of six men, and only weighing equal to one, will as readily raise five men into the air, as Mr. Degen can elevate himself by his own exertions; but by increasing the magnitude of the engine 10, 50, or 500 men may equally well be conveyed; and convenience alone, regulated by the strength and size of materials, will point out the limit for the size of vessels in aerial navigation.

Combustion of inflammable air.

Having rendered the accomplishment of this object probable upon the general view of the subject, I shall proceed to point out the principles of the art itself. For the sake of perspicuity I shall, in the first instance, analyze the most simple action of the wing in birds, although it necessarily supposes many previous steps. When large birds, that have a considerable extent of wing compared with their weight, have acquired their full velocity, it may frequently be observed, that they extend their wings, and without waving them, continue to skim for some time in a horizontal path. Fig. 1, Pl. V, represents a bird in this act.

Principles of the art.

Flight of a bird.

Let $a\ b$ be a section of the plane of both wings opposing the horizontal current of the air (created by its own motion) which may be represented by the line $c\ d$, and is the mea-

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provided the usual plan of a large boiler be given up, and the principle of injecting a proper charge of water into a mass of tubes, forming the cavity for the fire, be adopted in lieu of it. The strength of vessels to resist internal pressure being inversely as their diameters, very slight metallic tubes would be abundantly strong, whereas a large boiler must be of great substance to resist a strong pressure. The following estimate will show the probable weight of such an engine with its charge for one hour.

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sure of the velocity of the bird. The angle *b d c* can be increased at the will of the bird, and to preserve a perfectly horizontal path, without the wing being waved, must continually be increased in a complete ratio, (useless at present to enter into) till the motion is stopped altogether; but at one given time the position of the wings may be truly represented by the angle *b d c*. Draw *d e* perpendicular to the plane of the wings, produce the line *e d* as far as required, and from the point *e*, assumed at pleasure in the line *d e*, let fall *e f* perpendicular to *d f*. Then *d e* will represent the whole force of the air under the wing; which being resolved into the two forces *e f* and *f d*, the former represents the force that sustains the weight of the bird, the latter the retarding force by which the velocity of the motion, producing the current *c d*, will continually be diminished. *e f* is always a known quantity, being equal to the weight of the bird, and hence *f d* is also known, as it will always bear the same proportion to the weight of the bird, as the sine of the angle *b d e* bears to its cosine the angles *d e f*, and *b d c*, being equal. In addition to the retarding force thus received is the direct resistance, which the bulk of the bird opposes to the current. This is a matter to be entered into separately from the principle now under consideration; and for the present may be wholly neglected, under the supposition of its being balanced by a force precisely equal and opposite to itself.

Some practical observations.

Before it is possible to apply this basis of the principle of flying in birds to the purposes of aerial navigation, it will be necessary to encumber it with a few practical observations.

Problem.

The whole problem is confined within these limits, viz. To make a surface support a given weight by the application of power to the resistance of air. Magnitude is the first question respecting the surface.

Experiments on the resistance of the air.

Many experiments have been made upon the direct resistance of air, by Mr. Robins, Mr. Rouse, Mr. Edgeworth, Mr. Smeaton, and others. The result of Mr. Smeaton's experiments and observations was, that a surface of a square foot met with a resistance of one pound, when it travelled perpendicularly to itself through air at a velocity of 21 feet per second. I have tried many experiments upon a large scale to ascer-

tain

tain this point. The instrument was similar to that used by Mr. Robins, but the surface used was larger, being an exact square foot, moving round upon an arm about five feet long, and turned by weights over a pulley. The time was measured by a stop watch, and the distance travelled over in each experiment was 600 feet. I shall for the present only give the result of many carefully repeated experiments, which is, that a velocity of 11·538 feet per second generated a resistance of 4 ounces; and that a velocity of 17·16 feet per second gave 8 ounces resistance. This delicate instrument would have been strained by the additional weight necessary to have tried the velocity generating a pressure of one pound per square foot; but if the resistance be taken to vary as the square of the velocity, the former will give the velocity necessary for this purpose at 23·1 feet, the latter 24·28 per second. I shall therefore take 23·6 feet as somewhat approaching the truth.

Having ascertained this point, had our tables of angular resistance been complete, the size of the surface necessary for any given weight would easily have been determined. Theory, which gives the resistance of a surface opposed to the same current in different angles, to be as the squares of the sine of the angle of incidence, is of no use in this case; as it appears from the experiments of the French Academy, that in acute angles, the resistance varies much more nearly in the direct ratio of the sines, than as the squares of the sines of the angles of incidence. The flight of birds will prove to an attentive observer, that, with a concave wing apparently parallel to the horizontal path of the bird, the same support, and of course resistance, is obtained. And hence I am inclined to suspect, that, under extremely acute angles, with concave surfaces, the resistance is nearly similar in them all. I conceive the operation may be of a different nature from what takes place in larger angles, and may partake more of the principle of pressure exhibited in the instrument known by the name of the hydrostatic paradox, a slender filament of the current is constantly received under the anterior edge of the surface, and directed upward into the cavity, by the filament above it, in being obliged to mount along the convexity of the surface, having created

Our tables of angular resistance imperfect.

Concave wing of a bird.

created a slight vacuity immediately behind the point of separation. The fluid accumulated thus within the cavity has to make its escape at the posterior edge of the surface, where it is directed considerably downward; and therefore has to overcome and displace a portion of the direct current passing with its full velocity immediately below it; hence whatever elasticity this effort requires operates upon the whole concavity of the surface, excepting a small portion of the anterior edge. This may or may not be the true theory, but it appears to me to be the most probable account of a phenomenon, which the flight of birds proves to exist.

Experiments of the French Academy.

Six degrees was the most acute angle, the resistance of which was determined by the valuable experiments of the French Academy; and it gave $\frac{4}{11}$ of the resistance, which the same surface would have received from the same current when perpendicular to itself. Hence then a superficial foot, forming an angle of six degrees with the horizon, would, if carried forward horizontally (as a bird in the act of skimming) with a velocity of 23·6 feet per second, receive a pressure of $\frac{4}{10}$ of a pound perpendicular to itself. And, if we allow the resistance to increase as the square of the velocity, at 27·3 feet per second it would receive a pressure of one pound. I have weighed and measured the surface of a great many birds, but at present shall select the common rook (corvus frugilegus) because its surface and weight are as nearly as possible in the ratio of a superficial foot to a pound. The flight of this bird, during any part of which they can skim at pleasure, is (from an average of many observations) about 34·5 feet per second. The concavity of the wing may account for the greater resistance here received, than the experiments upon plain surfaces would indicate. I am convinced, that the angle made use of in the crow's wing is much more acute than six degrees: but in the observations, that will be grounded upon these data, I may safely state, that every foot of such curved surface, as will be used in aerial navigation, will receive a resistance of one pound, perpendicular to itself, when carried through the air in an angle of six degrees with the

Flight of the rook.

the line of its path, at a velocity of about 34 or 35 feet per second.

Let *a b*, fig. 2, represent such a surface or sail made of thin cloth, and containing about 200 square feet (if of a square form the side will be a little more than 14 feet); and the whole of a firm texture. Let the weight of the man and the machine be 200 pounds. Then if a current of wind blew in the direction *c d*, with a velocity of 35 feet per second, at the same time that a cord represented by *c d* would sustain a tension of 21 pounds, the machine would be suspended in the air, or at least be within a few ounces of it (falling short of such support only in the ratio of the sine of the angle of 94 degrees compared with radius; to balance which defect, suppose a little ballast to be thrown out) for the line *d e* represents a force of 200 pounds, which, as before, being resolved into *d f* and *f e*, the former will represent the resistance in the direction of the current, and the latter that which sustains the weight of the machine. It is perfectly indifferent whether the wind blow against the plane, or the plane be driven with an equal velocity against the air. Hence, if this machine were pulled along by a cord *c d*, with a tension of about 21 pounds, at a velocity of 35 feet per second, it would be suspended in a horizontal path; and if in lieu of this cord any other propelling power were generated in this direction, with a like intensity, a similar effect would be produced. If therefore the waft of surfaces advantageously moved, by any force generated within the machine, took place to the extent required, aerial navigation would be accomplished. As the acuteness of the angle between the plane and current increases, the propelling power required is less and less. The principle is similar to that of the inclined plane, in which theoretically one pound may be made to sustain all but an infinite quantity; for in this case, if the magnitude of the surface be increased ad infinitum, the angle with the current may be diminished, and consequently the propelling force, in the same ratio. In practice, the extra resistance of the car and other parts of the machine, which consume a considerable portion of power, will regulate the limits to which this principle, which is the true basis of aerial navigation, can be carried; and the

Applied to the theory of aerial navigation.

the perfect ease with which some birds are suspended in long horizontal flights, without one waft of their wings, encourages the idea, that a slight power only is necessary.

Farther observations promised.

As there are many other considerations relative to the practical introduction of this machine, which would occupy too much space for any one number of your valuable Journal, I propose, with your approbation, to furnish these in your subsequent numbers; taking this opportunity to observe, that perfect steadiness, safety, and steerage, I have long since accomplished upon a considerable scale of magnitude; and that I am engaged in making some farther experiments upon a machine I constructed last summer, large enough for aerial navigation, but which I have not had an opportunity to try the effect of, excepting as to its proper balance and security. It was very beautiful to see this noble white *bird* sail majestically from the top of a hill to any given point of the plane below it, according to the set of its rudder, merely by its own weight, descending in an angle of about 18 degrees with the horizon. The exertions of an individual, with other avocations, are extremely inadequate to the progress, which this valuable subject requires. Every man acquainted with experiments upon a large scale well knows how leisurely fact follows theory, if ever so well founded. I do therefore hope, that what I have said, and have still to offer, will induce others to give their attention to this subject; and that England may not be backward in rivalling the continent in a more worthy contest than that of arms.

Experiments have been made on a tolerably large scale.

Simple machine rising in the air by mechanical means.

As it may be an amusement to some of your readers to see a machine rise in the air by mechanical means, I will conclude my present communication by describing an instrument of this kind, which any one can construct at the expense of ten minutes labour.

a and *b*, fig. 3, are two corks, into each of which are inserted four wing feathers from any bird, so as to be slightly inclined like the sails of a windmill, but in opposite directions in each set. A round shaft is fixed in the cork *a*, which ends in a sharp point. At the upper part of the cork *b* is fixed a whalebone bow, having a small pivot hole in its centre, to receive the point of the shaft. The bow is then

then to be strung equally on each side to the upper portion of the shaft, and the little machine is completed. Wind up the string by turning the flyers different ways, so that the spring of the bow may unwind them with their anterior edges ascending; then place the cork with the bow attached to it upon a table, and with a finger on the upper cork press strong enough to prevent the string from unwinding, and taking it away suddenly, the instrument will rise to the ceiling. This was the first experiment I made upon this subject in the year 1796. If in lieu of these small feathers large planes, containing together 200 square feet, were similarly placed, or in any other more convenient position, and were turned by a man, or first mover of adequate power, a similar effect would be the consequence, and for the mere purpose of ascent this is perhaps the best apparatus; but speed is the great object of this invention, and this requires a different structure.

P. S. In lieu of applying the continued action of the inclined plane by means of the rotative motion of flyers, the same principle may be made use of by the alternate motion of surfaces backward and forward; and although the scanty description hitherto published of Mr. Degen's apparatus will scarcely justify any conclusion upon the subject; yet as the principle above described must be the basis of every engine for aerial navigation by mechanical means, I conceive, that the method adopted by him has been nearly as follows. Let A and B, fig. 4, be two surfaces or parachutes, supported upon the long shafts C and D, which are fixed to the ends of the connecting beam E, by hinges. At E, let there be a convenient seat for the aeronaut, and before him a cross bar turning upon a pivot in its centre, which being connected with the shafts of the parachutes by the rods F and G, will enable him to work them alternately backward and forward, as represented by the dotted lines. If the upright shafts be elastic, or have a hinge to give way a little near their tops, the weight and resistance of the parachutes will incline them so, as to make a small angle with the direction of their motion, and hence the machine rises. A slight

Method supposed to be employed by Mr. Degen.

A slight heeling of the parachutes toward one side, or an alteration in the position of the weight, may enable the aeronaut to steer such an apparatus tolerably well; but many better constructions may be formed, for combining the requisites of speed, convenience, and steerage. It is a great point gained, when the first experiments demonstrate the practicability of an art; and Mr. Degen, by whatever means he has effected this purpose, deserves much credit for his ingenuity.

III.

On Electro-Chemical Experiments. *By* Mr. G. J. Singer.

Mr. Davy's experiments not generally repeated.

THE important increase of chemical knowledge, which has attended the recent successful application of electrical powers to the improvement of analysis, cannot be well appreciated without a repetition of the experiments; but this has not been hitherto by any means generally attempted, although a considerable time has elapsed since the publication of Mr. Davy's first researches. The consequences of this delay are most prejudicial to the real interests of science, as the observations, which have been published in this country on the labours of that assiduous chemist, have been consequently supported by nothing but hypothetical reasoning, or loose conjecture; and have been therefore rather calculated to impede than promote the progress of discovery.

Supposed to require great powers,

From the want of a popular exposition of the facts already obtained, and of the mode of conducting similar researches, an idea has prevailed very generally, that much difficulty attends the repetition of the new experiments; and that in most instances the aid of a powerful Voltaic battery is required. This I have found is not by any means the case, most of the experiments may be performed with very moderate powers, when the requisite precautions are observed. As a want of attention to these circumstances has been in most instances the cause of failure, many experimenters

but these not necessary with due precautions.

rimenters having employed very considerable power without effect, it may be useful to describe that peculiar attention, which has been proved by experience most calculated to ensure success.

From a series of experiments made for the express purpose of ascertaining the best mode of employing the Voltaic battery, which I shall at a future period lay before the public, I have found, that the most usual is by far the worst that can be adopted when the instrument is intended for experiments of decomposition; this operation requiring the continued action of a power of nearly uniform intensity, a circumstance that rarely occurs in the ordinary mode of charging. By far the greater number of experimenters estimate the acting power of their instruments by the quantity of wire a given number of plates will fuse; and consider them most advantageously excited, when they fuse the greatest length. To attain this object, if the battery is not of great extent, a strong acid mixture is employed. This produces violent action for a short time, but which gradually decreases, and in a very limited period ceases altogether. The power thus excited, which I call the wire-melting power, is by no means desirable but for the performance of brilliant experiments; the most extensive and interesting class of chemical compounds being either partial conductors, or nonconductors, on which this action will be found less efficacious than a more moderate intensity. Best mode of applying the battery for decompositions.

The most active wire-melting power I have yet excited was by a mixture of one part strong nitrous acid, and ten parts water, with the addition of a very minute portion of muriatic acid; but from some observations I have recently made, I am induced to believe this mixture should never be employed in an apparatus used for general experiments. Most effective power for melting wire.

Three similar batteries were charged with equal proportions of the different acids, that charged with nitric acid fused the greatest portion of wire, that with sulphuric acid the next in quantity, and that with muriatic acid the least: their action on imperfect conductors was nearly similar. At the end of 14 hours they were again tried; the battery charged with nitric acid had completely lost its wire-melting power, as had also that charged with sulphuric acid, and Comparative powers of the three mineral acids.

and neither of them exerted more than a feeble action on imperfect conductors; but the battery charged with muriatic acid, to my great surprise, melted two thirds of the length of wire it had melted in the first instance, and appeared to decompose water with equal rapidity. The three batteries were suffered to remain. At the end of two days the first two had totally lost their acting power, but the last still melted one third of the original length of wire, and continued to melt wire till the fourth day. Its action on imperfect conductors was still evident after six days, when the experiments were discontinued. In all these experiments the plates were lifted out of the acid during the intervals. It was a long time before I could procure an equal continuance of action from the batteries which had first been employed with sulphuric and nitric acids; their powers appeared to be in a measure exhausted, and their action was comparatively feeble, but by persevering in the use of the muriatic acid, I at length brought them to an equal uniformity of action.

Proportion of acid to water.

The quality of the acid to be preferred was clearly proved by these experiments; but it was still necessary to determine the requisite proportion in which it should be employed. For the decomposition of potash this circumstance requires particular attention, as the strength of the mixture should vary with the extent of the apparatus. For any power not exceeding 200 plates of 4 inches, the proportions may be from 8 to 10 ounces of muriatic acid for every gallon of water. But if 300, 400, or any greater number be employed, or their size increased, the quantity of acid should be proportionally diminished, or the heat produced will destroy the metallic globules at the moment of their production.

Naphtha decomposed faster than potash.

In the first experiments I made on this substance, the operation was performed under naphtha, but in this way I found the naphtha was decomposed more rapidly than the potash, and the quantity of carbon liberated embarrassed the result. I now always operate in the open air, and use conductors of silver, which I find preferable to platina. A flat silver plate or spoon is connected with the negative (the copper) surface of the battery. On this I place a small

Silver preferable to platina.

piece

piece of potash, not moistened, and make a communication to its upper surface by a silver wire from the positive surface. In the space of about a minute, or less, metallic globules appear near the negative surfaces. (Some of these inflame, but for the most part they become covered with a crust of potash, which defends them from the farther action of the air). As soon as these globules appear, they should be attentively watched, and the instant they cease to grow larger, removed on the point of a silver knife, and plunged into a watch glass filled with naphtha; or if the experiment is intended merely to show their production, they may be immersed in water as they are removed, when each globule will produce a vivid inflammation.

When the circumstances I have mentioned are strictly attended to, the phenomena are usually as I have now described; but it sometimes happens, that no globules appear: in this case, the communication should be still continued for five or ten minutes, when the potash being taken from the spoon, the side which was in contact with it will be found studded with metallic globules, which may be removed as before directed. Sometimes no globules appear.

By following this mode of operation, I soon found, that a much lower power than I had at all suspected was adequate to the production of a distinct result. With a glass-partitioned battery of 50 pairs of 4 inch plates, the metallic base was produced in sufficient quantity to evince its principal properties with considerable ease. This result induced me to try, if it might not be effected by a lower power; and I have actually found, that, by carefully conducting the process, very distinct globules may be produced by a battery of 50 plates, of only 3 inches diameter, which battery has also the disadvantage of having been much corroded by former operations. The metallization of the alkaline earths and of ammonia by amalgamation with mercury may be also effected by the last described battery; so that even this small power, properly directed, is sufficient to afford a satisfactory illustration of the principal phenomena for the individual gratification of the chemical inquirer. The decomposition effected by a low power.

The transfer of acid and alkali may be very readily shown by an apparatus of this size. The most striking mode of illustrating Transfer of acid and alkali.

illustrating this action is the following. To a pint of water add two or three drops of sulphuric acid, and infuse in it as many minced leaves of red cabbage as it will cover. In a day or two, the water will be tinged of a fine red colour. Decant the liquor, and preserve it in a bottle closely stopped. When the experiment is to be performed, a portion of the red tincture is to be neutralized, by carefully adding a few drops of ammonia, till it assumes a blue colour. Two watch glasses connected by a moistened fibre of cotton, or bibulous paper, are to be filled with this red fluid, and placed in the circuit by connecting one of them with the negative, and the other with the positive wire of the battery. In a short time, this *alkali* will be attracted by the *negative* wire, and the fluid which surrounds it will consequently assume a *green* colour; while the *positive* wire, attracting the *acid*, converts the fluid which surrounds it to a fine *red*. In about half an hour the transfer will be complete, the fluid in the negative cup being of a beautiful green, and that in the positive of a bright red. If the situation of the wires are now reversed, so that the cup which was positive may become negative, and that which was negative assume a positive state, the colours will again change; the *green* will first become *blue*, and then *red*; and the *red*, after first returning to its original *blue*, will become *green*. This alternate transfer, which may be several times repeated with one charge, I have frequently produced by a trough of only 30 pairs of plates of 2 inches square.

Apology for minuteness.

I have been rather particular (perhaps it may be thought too much so) in the account I have given of these experiments, but I do not write for the instruction of the experienced chemist, and I am inclined to think the tyro will thank me for having attempted to diminish the difficulties of his pursuit. I am at present engaged in the extension of these experiments, and in the prosecution of others connected with the same inquiry, and intend shortly to publish an elementary work on the subject, in which I shall attempt the arrangement of a systematic series of familiar experiments, in illustration of the several phenomena.

3, *Princes Street, Cavendish Square,*
Sept. 21, 1809.

IV.

IV.

Extract of a Letter Mr. J. B. Van Mons *to Mr.* Sue, *on different subjects relating to Galvanism and Electricity.*

On repeating the experiments of Pacchiani on the pretended decomposition of water into muriatic acid, I satisfied myself, that the galvanic current is a conductor of heat likewise; by interposing between the plates pieces of pasteboard moistened with oximuriatic acid, nitric acid, or oximuriates, nitrates, &c.; which, during their action on the metal of the pile, setting caloric free, the fluid experimented on, or through which the current passes, is heated considerably.

Galvanic current a conductor of heat.

The substances transferred by the action of the pile are partly resolved into their ultimate elements. This resolution does not appear to be effected by means of chemical affinity, or an attraction of composition, but in consequence of the different degree in which these elements are conductible by the galvanic current; which conductibility is measured by the rapidity of the transmission, and the distance beyond the point of the two currents, the positive and the negative, at which it takes place. So that we cannot measure the degree of chemical affinity of a body by that of its decomposability by the pile; but a substance not being decomposable by this apparatus affords a strong presumption, that it is not decomposable by other substances. Substances have not been sufficiently subjected to the immediate action of the pile, or to the effects of the plates on the substance used for impregnating the fluid, with a view to effect their decomposition. Few decomposable substances would escape being decomposed: even the carbonates are then resolved into their ultimate elements, or set free carbon. When I say carbonates, I speak only of the carbonate of ammonia, for the others appear only to have their base separated from the acid. It would seem as if the activity of the pile were satisfied with the first effect of decomposition it exerts, and that the decomposition of the carbonic acid in the carbonate of ammonia is the effect of a secondary

Substances carried away by its current, and their elements separated by being acted on with more or less velocity.

This not connected with chemical affinity.

Almost all substances decomposable by the pile.

Carbonate of ammonia.

dary action, determined by the equal decomposability of the acid and the base; the free acid being too fugacious to be reached by the action of the pile. The facility with which the galvanic current vaporizes in some measure the most fixed substances, as earths, the fixed alkalis, metals, &c., and causes them to circulate with it, is astonishing. The current must intimately dissolve, or strongly divide, the substances it transfers; since after this transference these substances crystallize, as in the beautiful experiments of Brugnatelli, and in experiments similar to those of that illustrious Italian chemist and natural philosopher, which I have performed with earths and alkalis. When the decomposition of water is effected without a perceptible separation of gas, as in the last experiment mentioned by Pacchiani and others, the two gasses follow the galvanic current along the wire with different velocities, and separate only in succession on the body of the pile itself. Most of the substances, that are decomposed by the immediate action of the plates, do not quit the current till their return to that plate or element of the pile, from which they set out; and there they are deposited, recombined, or extricated. To this effect is owing the long continued activity of piles, in which the interposed substance is of a nature to be transferred without being dissipated. I have subjected to the immediate action of the plates all known substances, both solid and liquid, and aeriform in a state of composition; and I have obtained results as extraordinary with respect to the influence of the pile on these substances, as of these substances on the action of the pile. They form a body of facts, from which I have yet deduced but few consequences: but the first moment of leisure I have from my extensive occupations, I shall arrange them, and lay them before the Institute.

The galvanic current easily vapotizes substances.

Its effect.

Decomposition of water.

Long continued activity of some piles.

Numerous experiments of the author.

I cannot conceive how some persons persist in admitting the decomposition of water into muriatic acid by the pile, while the experiments I have inserted in my Chemical and Physical Journal evidently demonstrate, that, in all cases where this acid is obtained, it comes from a muriate, with which the interposed pasteboards are moistened. This is so true, that, if a solution of some other salt with an indecomposable

Water not decomposed into muriatic acid.

Source of the acid.

Other acids produced.

posable acid, as a borate or a fluate, be used instead of a muriate for moistening the disks, the acid transferred corresponds with that of the salt employed. The salts with decomposable acids, as nitrates, sulphates, phosphates, acetates, &c., are in part decomposed by the combined effect of the action of the pile, and of the attraction of the metallic plates for their oxigen. This action appears to strengthen the action of the pile, at the moment when it takes place, if the moistening salt be an oximuriate, or a nitrate, the oxigen of which gives out caloric on entering into a more solid combination. But if it be a salt, the acid of which has what Brugnatelli calls an *oxigenizable* radical, the energy is not very different. This appears to prove, that it is owing to the caloric separated from the oxigen, which, on entering into the galvanic current, is partly transformed into electric fluid. However, I have fancied I have observed a difference of effect here, that is somewhat singular, and shows the great influence of disposing affinity: this is, that the energy of the pile, in respect to what is called its charge, is increased only when the substance subjected to its action is to be decomposed, and that it is almost the same as with other salts, when it is to be composed. We know, that in the first case, namely when principles are to be separated by the direct action of the electric fluid, this fluid, without altering its nature, enters into combination with the principle separated, which it converts into a gas: while in the second case, or when it determines the union of principles that have been separated, it is transformed into heat, simply to raise the temperature: hence the caloric separated from the oxigen will circulate either as electric fluid, or as heat, according as the effect to be produced disposes it to assume or retain the one or other of these modifications. It is not however a determining attraction, that produces the elevation of temperature, of which I spoke in the commencement of my letter. The chemical action then does not heighten the activity of the pile, except as far as this action is accompanied with an evolution of heat.

But no decomposable acid.

The evolution of oxigen strengthens the action of the pile.

Influence of disposing affinity.

Chemical action increases that of the pile only when heat is evolved.

I have said, that the circumstances I just mentioned increase the energy of the pile momentarily: its activity afterward diminishes, but less by its exhaustion than by the alteration

The energy of the pile diminished by the

alteration of the plates.

alteration the plates experience. I have noted down a multitude of facts respecting the circumstances that increase or diminish the activity of the pile, which would be sufficient perhaps to form the basis of a theory in this respect.

Decomposition of the salt.

To return to my subject, from which I have wandered far. Not only does the acid transferred in Pacchiani's experiments correspond with that of the salt in the solution with which the pasteboards are moistened; but the base, that fixes the acid, is the same as that of the salt. If this base be a fixed alkali, or an earth, it passes without being decomposed: but if it be a metal, this is partly reduced by the same cause as decomposes the acids with known radicals, and is more difficult to be transferred. It is a singular effect of the galvanic current, to transfer substances so combined with it, or rather with such great rapidity, that it traverses with them substances for which they have the greatest affinity, without leaving them power, or rather time to enter into combination with them. It is nearly as Ducarla supposed in his excellent paper on *perfect fire*, that light traverses the air, or any other diaphanous medium, without heating it, because it does not remain long enough in a place to exert its calorific powers. The figure is sufficiently just, but the cause assigned is false: this action of traversing diminishes, but does not cease, when the current is transmitted through an interposed substance, that is but a semiconductor. What is farther singular is, that the same current transfers with more rapidity and facility substances eminently insulating, as the sulphur of alkaline sulphurets, the resins of ethereous and alcoholic tinctures, &c*. I first observed and made known, not only that the alkali or earth of a muriate employed in the moistening solution will traverse muriatic acid, or any other acid interposed to the current; or that these acids will equally traverse an earthy or alkaline solution, interposed in the same manner, without entering into combination with it; but that the two principles of the salt employed meet in the course of the circulation without uniting,

Why light does not heat the air.

The current transfers non-conductors with most rapidity.

Substances that have the strongest affinity prevented from exerting it by the force of the current,

* If the current act mechanically, and not chemically, it is not singular, but natural, that it should exert a greater impelling power on the particles that resist its course, than on those over which it glides easily. C.

before

before they return to the point of departure on the decomposing plate; and this after having travelled together, though no doubt with different velocities, along the negative wire. For this effect the wires must not be interrupted by substances, that the current unites, or separates; for in this case it quits, at least in part, the substance it transports, either to enter into combination with one of the principles of the substance it would decompose, or to transform itself into heat, and raise the temperature of the principles it would dispose to unite.

If there be no connecting substance, that the current unites or separates.

I have said, that the two principles of the substance employed in the moistening solution meet without uniting. Not that I mean to assert they proceed in opposite directions; for nothing would be more absurd than to suppose, that the negative of the pile, which is a nonentity, a negation of quality, a privation of power, can convey a substance; or even that a body can slide along a wire, because it is deprived of the electric fluid: but the two principles are carried along by the fluid, which passes along the positive wire to return to the plate from which it set out, but carried with unequal velocities. This difference of velocity would not be sufficient to prevent these principles from meeting with each other, unless the separation was made in a single instant, or all at once: but as this separation takes place in an infinite number of instants, and almost without interval, it is not possible that the acid for example, which is conveyed first, or with the greatest velocity, since it arrives first at the interposed tube, should not gain upon the alkali separated immediately before it, and pass by it, or unite with it, if it could unite with it. The current then passes along the two wires, and in the same direction, as if they were a single wire; and the negative state advances on the wire termed negative only in proportion as the positive state withdraws itself from the pile to advance on the positive wire. These wires then are to be considered only as prolongations of the two opposite states of the apparatus.

The elements separated do not properly meet, but one passes by the other.

The acid passes by the alkali.

To return to the experiments of Pacchiani. Do you not perceive, that they, who reject his conclusions, yet admit the production of an acid, are chargeable with a forced explanation, when they ascribe the origin of this acid to the substance

The acid not derived from the interposed pieces,

or the alkali from the glass.

stance of the interposed pieces, and that of the alkali to the decomposition of the glass; though the experiment, however long continued, does not cease to furnish the same acid and alkali, provided the activity of the pile be not diminished by the oxidation of one of the plates; which, as I have already said, appears to be the determining agent of the decomposition of the impregnating salt; and though the glass, into which the acid is received, loses nothing of its polish? Besides, the production of an acid and alkali equally takes place, if we use wires or slips of metal for conductors, and a metal vessel for a receiver: and it does not take place at all, if we operate with pure water instead of muriates, whether we use animal or metallic substances to conduct the current, and receive the acid in glass or in metal.

Both are produced without these,

but not without a saline solution.

Girtanner's hypothesis, that hidrogen is evolved from muriatic acid on dissolving a metal.

Since the pretended decomposition of water into muriatic acid by the pile, the opinion of Girtanner has been revived, which I controverted in the 1st volume of the Memoirs of the Institute, and according to which the hidrogen, evolved in solutions of metals by the muriatic acid, is said to come from the acid, and not from the water. But if, according to the assertion of Pacchiani, "water be superoxigenated muriatic acid, or oximuriatic acid with the addition of a fresh quantity of oxigen," still it would be this fluid, and not the acid, that must yield the oxigen; for, according to every law of affinity, it is the last portion of a principle combined, or its supercombined part, that separates first, being retained by a less powerful attraction.

Pacchiani's, that water is superoxigenized muriatic acid.

The pile should not be insulated.

In galvanic experiments great care is always taken, to insulate the pile, as if this apparatus, composed of a negative and a positive part, or a succession of surfaces alternately charged and discharged, were not, like every other electrified substance, the natural conservator of its own charge. Besides, the pile is not only incapable of losing any part of its fluid by communication with the ground, but its charge, which arises spontaneously, or without foreign accumulation, does not destroy itself on forming a communication between its surfaces, or opposite poles. In the charge of a body not insulated by itself, as a conductor, it must be insulated from the Earth, from which the fluid is extracted, and to which it has a tendency to return. In that of a bottle, plate of glass,

or

or other substance, the insulation is made by the substance itself; and, to preserve it, the charged part must always be kept from any communication with the ground. But the pile has nothing to do with the ground: it can neither take any thing from it, nor give any thing to it, its charge being fixed by its discharge, and *vice versa*. It is only in cases where the pile would be exhausted by furnishing its fluid to decompose a substance, as to convert the two elements of water into gas, &c.; or to compose one, by raising the temperature; or lastly to make an explosion through air, and for this purpose transform itself into light and heat; that the pile can reestablish its charge, or rather its natural state, at the expense of the ground. In this case its not being insulated would be advantageous, rather than otherwise: and in fact, as I have already made public, an uninsulated pile retains its energy much longer than another that is insulated. I shall not say however in what way I conceive this restoration can take place in an apparatus, which for its revival has the resource of decompositions of heat [*décompositions calo-riques*] by its plates.

Uninsulated pile retains its energy longest.

I cannot conceive how in France, the seat of an academy that admits only strict deductions, the dualist electric theory, or that of two fluids, is still maintained in preference; and this in a more extravagant sense than that of Symmer himself, who was the author of it. In the hypothesis of two opposite electricities, what in fact are two fluids of the same nature, that repel each other? What are opposite powers (which naturally must endeavour to destroy each other, without being much concerned about the substances, that are interposed between them), which, applied to the particles of bodies, tend with the greatest energy to disunite their elements? as if the electric fluid, which in the case of its being set in motion, or in its operation, does not adhere to bodies, unless it enters into combination with them, but glides over their surfaces with the rapidity of lightning; and which in its state of rest keeps itself on the surface of the bodies to which it adheres; and diffuses itself in zones, or strata of opposite states, which mutually enchain or destroy the activity of each other, in bodies to which it does not adhere; could exercise the least action in these cases. This supposed

Hypothesis of two electric fluids.

conflict

conflict is a little like that formerly admitted between alkalis and acids, on which their effervescence was imagined to depend. For how can we suppose an action without the direct intervention of the acting substance, and without admitting in this substance an affinity, if not of combination, at least disposing, or of indirect or remote combination with one of the principles of the substance on which it acts; simply to ascribe its decomposition, and that in its most intimate connexions, to a conflict between two opposite powers? a conflict that manifests itself in no way, that all the phenomena contradict, that resembles no other action known, that want of reflection alone could suggest, and that is founded only on a big word without meaning, on a fine phrase, under cover of which so many falsities have passed without examination from age to age. It is not thus in nature, where substances and powers of the same quality attract each other, are confounded together, and concur to the same end; and where substances and powers of opposite qualities either attract each other to combine peaceably, or are indifferent to each other. If the action ascribed to the electric fluid could exist, if a passive action without object and without end were not a contradiction in terms, it would be to reduce the influence of this powerful fluid to something very trifling, it would be to make it perform a very inferior part.

Substances or powers of the same properties coalesce; those of opposite qualities attract or are indifferent to each other.

The following is nearly the true mode of action of the electric fluid. I repeat what I have said elsewhere in giving it, but when we perceive the wisest heads going astray, the principles, that must cure them of their errour, cannot be too frequently brought forward.

The author's theory.

When the electric fluid, either of the common machine or of the pile, decomposes substances, one or more principles of which are bases of permanent gasses, it enters into combination with these bases, which it convertes into gas in the same manner as light does. Thus it decomposes water, nitric acid, ammonia, metallic oxides, &c. It acts on this occasion as a true chemical power, since it destroys combinations that exist by chemical affinity. In these decompositions it enjoys an advantage over light, being in the state in which permanent gasses appear to contain caloric, and into

Electricity combines with an element and forms a gas, more directly than light:

to which light must transform itself to be able to unite with their bases. The nature of this power is farther confirmed by that which is requisite to disengage permanent aeriform combinations.

In the compositions and decompositions of substances by one another, it acts sometimes by concurrent at others by disposing affinity. In the first case it unites with one of the principles of the substance, which principle must always be a base of a permanent gas; and in the second it merely favours the action of a second principle by acting as heat. Thus it occasions the oxidation of metals by water, by converting the hidrogen into gas, &c.; and calls into action the affinities of all substances in the same manner as caloric, by diminishing their attraction of cohesion, or dissolving them.

assists in the composition or decomposition of substances by a concurrent, or by a disposing affinity:

It is as caloric too, that it effects direct combinations, such as those of the bases of water, &c.

as heat effects direct combinations;

It decomposes, and that particularly in the current of the pile, certain insulated substances, on the principles of which it exerts no attraction but that of conveying with different velocities. This action, I confess, is singular, and supposes in the fluid a great attraction of adhesion to the principles of these substances, as it can make them follow the rapidity of its translatory movement. The decompositions of salts with indecomposable acids and bases particularly take place in this way. I have remarked, that this action is scarcely at all exerted by the pile, when the communication is established by means of thick wires, or other substances with an extensive surface.

decomposes substances by carrying along their elements with different velocities:

To effect combinations that require a red heat, as most combinations, &c., the fluid should be made to pass through a stratum of air, in order that it may concentrate itself sufficiently to force this passage through a medium, which refuses it as a nonconductor, and thus establish itself in the state of light and heat. Thus it is in quality of these two modifications of caloric, that it produces inflammations, &c. The presence of light appears to be necessary in these operations, to impart the luminous constitution to the electriform caloric, which converts the oxigen into gas.

effects some combinations by concentrating itself into the state of light and heat:

It appears, that, every time the electric fluid no longer finds occasion to exercise its attraction of adhesion and expansion

and in part assumes the state of light, unless

when its tendency to expand is so restrained by a nonconducting medium, as to exercise the attraction of adhesion.

pansion on bodies, as in gliding along a nonconductor, in traversing a nonconducting medium, in a vacuum, &c., it assumes in part the state of light. This attraction of adhesion is always the result of the tendency of the fluid to expand, and of the restraining action of the air.

Objections to the hypothesis of two fluids,

To return to the theory of electricity, beside that Symmer's, namely the hypothesis of two fluids, or rather of three, the resinous, vitreous, and both combined, is more complicated; and that we perceive no difference between the two fluids in respect to their action, for the operator can only judge by the comparison and opposition of these fluids, whether he have excited the one or the other, a circumstance which has nothing analogous to it in natural philosophy;

which is shown to be fundamentally wrong.

this hypothesis is fundamentally overturned by the fact, that glass and resin excite both the electricities indifferently, accordingly as they are rubbed with substances more or less conducting than themselves.

Franklin's theory.

The Franklinian theory on the contrary is simple, adapted to the nature of things, and agrees with the received doctrine of the exercise of material powers; its attractions and repulsions are effects of the same cause, and the natural result of an elastic fluid, eager for an equilibrium, the opposite states of which, in order to find this equilibrium, rush from the place where it is plus toward that where it is minus, carrying with it the substances on which they are excited, if these substances be light, and freely suspended, or movable.

Always acts by attraction,

It is never repulsion therefore, but always attraction, that causes the motion of these substances, which in the ball electrometer separate by the communication of positive electricity, for the purpose of depositing the excess of their fluid on the sides of the glass, in which this excess has excited a state of defect; and by the communication of negative electricity, to repair their defect of fluid from the same sides of the glass, in the substance of which this defect has excited a state of excess. The same thing takes place in the air: besides, the separation in both cases is necessary for the formation of the opposite atmosphere, without which no electrical charge can be established or retained, and no discharge take place.

even where this appears to alternate with repulsion,

In the other cases of alternate attraction and repulsion, as in that of an insulated ball immersed in the atmosphere of an electrified

electrified substance, this ball only acts the part of a messenger, or serves as an instrument to reestablish an equilibrium between the opposite states. Hence it was erroneous to oppose to the theory of one sole fluid, with the affectation of so much confidence, the phenomena of repulsion between two substances negatively electrified, and the direction of a needle to the same point, whether moved by a current flowing to it, or a current escaping from it. In one case it moves to get rid of the fluid received, in the other to regain that which has been taken away.

No intelligent partisan of the theory of a single fluid ever said, that the air condenses round a substance negatively electrified, or electrified by subtraction.

Air never condensed round a substance electrified minus.

In the experiment proposed by Girault to decide between the two theories, an experiment that has been a thousand times made, the fluid is not transferred from one coating to the other, but diffuses itself in the void space, which no longer offers the resistance, on which its condensation and its adhesion to substances depend: yet as in this experiment the vacuum cannot form at once, the electric fluid separates in succession as the air rarefies, and prevents us from observing whether it flow from the exterior or interior surface of the bottle. For this experiment it is not necessary, to charge the bottle at a machine with a plate of resin: for to charge it negatively within, it is sufficient to present its outer coating to the conductor, while its interior coating communicates with the ground, or to charge it in the usual way, presenting its hook to the cushions of a machine, the conductor of which is not insulated. In electrical experiments made in vacuo, the fluid that ceases to be applied is transformed into light, traverses the receiver, and is dissipated in the air.

Girault's experimentum crucis.

I have written you insensibly a long letter, yet what I have said has not been the more interesting on this account. My head and my papers are filled with memorandums of facts and ideas, which my occupations do not allow me to set in order, and which render me diffuse when I have occasion to speak of them.

V.

V.

Description of the Process I have employed to ascertain the existence of Alumine in Meteoric Stones, by B. G. SAGE, *Member of the French Institute, Founder and Director of the First School of Mines**.

Fusion of a stone with alkali alters the nature of some of its principles.

MARGRAFF and Bayen proceeded to the analysis of stones by vitriolization, because they had found, that fusion through the medium of alkalis altered the nature of some earth. This appeared to me unquestionable, since chemists the most justly celebrated, as Klaproth, Fourcroy, Vauquelin, &c., who have given analyses of the meteoric stones, named aerolites by Mercati, have not mentioned the alumine, which I can affirm exists in them: for, having vitriolized some of the meteoric stones of Aigle and Salles, near Villefranche in the Lyonese, I obtained alum from both, but in unequal proportions, since the aerolite of Aigle yielded me near a fourth, while that of Salles did not afford above an eighth.

Alumine in meteoric stones.

That of Salles contained malleable iron.

I powdered and sifted through a silk searce some of the meteoric stone of Salles, an eighth part of which was irreducible to powder, because it contained portions of malleable iron, attractable by the magnet. This, being fused with glass of borax, produced ductile iron, that had the brilliancy of the purest steel when passed through the flatting mill; while the malleable iron obtained from the aerolite does not assume an equally brilliancy after being laminated. A portion of this iron had coloured the glass of borax black, and rendered it attractable by the magnet.

Treated with sulphuric acid.

To vitriolize the magnesia and alumine, which make part of meteoric stones, I introduced into a retort eighteen nominal cwt. of the aerolite of Salles, powdered and sifted; poured in an equal quantity of concentrated vitriolic acid; and proceeded to distil to dryness in a reverberatory furnace. Sulphurous acid was at first evolved, accompanied with yellow sulphur, which was found in the proportion of a

* Journal de Physique, vol. lxvi, p. 460.

thirtieth

thirtieth in the meteoric stone. At the bottom of the retort was left a grayish mass, which, after having been diluted in three parts of water, produced a sensible heat.

This solution, when filtered, was of a fine green colour, owing to the nickel and iron. When evaporated, it produced by refrigeration tetraedral prismatic crystals of a light green. Solution filtered and evaporated.

The crystallization being confused, I dissolved the salt, and obtained crystals of two distinct forms. Those of vitriol of magnesia were in tetraedral prisms, intermingled with crystals of alum, exhibiting octaedra bisected diagonally. Both these salts had a green tinge. Sulphates of magnesia and alumine crystallized.

The small quantity of alum produced by this first vitriolization showed me, that only a part of the alumine was acted on. I therefore distilled the dried residuum, which was diminished five nominal cwt. with eighteen nominal cwt. of vitriolic acid. The residuum, after being lixiviated, afforded me a solution less tinged with green; which, being evaporated, yielded me more alum than the former. The residuum of this lixiviation being dried and weighed, I found there were two nominal cwt. of alumine and magnesia vitriolized in this operation. Residuum treated afresh with sulphuric acid. More alumine and magnesia.

In order to disengage the last portions of alumine and magnesia, that remained still interposed among the silex, or pulverized quartz, I distilled the residuum a third time with twelve nominal cwt. of concentrated vitriolic acid. Treated a third time with sulphuric acid.

After what remained in the retort had been lixiviated, filtered, and dried, I found that the sulphuric acid had vitriolized two more nominal cwt. of alumine and magnesia. By distilling the residuum a fourth time with vitriolic acid, I satisfied myself, that it contained no more alumine or magnesia. Nothing remained in the retort but very white silex, weighing nine nominal cwt. More alumine and magnesia.

When I analysed the meteoric stone of Aigle, I added together the solutions of the three vitriolizations, which, on being evaporated, produced me at first alum, and afterward vitriol of magnesia. But in the analysis of the aerolite of Salles, I evaporated the solutions of the three vitriolizations separately: and hence I learned, that the sulphuric acid vitriolized the magnesia first: since the first lixivium yielded Meteoric stone of Aigle.

but

but very little alum, while the second and third produced more. This alum comports itself in the fire like that of the shops. It swells up, and assumes a reddish tinge, owing to the martial vitriol it contains.

Proportions of the component parts vary except the silex.

These comparative experiments show, that the proportions of magnesia and alumine in meteoric stones are not always the same. Those of iron varying too, it is not possible to determine precisely the quantity of these different substances, that make part of the aerolites: but the silex pretty uniformly afforded me half the weight of the meteoric stone, of which sulphur constitutes only a thirtieth.

Treatment with sulphuric acid necessary to a perfect analysis.

The existence of alumine in meteoric stones being confirmed by means of vitriolization, which likewise detects the presence of this earth in hornblende; though it escapes us, when we proceed to the analysis of these substances by means of caustic alkali, since the able chemists I have cited make no mention of it; it is necessary, for the purpose of an accurate analysis, to have recourse to the two methods.

Silex.

The silex, or quartz in a state of division, mentioned as an integrant part of most stones, may be nothing, in many cases, but the result of the decomposition of igneous salts with base of natron, the fixed alkali of tartar having more affinity with acids than natron has.

Meteoric stone cut and polished.

The fracture of meteoric stones making known but very imperfectly the arrangement and brilliancy of the native iron they include, I resolved, in order to examine it on a large surface, to get a vase turned from an aerolite of Salles near Villefranche, in the Lyonese. It was found difficult to fashion, because irregular splinters broke off before the tool; in consequence recourse was had to the file, and to rubbing it dry on a plate of cast iron covered with powdered gritstone and emery.

The last polish was given with emery and Venetian tripoli, using no water, that the iron might not rust.

Its appearance in this state.

The vase, which I offer to the inspection of the Institute, exhibits parcels of iron of irregular configurations, which have a silvery lustre, intermingled with very small spots of a greenish yellow, disseminated in a quartzose gangue of an ashen gray.

VI.

VI.

Letter from Mr. Rampasse, *formerly Officer in the Corsican Light Infantry, to Mr.* Cuvier, *on a Calcareous Breccia containing fossile Bones found in Corsica*.*

SIR,

I Mentioned to you a calcareous earth containing bones which I had found in Corsica, and which might not appear indifferent to a man of science; but I did not enter into any particulars on the subject. Having at present before me the memorandums I made of my geological travels in that island, I shall give you an account of that very curious earth, acquainting you with all the circumstances that gave rise to its discovery.

Calcareous earth containing bones in Corsica.

Visiting the north part of the environs of Bastia, that faces the east, and desirous of visiting likewise the upper part of the chain separating the gulf of San Fiorenzo from that of Bastia, I took my departure from the seashore near the Jesuits tower, distant from the city about a mile and half. I ascended a small narrow hill, the steeply sloping sides of which are full of rocks, some in their natural situation, others loose. When I had proceeded on the hill to the distance of about a mile and half from the sea, and about two hundred yards above its level, being on the side opposite to that on which I began my walk, a considerable ledge of calcareous stone presented itself to my view in an oblique situation from south to west, steep, and having on it the appearance of an irregular column, reaching from top to bottom, with a brownish red ground; and at a distance three others much shorter, being only two or three feet high. The rest of the rock was of a blue ground mixed with white. On examining this vast mass of stone, I perceived that a quarry had formerly been opened in it; and desirous of knowing at what period, I inquired concerning it of some of the vine-dressers, among whom were some old inhabitants of the villages of Santa Lucia and Leville, near the spot. They told

Hill near Bastia

containing a stratum of calcareous stone

intersected perpendicularly by a stone of a different appearance.

Formerly quarried.

* Journal de Physique, vol. LXV, p. 426.

me, that in 1774 a great quantity of stone had been taken from this place, to build several houses, and walls of enclosures among the surrounding vineyards. In fact this calcareous mass had been wrought so much in one part, that it was not more than two or three feet thick; while the other part, which was yet untouched, was twenty-five or thirty: which led me to judge, that the general height of the whole mass might have been about five and twenty feet.

The stratum described.

This ledge, about seventy or eighty yards long, was intersected in some parts from top to bottom by a reddish brown earth; very hard, and as it were enchased in the rock, as I have said, in the shape of irregular columns. Before the opening of the quarry, this earth exhibited four columns, one of which alone remained entire, and sloped from its middle to the top. The other three exhibited only about two feet of the shaft, reckoning from the base, the rest having been cut away with the rock. Each of these columns was from three to four feet broad, and from fifteen to eighteen thick. They, as well as the rock which appeared to enclose them, were imbedded in the mass of earth at their back, throughout the whole of the extent of the ledge both in length and height; which must formerly have exhibited the appearance of a very extraordinary intercolumniation, both on account of the colour of the earth, which was very different from that of the stone, and from the irregularity of these columns, which had altogether the appearance of so many distorted walls, constructed in the interior of the stony mass.

Similar appearance in other strata.

I had before had an opportunity of observing a similar natural architecture in other calcareous ledges still more extensive than this, as those situate to the south near the city of Bastia, on the estate of Messrs. Pallavicini of that city, in which appears, independent of a resemblance of pillars of a blackish gray, an earth not so hard as that mentioned above, of a different colour too, and not so thick, which is arranged horizontally in strata between the beds of stone, but containing only small nodules of the same earth harder than the body of the earthy mass.

From the mines sprung in the quarry, this brownish red earth, being blown up with the rock to which it appeared

to

to adhere, was scattered about the bottom of the quarry in large blocks. These blocks, when blown up, had left large vacuities in their old place, in which were observable a number of cavities five or six inches in diameter.

This vast ledge is in the midst of a wood of wild and domestic olive-trees, on the ridge of the hill I have mentioned, where it forms a sort of little mountain. It is surrounded also by a number of blocks of stone, likewise calcareous; some of which, having their angles broken off, appear to have already undergone a change of place; while others perhaps have come from the ledge itself, for there is no doubt, that it was formerly more extensive, than when the quarry was opened in it, since every thing indicates a derangement of things in this place. This ledge, of a circular form, rests principally on a bed of the same reddish brown earth, perfectly resembling that which composes the columns, and a blackish vegetable mould forms its base. The north and east are the two points, toward which the part wrought looks; and that untouched faces the west; so that the whole ledge forms a semicircle.

Situation of the stratum.

On attentively observing this calcareous mass, I perceived that a number of little bodies, which appeared to me homogeneous, were embedded as it were in the brownish red earth, and, being equal in hardness to the stone, induced me to give it the name of calcareous breccia. I noticed three different kinds of these small bodies: some of a calcareous nature, and a rhomboidal figure, inserted in it in groupes; others of a refractory nature, with the aspect of a foliaceous granite, containing little laminæ of mica in a state of alteration; and lastly little round long bones, perforated at one end, and destitute of spongy texture, which appeared to me tibias of some large bird or small quadruped. Continuing my remarks, and desirous of being more fully acquainted with the contents of this earth, I attempted to break several blocks, to get a good specimen. Not being able to accomplish this without a great deal of trouble and exertion, and my curiosity not being satisfied, I bethought myself of recurring to the cavities and vacuities, which the blowing up of the rocks had laid open. In fact by this means I was more successful, and with less labour; for

The interposed stone contained fragments of other stones, and bones.

without much trouble I separated with my hammers from these cavities, the sides of which were already shaken by the explosion of the mine, the fine specimens I brought away, and which I take great pleasure in sending to you for your examination.

Two specimens.

In the large specimen, and in the small one which was broken off afterward, may be distinguished a head; a pretty large rib, the spongy texture of which is converted into earth; and other bones, that appear to have belonged to small quadrupeds. The leg, thigh, and foot bones, and other bony parts, observed in other places, appear to be those of birds; and lastly in other specimens are portions of shells, that I believe to be of the helix genus.

A similar stone found at Gibraltar, Cette, and Nice.

This earth, or calcareous breccia, having led me to various reflections, I would willingly add to the circumstances I have related important details, to which its discovery leads; but it would swell my letter too much, to trace the causes that have produced these interesting facts. I shall only say, that a similar earth has been found at four different points of Europe, Gibraltar, Cette, Nice, and Corsica: and as these four points, compared with all Europe, may be considered as one, I conceive, that the discovery of this earth in Corsica not only indicates this island as the point, to which the eye of him who would observe the grand revolutions, that every thing announces to have existed, should be turned; but also becomes a fertile source of luminous ideas respecting those great catastrophes, that have taken place at a very remote period in this part of the Mediterranean. Time, and tours undertaken and pursued without interruption, can alone acquaint us with the extraordinary events, some proofs of which have already been found by enlightened men.

Mr. CUVIER'S *Answer to Mr.* RAMPASSE.

The bones belong to the genus lagomys.

I have been greatly interested, sir, by the observations you have communicated to me respecting the bony breccia of Corsica, and have examined with the greatest care the bones they contain. Among them is a head well characterized,

terized, which must have belonged to the genus lagomys, of which there are at present but three species known, all of them discovered in Siberia by Pallas.

It would be a subject of some curiosity, to examine these breccia still farther on the spot, and obtain from them a larger quantity of bones, in order to discover whether these animals were buried there in great numbers; whether the bones of other animals accompany them, and, if so, of what countries these are natives; and lastly if their bones are worn, broken, and have the appearance of having been brought from a distance.

Farther inquiries respecting them pointed out.

You are aware, sir, without my entering into the subject, how much light the solution of all these questions would throw on the history of the revolutions of our globe, at present so obscure.

VII.

Extract of a Letter from Professor Picot *of Geneva to the Editors of the Bibliothèque Britannique*.*

I Shall first speak of that beautiful comet, which excited such a lively and general curiosity last year [1807]. Discovered in September, immediately after passing its perihelion in the constellation of the Serpent, it travelled the following months nearly at the rate of a degree a day in those of Hercules and the Lyre. Imperceptibly diminishing in lustre as it increased its distance from us, and even ceasing to be visible to the naked eye, it was followed only by a few astronomers in those intervals, when the fogs and winds permitted observations to be made. Mr. Olbers availed himself of these favourable moments, and observed it till the 19th of February, when his labours were interrupted by an illness, from which he was not recovered the 28th of

Comet of 1807.

* Journal de Physique, vol. LXVII, p. 133.

April,

April, when his letter to me is dated. Mr. Bessel, the coadjutor of Mr. Schroeter, in his fine observatory at Lilienthal near Bremen, was able to follow it till the 24th of February; and by him were calculated the elements, that will appear in this paper, from the observations made at Bremen and Lilienthal.

Its period perhaps 1900 years.

He imagines, that he can determine the period of the return of this comet to its next perihelion. According to him its revolution in its orbit is 1900 years: but Mr. Olbers says, that we cannot depend on the accuracy of this determination. It is much to be wished, that he, or some other astronomer, would collect at leisure all the accurate observations of this beautiful comet, made at different places, revise these calculations, and endeavour to arrive at a probable result. The reappearances announced of two comets; that of 1456, which has been seen four times, and that of 1532, which has been seen twice; demonſtrate the general proposition, that these returns may be predicted. If however Mr. Bessel be near the truth with respect to the length of the revolution of this comet, the approaches it may make during so many centuries to the large gravitating bodies belonging to our system may occasion perturbations in its course, of which no calculation can be formed.

Elements of the last 21.

To finiſh this article of comets, I ſhall annex the elements of the last twenty-one from Mr. Olbers. They are valuable, both because he himself has observed them, and calculated several of their elements, namely, those marked with a star; and because they determine with more precision than the *Connoissance des Temps* one of the most essential circumstances for the calculation of their periodical revolutions, namely, the precise instant of their passing their perihelion. I set out with the numeration of Pingré in his *Cométographie*, to indicate the number answering to each of these last comets in the complete catalogue of those, the orbits of which are calculated.

Elements

Elements of the laſt Twenty-one Comets, according to OLBERS.

No.	Year.	Passage through the Perihelion in mean time at Paris.		Longitude of the Perihelion of the Comet.	Perihelion Distance the Mean of the Earth being 1.	Longitude of the ascending Node.	Inclination of the Orbit.	Direction of the Motion.
			h. ′ ″	s. ° ′ ″		s. ° ′ ″	° ′ ″	
77	1790	Jan. 15.	5 15 0	2 0 14 32	0·75310	5 26 14 46	31 54 15	Retrogr.
78	1790	Jan. 28.	7 45 30	3 21 44 37	1·06329	8 27 8 37	56 58 13	Direct.
79	1790	May 21.	5 56 15	9 3 43 27	0·79796	1 3 11 2	63 52 27	R.
80	1792	Jan. 13.	13 44 13	1 6 29 42	1·29302	6 10 46 15	39 46 55	R.
81	1792	Dec. 27.	7 56 27	4 15 52 35	0·96683	9 13 14 44	49 7 13	R.
82	1793	Nov. 4.	20 21 0	7 18 42 0	0·4[illegible]34	3 18 29 0	60 21 0	R.
83	1793	Nov. 18.	15 38 0	2 11 0 0	1·5045	0 2 20 0	51 56 0	D.
84	1795	Dec. 15.	8 29 50	5 10 29 0	0·24379	11 23 14 0	22 10 0	D.*
85	1796	April 2.	19 55 6	6 12 44 13	1·57816	0 17 2 16	64 54 33	R.*
86	1797	July 9.	2 40 31	1 19 27 8	0·52661	10 29 15 37	50 40 34	R.*
87	1798	April 4.	12 7 37	3 15 6 57	0·48459	4 2 12 21	43 44 42	D.*
88	1798	Dec. 31.	22 5 15	1 3 35 5	0·77479	8 9 30 2	42 14 52	R.*
89	1799	Sept. 7.	5 43 26	0 3 39 10	0·84018	3 9 27 19	50 57 30	R.*
90	1799	Dec. 25.	19 3 50	6 10 14 52	0·26688	10 26 27 18	77 0 47	R.
91	1801	Aug. 8.	13 0 0	6 1 1 0	0·249	1 12 8 0	20 20 0	R.*
92	1802	Sept. 9.	21 32 29	11 2 9 4	1·09411	10 10 15 39	57 0 47	D.*
93	1804	Feb. 13.	14 16 16	4 28 44 51	1·07117	5 26 47 58	56 28 40	D.
94	1805	Nov. 18.	3 14 27	4 27 51 28	0·37862	11 14 37 19	15 36 32	D.
95	1805	Dec. 31.	6 21 1	3 19 21 51	0·89193	8 10 33 35	16 30 32	D.
	1806	Dec. 28.	22 2 10	3 4 4 30	1·08193	10 22 18 37	35 4 5	R.
7	1807	Sep. 18.	7 59 48	9 0 56 0	0·64648	8 26 46 3	63 10 53	D.

General conclusions.

In this table the reader may perceive,

1. That, during the last eighteen years, observations on comets have been more frequent than ever; and that the vigilance of astronomers to discover new ones has equalled that they have employed in discovering also new planets.

Perihelion distance.

2. That, of the comets observed, those which in their perihelion have approached the sun nearer than the Earth's mean distance from it are double the number of those, the perihelion of which exceeds this distance. Four of them have approached nearer to the sun than the tenth of the Earth's mean distance, and four others nearly within one fifth of it.

Direction.

3. That, with regard to the direction of their motion, twelve have been retrograde, and nine direct.

Longitude of the node and perihelion.

4. That, as to the longitude of their ascending node, and of their perihelion, it answers indifferently to the 360 degrees of the circle, on which that longitude is reckoned.

Difference between comets and planets.

Nothing in the solar system is more remarkable, than the indeterminateness of the places of the orbits of comets, of their inclination in all angles to the plane of the ecliptic, of their eccentricities, of the place of their perihelion, and of the direction of their movement, if compared with the precise determinations to which the planets are subjected. The orbits of the latter are nearly circular, and very little inclined to the plane of the ecliptic: all the planets, both primary and secondary, move in the same direction, from west to east; and those, the rotation of which we have been able to observe, turn on their axis in the same direction. Thus the planetary system, says Mr. Laplace, in his *Systéme du Monde*, displays to us forty-two movements in this direction, and it is four millions of millions to one, that this arrangement was not the effect of chance. Different final causes therefore must have presided over the different formation and destination of the planets and comets.

Not owing to chance.

VIII.

VIII.

On the Influence the Shape of a Still has on the Quality of the Product of Distillation: by Mr. Curaudau, *Member of the Pharmaceutical and several other Societies*.*

When Mr. Chaptal pointed out the fault of our common stills, and proposed to substitute for them broad and shallow alembics, I was one of the first, to consider the reform as very useful, and at the same time highly conducive to the interest of the distiller. Accordingly, having had occasion to write on the same subject, I proved, that I coincided in opinion with Mr. Chaptal, by extolling the advantages, that shallow stills possessed over deep ones.

Shallow stills proposed by Chaptal,

Though I had no foundation for my opinion but theory, and the particulars advanced by Mr. Chaptal in support of the system he proposed, I was far from thinking, that I should have to retract the assertions I had made, and that experience would destroy the plan of reform, the adoption of which I had sought to promote.

and recommended by the author:

However, as it is the duty of a man, who studies useful improvements in the arts, not to compromise the progress of science, or sacrifice to selflove whatever tends to correct the errours, into which he may have fallen, I hasten to communicate to the physical and mathematical class of the Institute the observations, that have arisen from the objections made to me by those, who have employed shallow stills.

but his opinion now changed.

In deep stills, the liquor, at a certain time, receives more heat, than it gives off by evaporation: the temperature then may rise, till it reaches the term at which the ebullition is complete, an essential condition for effecting the combination of the alcohol with the aroma of the wine, before it is separated from it.

In deep stills the liquor heated more,

and finer flavoured.

No doubt shallow stills greatly shorten the time of distillation; this is a fact, on which all distillers agree: but they

Distillation quicker in shallow ones.

* Sonnini's Biblothèque physico-économique, 1808, tom. I, p. 106.

say

say too, and this cannot be disputed, that the brandy obtained in this method contains nothing or next to nothing of that aroma, which is so grateful to the smell, and communicates the agreeable flavour, that distinguishes well made brandy.

Experiments proving

It is this difference in the quality of the products, that has engaged the attention of distillers. I thought at first, that they might have been deceived by their prejudices, and boldly disputed their opinion: but finding, that shallow alembics fell more and more into disrepute, I resolved to examine for myself, whether the objections made to them were well founded. What I thought it particularly necessary to ascertain was, whether the difference in flavour between brandies distilled in alembics of the different forms were sufficiently perceptible, to authorize the preference given to one over the other. Accordingly I subjected to distillation a quantity of wine, part in a shallow alembic, part in one of the common construction.

the inferiority of the shallow still.

When I had finished the distillation, I examined both sorts of brandy, and gave them to different persons to taste, all of whom, as well as myself, uniformly gave the preference to that produced from the deep still. Thus I was convinced, that the objections of the distillers were not the result of unfounded prejudice; and that the difference observed in the products of two analogous operations must depend on the circumstances of the evaporation; which were not the same in the two stills, since I satisfied myself, that, in the common still, the evaporation of the spirit does not begin to be very copious, till the heat is 70° or 75° of Reaumur [190° or 200° F.], while on the contrary in the shallow still it is very abundant from 45° to 55° [133° to 156° F.].

Difference of the evaporating heat.

This the cause of difference in quality.

This difference in the intensity of the heat produced, at the moment when the alcohol separates from the liquor that contains it, appeared to me worthy of remark, and tending to explain why the products must differ. In fact, is it not well known in chemistry, that wine distilled at the heat of a vapour bath yields a spirit much inferior in quality to that, which is produced by distillation on a naked fire?

Experience

Experience proves then, that it is necessary, to bring the wine to boil, before the alcohol is abstracted from it. This boiling favours the reaction of the principles of the wine, and is the cause of a new combination by their mutually acting upon each other, which renders the spirit more aromatic and highly flavoured, than that obtained from wine to which a similar degree of heat has not been given. Ebullition necessary.

To explain why the liquor cannot be raised to the same degree of heat in a shallow still, as in a deep one, it is sufficient to observe, that, in the former, the evaporation always keeps pace with the heat produced: in other words, if we increase the fire, we only accelerate the evaporation, without perceptibly increasing the temperature of the fluid. Cause of the difference in heat.

Hence it is evident, that shallow stills are far from being well adapted to attain this end; and the circumstance, that is essential to fit them for a speedy evaporation, is here a defect, instead of an advantage, in proportion to its efficacy.

From what has been said we may conclude:

1. That shallow alembics, though very fit for the distillation of certain fermented liquors, may sometimes alter the quality of the products of distillation. General conclusions.

2. That the inconveniences arising from the employment of shallow alembics in distilling wines arise from the facility, with which evaporation takes place in them.

3. That a high temperature is always necessary, to carry over the peculiar aroma of the wine, and perhaps too that arising from the action of heat on the principles of the wine.

4. That deep alembics ought to be preferred to shallow ones for the distillation of wine.

5. Lastly, that the best dimensions for an alembic, without regard to its figure, must be such, that the surface of the liquor heated shall be constantly greater than that from which the evaporation takes place. Thus for instance we may consider it as a rule, that the proportion between the two should be as four to one.

ANNOTATION.

ANNOTATION.

Shallow still best for malt or melasses spirits.

From the practical observations of Mr. Curaudau we may infer, as indeed he hints in his first general conclusion, that the shallow still is preferable, where the object is to prevent the peculiar flavour of the liquor distilled as much as possible from rising; as in distilling from malt, or melasses, the common materials in our country: and this not only on account of the saving in time and fuel, but of superiority in point of flavour. On the contrary, in respect to the simple or spirituous distilled waters, as they have commonly been called, where a full impregnation with the peculiar flavour of the vegetable substance employed is desirable, a deep still would appear to be preferable. The proper proportions for stills for some of the finer productions of this kind however may deserve a particular inquiry. C.

Deep still for aromatic herbs or perfumes.

IX.

On Vegetable Astringents. *By* John Bostock, *M. D.* *Communicated by the Author.*

Action of reagents on various astringents.

WHILE I was engaged with the experiments on the combination of tan and jelly, the results of some of which I have already transmitted to you, I was led to observe the action of a number of reagents upon the different astringent substances which I employed. The conclusions that I have been induced to form are, in some respects, different from those adopted by the most approved systematic writers, as well as by those experimenters, who have particularly directed their attention to this class of bodies. I propose to confine my remarks in the first instance to the gall-nut; and having adopted this as a kind of standard, I shall afterward make a few comparative observations on catechu and the extract of rhatany, substances which have been considered analogous to galls in their chemical properties.

In

Galls not homogeneous.

In Aikin's chemical dictionary* we have an account of the great diversity, which exists in the structure and appearance of different gall-nuts, a circumstance which appears previously to have been but little attended to. I have found my observations to correspond with those of the Mr. Aikins, and to be fully confirmed by my experiments. Although the same quantity of materials be employed, it is very seldom that two infusions of galls are obtained of the same strength, and I have found the difference amount to no less than $\frac{1}{3}$ of the whole weight of the solid contents. In general, however, if finely powdered galls be infused in ten times their weight of boiling water for two hours, a fluid is procured containing $\frac{1}{10}$ of its weight of solid matter. An infusion of the same strength will generally be obtained, if the powdered galls be macerated in ten times their weight of cold water for 24 hours. If powdered galls be boiled or infused in hot water, the fluid is commonly, though not always muddy, and does not become transparent, until it has been kept for some days, or even weeks, and a considerable part of its contents have separated from it, in the form of mould or sediment. The muddiness is not removed by filtering the fluid, and there is often considerable difficulty in passing it through the common bibulous paper. This muddiness renders the warm infusions improper to be employed in experiments that require any great degree of delicacy. If the galls be only coarsely powdered, warm water still produces an opake infusion, but if successive portions of warm water be applied to the same galls, the infusions will gradually become less and less muddy, until after the 3d or 4th they will be transparent; but the period when the muddiness ceases is not the same in all cases. It is probable, that the muddiness in these instances does not depend upon any part of the galls which is originally insoluble, but upon some one of their constituents which is rendered so during the process; for if a transparent infusion of galls be slowly evaporated, and the residue be afterward digested in cold water, a perfect solution of the whole can no longer be obtained. Facts of this kind have been frequently noticed

Infusions of them differ by one third of their solid contents.

Great portion soluble.

Decoction, or hot infusion, generally turbid,

even after filtration.

Successive hot infusions less and less muddy.

Some part rendered insoluble by the heat.

* Article, Gall-nut.

in

in the analysis of vegetables, and have been generally ascribed to the extractive principle, which is said to become insoluble by the absorption of oxigen*. It seems, that in all cases, the more finely the gall nuts are powdered, the stronger is the infusion, which equal weights will produce.

Galls should be finely powdered.

Successive infusions necessary.

A considerable proportion of the gall-nut is soluble in water, but for this purpose it is necessary, that several successive infusions be employed. Water will readily take up $\frac{1}{10}$ of its weight of the soluble part of the galls †; and yet if a quantity of the powder be infused with 20 times its weight of water, a 2d quantity will extract something which had escaped the first infusion. This circumstance is particularly noticed by Trommsdorf; for although he infused galls for three days in above twelve times their weight of water, yet it required four of these infusions to remove all the soluble matter ‡. The proportion of matter, which remains insoluble after these successive infusions, has been very variously estimated. Mr. Deyeux speaks of the insoluble part as a very small quantity, without stating its amount §; while Mr. Davy informs us, that he had 315 parts left out of 500, or nearly $\frac{5}{8}$ of the whole ||. In four different trials which I made with a good deal of care, I found the residues to be in the different cases nearly as $\frac{1}{7}$, $\frac{1}{8}$, $\frac{1}{10}$, and $\frac{1}{11}$ of the whole. In these experiments the number of successive infusions was from 12 to 14, and the quantity of water employed at each infusion was ten times the weight of the original quantity of galls. What is left is a dark coloured and hard body, upon which alcohol and the caustic alkalis have no action. Muriatic acid, by being boiled on it, breaks it down into small pieces, and is itself tinged of a light brown

Proportion of insoluble matter.

Action of reagents on it.

* Fourcroy, Analyse de Quinquina, Ann de Chim. VIII, 122, & alibi.
——— Système, VII, 312.
Davy, Philos. Trans. 1803, p. 237.
Thomson's Chem. V, 107.
Aikins' Dictionary, Art. Extract, p. 422.

† Mr. Davy formed infusions, which contained between 1-7th and 1-8th of their weight of solid matter. Phil. Trans. 1803, p. 240.

‡ Thomson's Chemistry, II, 355.

§ Ann. de Chim. XVII, 11.

|| Phil. Trans. 1803, p. 251.

colour;

colour; potash throws down a very minute precipitate from the acid, while the residue is rendered quite black, and strongly resembles charcoal powder; a circumstance which seems to show, that the blackness of charcoal is not necessarily connected with the the process of combustion or oxidation.

Blackness of charcoal not connected with oxidation.

In forming infusions of galls it occasionally happens, that we obtain them of a bottle-green hue. Mr. Deyeux and Mr. Davy both mention this as occurring in the latter infusions, where the same galls have had repeated quantities of water poured on them *; but it never occurred to me to observe the green colour under these circumstances, while, on the contrary, I have met with it in an infusion of fresh galls, where no shade of green could be observed in any of the subsequent infusions. It is attributed by Mr. Deyeux to a green colouring matter, which he enumerates among the constituents of the galls; while Mr. Davy ascribes it to the gallate of lime. My observations lead me to question the accuracy of this latter opinion. In the first place it seems an almost decisive objection to it, that, if lime water be added in small quantities to the recent infusion of galls, so that the tan be not precipitated, the green colour is not produced; yet in this case the lime must be employed in saturating the uncombined gallic acid, and thus forming the gallate of lime. In one of the greenest infusions that I ever procured, the oxalate of ammonia did not produce the least effect, while the subsequent addition of the most minute portion of lime water immediately caused the precipitation of the oxalate of lime. If to the infusion of galls pure potash be added, the brown colour is at first rather increased, but after some time a shade of green becomes visible. The effect is much more speedy and more decisive where the carbonate of potash is employed; a similar effect is produced by lime water, except that the green is more of the gluceous hue. In all these cases the green colour is instantly removed by an acid; where potash has been employed the fluid acquires a reddish tinge, and where lime water was used, a delicate violet. The green colour always disappears

Green tinge of the infusion

not owing to the gallate of lime.

Effect of reagents in producing or removing the green tinge.

* Deyeux, ubi supra, p. 12; Davy, ubi supra.

by exposure to the atmosphere, and it is also removed by boiling, although in this latter case it is partly reproduced after an interval of two or three days, but finally disappears. There seems to be an analogy between these changes, and the effect of acids and alkalis on many other vegetable substances, they are rendered green by an alkali, and are reddened by an acid. But the resemblance does not hold good in every respect; for the alkaline mixture loses its green colour, although its alkalescent properties continue; and I have observed the green colour to be removed by lime and ammonia, while, on the contrary, I have obtained infusions, which have exhibited the green colour, and yet by the test of litmus have proved to be decidedly acid. The green colour, wherever it exists, is immediately destroyed by the acetate of lead*. With respect to the existence of lime in the infusion of galls, the experiments which I have made on the subject lead me to conclude, that, although it may exist in the gall-nut, yet it is not taken up by the water. I have added to the recent infusion of galls both uncombined oxalic acid, and the oxalate of ammonia, without any precipitate being produced. If the ammonia be in excess a considerable effect takes place, but this is to be ascribed to the union of the uncombined alkali with the tan.

No lime found in infusion of galls.

If the infusion of galls be kept for any length of time, it always becomes covered with mould, and a sediment also falls to the bottom of the vessel. The moulding has been attributed by Deyeux, Trommsdorff, and others, to the presence of mucus, as mucus is said to be the only substance, which is capable of supporting this species of vegetation †. I conceive, however, that this opinion is not correct; and that, even if there be any thing in the infusion, to which the name of mucus properly applies, it is not the immediate cause of the formation of the mould. The muriate of tin, and the solution of jelly are the two principal reagents,

* I employ the term acetate of lead in the restricted sense, in which it is used in the new Pharmacopœia of the London College, where I may remark, the distinction which I pointed out between Goulard and cerussa acetata is recognized, and the appropriate nomenclature adopted. Powel's Translation of the New Pharm. p. 157.

† Ann. de Chem. XVII, 15; and Thomson's Chem. II, 356.

which

which are employed in the analysis of galls, the first being supposed to indicate the presence of the extractive principle, the latter of the tan. The accuracy of this deduction I shall hereafter examine, but admitting it for the present, I may observe, in the first place, that an infusion of galls, which, when recent, was copiously precipitated both by the muriate of tin and by jelly, after it has undergone the process of moulding, will be found no longer capable of being acted upon by the first of these reagents, while the effect of the second is very considerably diminished. Secondly, if successive infusions be formed from the same galls, it is only the first infusions, which are capable of moulding, and it is these only which form a precipitate with the muriate of tin and with jelly. Hence we may conclude, that the capacity of moulding is intimately connected both with that part of the galls which precipitates the muriate of tin, and also, though perhaps in a less degree, with the tan*.

In

* I think it probable, that by proper management, an infusion of galls might, by the operation of moulding, be deprived of all its tan, as well as of what has been called the extract. I kept a quantity of the infusion exposed to the atmosphere for several weeks, and from time to time destroyed the covering of mould as it was produced. Long after the infusion ceased to be affected by the oximuriate of tin, the mould continued to be formed, and the power of affecting jelly obviously decreased, until at length it did no more than produce a degree of turbidness without throwing down a precipitate. At this period, however, the whole of the fluid became so filled with the remains of the mould, and with the sediment which was deposited at the same time, that the experiment could not be pursued. Trommsdorf, as I have noticed above, attributes the formation of the mould to mucus, and even employs this operation to remove this substance, in order to obtain tan in a state of purity. I could not repeat his process, because I was not in possession of any perfectly pure alcohol, which is essential to its success. I would be understood therefore as speaking with much diffidence, when I observe, that I doubt whether it will be found practicable. It goes upon the assumption of the two data, that the extract alone is rendered insoluble by the application of heat and by exposure to the atmosphere, and that the mucus alone is separated by the moulding, both which, according to my experiments, are incorrect. Mr. Deyeux himself has observed (a), that the residue obtained by evaporating the tincture of galls, when dissolved in

Infusion of galls might perhaps be deprived of all its tan by moulding.

Moulding not confined to mucilage.

(a) Ann. Chem. XVII, 16.

water,

Two varieties of muriate of tin: In speaking of the muriate of tin it is necessary to observe, that there exist two well known varieties of this salt, which differ, both in the relation of the acid to the metal, and in the state of oxidation of the metal itself. The latter is perhaps the more essential difference, and it is that to which their characteristic effects upon the oximuriate of mercury, and the nitromuriates of gold and platina, are referred. Both the muriates of tin seem to contain an excess of acid, or to be in the state of supermuriates, but it will be sufficient at present to distinguish them by the titles of muriate and oximuriate of tin. The muriate is formed by

the muriate, simply boiling tin in muriatic acid, and preserving it carefully excluded from the atmosphere, and keeping a small quantity of the undissolved metal immersed in the fluid.

and the oximuriate. The oximuriate is procured, either by permitting tin to dissolve in the nitromuriatic acid, or perhaps more accurately, by forming a nitric oxide of tin, and then dissolving this oxide in muriatic acid; this latter is the method that I have generally adopted. In order to ascertain, that the fluids are accurately formed, it is proper to examine their effects

Tests of their being accurately formed. upon the oximuriate of mercury, and the nitromuriates of gold and platina; the muriate of tin, in consequence of its strong affinity for oxigen, throws down from the first a gray powder, from the second, what has been called the purple powder of Cassius, and from the platina, a reddish brown precipitate. The oximuriate of tin has no effect upon these solutions. I have not observed, that any difference has been noticed in the effects of these two muriates upon astringent infusions, nor indeed is it stated which of them has been employed*, yet their action is by no means identical. As I

Oximuriate preferable re- have found the muriate of tin a less delicate reagent for the different infusions than the oximuriate, I have employed the

water, is subject to mould; a fact which I have had occasion to notice, and which seems almost incompatible with his opinion of the connection between the mould and mucus. I have also found, that Mr. Hatchett's artificial tan is capable of moulding

* Davy, Proust, and others denominate the substance upon which they operate the muriate of tin; but from the effects which it produces, I apprehend it must have been either what I have styled the oximuriate, or a mixture of the two.

latter

latter in my experiments. In operating with the oximuriate of tin there is a circumstance to be attended to, which may interfere with the results; when the aqueous solution of this salt is very much diluted, it becomes insoluble, and a precipitate is formed, which in experiments on vegetable infusions might be mistaken for the effect of a combination of the oxide of tin with some of the constituents of the substance under examination. The precipitate seems in this case to depend upon the water removing a quantity of superabundant acid, which is necessary to render the salt soluble in water*. Having had occasion to make frequent use of the oximuriate of tin as a reagent, I wished to ascertain what degree of minuteness it possessed as a test for tan or extract, and for this purpose, an infusion was formed by macerating a quantity of finely powdered galls in eight times their weight of cold water for twenty-four hours. Portions of this infusion were successively added to 10, 20, 30, 40, and 50 times their weight of water, and even in the last instance the oximuriate of tin caused a slight precipitate, but no effect could be perceived when the infusion was mixed with 100 times its weight of water. The nitromuriate of tin seems to be nearly as delicate a test, and they are both considerably more so than the simple muriate.

agent for astringent infusions.

but it is precipitated by great dilution.

Its delicacy as a test.

I have had occasion to refer to the effects, that are produced by subjecting the same portion of galls to a number of successive infusions, and I shall now describe these effects a little more fully. A quantity of finely powdered galls was infused in ten times its weight of water, kept at the boiling heat for an hour, and then suffered to ſtand until the following day, when the fluid was drawn off; the same quantity of water was then added to the residue, which was boiled as before, and the operation was repeated for twelve successive days. This twelfth infusion was colourleſs, it afforded no precipitate with jelly or the oximuriate of tin, and only a slight gray tinge with the oxisulphate of iron. These infusions were kept for a fortnight, and were then examined. The first infusion contained a large quantity of sediment, and was covered with a thick coating of mould. The 2d,

Twelve successive infusions of the same galls at a boiling heat.

* Berthollet, Stat. Chim. II, 457.

and following infusions, up to the 7th inclusive, were also more or less covered with mould, and had deposited a sediment, but the fluid was now in all of them transparent, and of different shades of brown. The remaining infusions, after the 7th, had undergone no change, their colour was very bright, and in the two last, scarcely perceptible. A comparative experiment was made at the same time with another portion of galls, which was subjected to the same operation, except that it was not boiled, but only suffered to remain for twenty-four hours at the common temperature of the atmosphere. The cold infusions were generally of a deeper brown colour, they continued to act upon the reagents longer than the warm infusions, so that it was not till after the 14th, that the effect of the iron ceased to be visible. Generally the cold infusions begin to mould sooner than the warm ones, but I thought that they deposited less of the sediment. The effects of the three reagents, jelly, the oximuriate of tin, and the oxisulphate of iron, upon the infusions were noticed in every instance when they were first formed; in the earlier infusions the precipitates were very copious, but their quantity gradually diminished, until first, they were no longer produced by the oximuriate of tin, and shortly after by jelly, but it required a considerable number of additional infusions to exhaust the whole of the gallic acid. If the infusions be formed as in the above experiment, it generally happens, that after the 7th or 8th period the oximuriate of tin ceases to produce a precipitate, jelly continues to be perceptible for one or at most two infusions more, while the iron produced the black stain until the 12th, 13th, or 14th infusion.

Successive infusions in cold water.

Effects of reagents.

Gallic acid more readily soluble than tan.

When the three reagents mentioned above are added to the infusion of galls at different lengths of time after its formation, the iron is the first which produces an effect; while the jelly and the oximuriate of tin commence later, and nearly about the same period. The gallic acid is so readily soluble in water, and it is detected with so much minuteness by the oxisulphate of iron, that almost at the same instant that the galls are added to the water, does the fluid become capable of producing the gallate of iron. I have uniformly found the effects of these reagents to follow this order, although

though it is generally stated, that gallic acid is less soluble than tan*, and it is upon this principle, that Mr. Biggins founded his process for ascertaining the relative proportion of tan and gallic acid in the different substances employed in the formation of leather†.

Tan and extract nearly equal in solubility.

The solubility of the tan and the extract, so far as their presence is indicated by jelly and the oximuriate of tin, appears to be nearly equal. This is also contrary to the generally received opinion‡, but I ground my position upon the following experiment. A quantity of galls was employed in the state of coarse powder, in order that it might more readily subside from the infusion. The proportion of the galls, and the length of time occupied in the infusion, were gradually diminished, until I found, that by infusing the galls in fifty times their weight of water for only five minutes, a fluid was obtained, capable of forming precipitates with jelly and the oximuriate of tin, which were barely visible, but as far as could be judged by the eye, equal to each other.

Other reagents employed.

Beside jelly, the muriate of tin, and the oxisulphate of iron, there have been other agents employed in the analysis of galls. Of these the principal are the sulphuric and muriatic acids, the carbonated fixed alkalis, the aluminous salts, lime water, and the acetate of lead. The acids have been considered as acting principally upon the tan, and with this view have been proposed as a means of separating it from the other ingredients of the infusion§. They are, however, less delicate tests than jelly; for I have found, in the successive infusions, that jelly still throws down a considerable precipitate, when they have ceased to act; of the two, the sulphuric is the more delicate. Nearly the same remarks apply to the carbonated alkalis and to lime water, as to the acids; they both throw down copious precipitates

Acids.

Limewater.

* Seguin and Chaussier, Journ. Polyt. IV, 678.

† Phil. Trans. 1799, 261. Thomson's Fourcroy, III, 93.

‡ Davy, Phil. Trans. 1803, p. 234.

§ The proposal seems to have been first made by Mr. Dizé, but Proust and Vauquelin both agree, that the tan may be completely separated by the acids. Ann. de Chim. XXXV, 37.

from

from the infusion of galls*, but in the successive infusions, for some time after they have ceased to act, jelly still continues to produce a precipitate. Lime water has been proposed by Mr. Merat-Guillot as the most commodious agent for separating the tan from the other ingredients in the infusions, in order to obtain it in a pure state†, and Mr. Murray seems to regard it as the least exceptionable process‡. The aluminous salts, alum, the sulphate, and the muriate of alumine, have been employed to denote the presence of extract in the infusions§: but whether they act upon the tan or extract, they are much less delicate in their operation, than either galls or the oxides of tin. By successively diluting an infusion of a known strength, and examining it at different periods with jelly, the oximuriate of tin, and alum, I have always found the effect to cease first in the alum. Sulphate of alumine is rather more delicate than a saturated solution of alum, while muriate of alumine seems to be less so. The most delicate and the most universal precipitant of vegetable infusions is the acetate of lead, which acts equally upon all the constituents, the tan, the extract, and the gallic acid, and removes them completely from the fluid. In the detection of the gallic acid it seems to exhibit even more delicacy than the oxisulphate of iron. Mr. Vauquelin, in an elaborate and valuable paper on the effect of reagents on the different species of cinchona||, employed the tartarized antimony as one of his tests. I formed a saturated solution of it in water, and observed its action on the infusion of galls. The effect is very considerable, converting, as it were, the whole of the fluid into a pulpy mass; the precipitate subsides very slowly, but it is easily separated by a filtre, and leaves the infusion perfectly transparent and

Aluminous salts.

Acetate of lead

Tartarized antimony.

* Deyeux, Ann. de Chim, XVII, 19.
Proust, ibid. XXXV, 32.
Although Mr. Deyeux first noticed the action of the carbonated alkalis upon tan, he does not appear to have attempted to procure it in a state of purity by this process.

† Ann. de Chim. XII, 323.

‡ Chemistry, IV, 275

§ Davy, Phil. Trans. 1803, p. 268.

|| Ann. de Chim. LIX, 113. Journal, vol XIX, p. 106, 203

colourless.

colourless. To this filtered fluid jelly and the oximuriate of tin were added without the slightest effect, and the oxisulphate of iron only produced a blackish green tinge. In this case it would appear, that the whole of the tan and the extract, and the greatest part of the gallic acid were removed by the antimony. In consequence of the readiness with which the nitromuriate of gold parts with its oxygen, I thought of trying the effect of this reagent on the infusion of galls. Its first effect was to convert the brown colour of the infusion into a dull blackish green, and after some time a brown precipitate was thrown down in moderate quantity. I was led by analogy to try the nitromuriate of platina; the infusion was rendered instantly opake, and a reddish brown precipitate was formed.

Nitromuriate of gold,

and of platina.

These observations on the effect of the different reagents upon the infusion of galls naturally lead to some considerations respecting the constituents of the infusions, and also of the galls themselves. The soluble part of the gall-nut is said to consist of four principal ingredients, tan, gallic acid, extract, and mucus. The distinct existence of each of these substances is supposed to be proved, either by our being able to procure it in a separate state, or by the employment of some tests which may recognize its presence. To the tan and the gallic acid, both these methods of proof are, to a certain extent, applicable; they may, in some degree, be separated from the other parts of the galls, and we are able to ascertain their presence by tests of the greatest delicacy. There is reason to conclude, that, whenever jelly throws down a precipitate from a vegetable infusion, tan is present, and is the immediate cause of the effect; although it is probable, that it is not tan alone which unites itself to the jelly. The existence of gallic acid is most distinctly proved; it may be obtained in a state of almost perfect purity, and it may be detected by the oxisulphate of iron in a way that can scarcely be mistaken. But the proof of the existence of extract is not so direct; it is confessed, that we are unable to procure it separate from the other parts of the galls, and therefore we are obliged to form our opinion, either from the effect of tests, or from the observance of some changes that the infusions undergo, which are thought not to

Constituents of the soluble portion of galls.

Tan and gallic acid.

Extract.

to be referable to the other constituents. The circumstances, that have been adduced to prove the existence of extract in the infusion of galls, may be referred to the following heads. 1. When jelly has been added to the infusions, until it no longer produces a precipitate, the fluid will still be precipitated by the oximuriate of tin. 2. If an infusion of galls be exposed for some time to the atmosphere, and especially, if it be kept at an increased temperature, a part of its contents will be rendered insoluble, and will separate from the fluid. 3. If two portions of galls be infused in water, one for a short space of time, and the other for a longer period, they will be found to be differently affected by the reagents; the quick infusion being proportionably more acted upon by jelly, and the slow infusion by the oximuriate of tin. I shall consider each of these points individually, and shall examine how they authorize our conclusions in favour of the existence of extract.

The whole of the precipitate by jelly should be separated, before more jelly is added.

The first of them I do not propose to controvert, and yet I thing it presents some degree of ambiguity, of which those who have written or experimented upon the subject do not seem to have been perfectly aware. I am disposed to believe, that the effect has been a good deal exaggerated. When we add jelly to the infusion of galls, it seldom happens, that the whole of the precipitate is separated at once; a part of it remains suspended in the fluid, giving a greater or less degree of opacity; and if in this state more jelly be added, it will appear to produce no more effect, or even by farther diluting the fluid, to render it more transparent, and partially to redissolve the solid contents*. If however, in

* Mr. Davy states, that in the addition of jelly to an infusion of tan, if the jelly be added in excess, part of the precipitated compound will be redissolved. In order to observe this effect the following experiment was tried. A quantity of a weak infusion of galls had about twice as much jelly added to it as I supposed would form the most perfect compound; a dense substance was precipitated, and the whole of the fluid was rendered milky. Two equal quantities of this milky fluid were put into separate glasses, to one an additional portion of jelly was added, and to the other the same bulk of pure water. Both the fluids were rendered more transparent from the effects of dilution, but I did not perceive that it was more so in one case than in the other, although the tan must now have had about ten times its proper quantity of jelly added to it.

this

this case we defer the addition of the jelly, until the fluid shall have had time to deposit its contents in a solid form, we shall find, that a fresh quantity will be precipitated. If this operation be repeated, until the fluid no longer affords any farther precipitate with jelly, the oximuriate of tin will indeed still produce some effect, but not in general a very considerable one. Nor indeed does it certainly follow, that this small quantity of precipitate ought to be attributed to the union of extract with the oxide of tin. Although the infusion has ceased to precipitate upon the addition of jelly, although the jelly was added in small quantities at once, and no more added than what seemed necessary, yet I believe that the fluid may still contain both jelly and tan. I found my opinion upon the circumstance, that in the successive additions of jelly to an astringent infusion, the first quantity added unites with a large proportion of tan, and forms a more insoluble compound, than any of the subsequent ones; and that in proportion as we proceed, the jelly becomes incorporated with less and less tan, and forms a compound less and less insoluble; until at length a substance is formed, which remains in a state of half solution, and which renders the fluid opake, without ever producing a complete precipitate. This kind of combination between jelly and tan may be inferred from some of the experiments, which I mentioned in my former paper, and will be farther supported by the following considerations. The weight of the precipitate formed by the addition of jelly to an astringent infusion is considerably influenced by the manner in which the jelly is added, whether all at once, or in successive portions. If we add together at one time the proportion of tan and jelly which we suppose will mutually saturate each other, we procure a dense precipitate, which separates immediately, and leaves the fluid transparent; whereas if jelly be added to tan in successive portions, a larger quantity is necessary before the fluid exhibits an excess of jelly, the precipitate separates more slowly, it is in larger quantity, less dense in its consistence, the fluid retains a degree of opacity, and continues for a considerable time to deposit a sediment. The following comparative experiment bears also upon the same point. An equal quantity of the extract of rhatany and of prepared jelly, each in solution, were added

Compound of jelly and tan less and less insoluble,

and differs according to the mode in which the jelly is added.

Experiments with rhatany.

added together, and a dense precipitate was formed, the fluid was left transparent, and nearly in a neutral state. The same quantity of jelly as before was then added to ½ of the former quantity of rhatany; here a precipitate was thrown down, which was less dense and of a lighter colour, the fluid was rather less clear than in the former case, and it produced a slight precipitate by the addition of more rhatany. A third experiment was then performed the reverse of the last. The original quantity of rhatany was added to ½ of its weight of jelly; the fluid was rendered perfectly opake, but the precipitate very slowly subsided, and it continued for several days to deposit fresh quantities of sediment, but no farther precipitate was produced by the farther addition of jelly. Beside this imperfect compound of tan and jelly, which I suppose to be still retained in the fluid, it certainly contains gallic acid, and probably the neutral and earthy salts, which are found in the gall-nut. I am not prepared to say, that it is upon any of these substances that the oxide of tin acts; but I think, that we are justified in hesitating before we conclude, that the precipitate which is formed depends upon a substance, the existence of which is only rendered evident by the process in question.

Precipitation by exposure to the atmosphere and heat questionable proof of the presence of extract.

The second proof, that has been brought of the existence of extract in the infusion of galls, is the circumstance of a part of the matter in solution being rendered insoluble by exposure to the atmosphere, or by the application of heat, a property which is thought to be characteristic of this peculiar substance. Here again, without disputing the fact, I am inclined to hesitate as to the inference, and to doubt, whether the matter which separates be confined to this peculiar constituent of the fluid. If the infusion of galls be evaporated by a heat not greater than that of boiling water, a deep brown, brittle, transparent mass will be obtained, which cannot be entirely redissolved; and if the clear part of the solution be poured off, and treated in the same manner, an insoluble part will again be obtained; and this operation may be repeated for several times in succession on the same portion of fluid with the same result. I have carried it to the fourth period, and I have observed no change in the nature of the fluid, nor did its power of producing

ducing the insoluble residue appear to be diminished. This experiment proves, that the supposed extract of an astringent infusion cannot, according to the common opinion, be separated by one evaporation; and I think it may lead us to doubt, whether the effect is not rather produced upon its solid contents in general, than upon any one part of them. In confirmation of this supposition we may observe, that there are several processes, in which the tan itself is rendered insoluble by the addition of oxigen. Such is thought to be the method, in which the oxide of tin operates upon it; for although what is thrown down is a compound of tan and oxide; yet if the oxide be removed by the action of a hidrosulphuret, the tan still remains insoluble. The same kind of effect is also produced by the nitric and the oximuriatic acids; they throw down from the infusion of galls an insoluble compound, and deprive the fluid of the property of precipitating jelly. The idea, that insolubility after evaporation was a specific characteristic of extract, seems to have originated from the experiments that were performed by Mr. Fourcroy on the bark of St. Domingo; but the constituents of this bark are so different from those of galls, that we are not authorized in extending the analogy from one to the other, unless it be supported by some independent facts. So far therefore as I may be warranted to draw any conclusion on this subject from my own observation, either of the effect of successively evaporating the same infusion, or of the changes mentioned above, which are produced by long exposure to the atmosphere, I should conclude, that the tan itself is rendered insoluble in both these operations.

Not distinguished from tan by its greater solubility.

The third circumstance, which has been adduced to prove the existence of extract, and especially to distinguish it from tan, is the greater insolubility of the latter while they both exist in their natural state. In analysing vegetable astringents we are told, that by subjecting them to a hasty infusion, we shall procure the tan nearly free from extract, while by continuing the infusion for a greater length of time we get a fluid which chiefly consists of this substance. It is true, that in applying water to galls, the first portion takes up more than the subsequent ones; but that

which

which is afterwards taken up does not seem to be materially different from what is first dissolved; it only appears, that in this, as in every other instance, the latter portions of soluble matter are retained more obstinately by the insoluble part. The relative effect of jelly and the oximuriate of tin were always, as far as I could judge, exactly in proportion to the strength of the infusion, whether it formed in a longer or shorter time; in the infusions which were made the most hastily, both the reagents produced a precipitate, and however long the maceration had been continued, still the effects seemed to be proportionate to each other. Indeed the results which are obtained, when we make a number of successive infusions, are directly adverse to the commonly received opinion; for I found, as I have already stated, that in the last infusions jelly was frequently capable of forming a precipitate after the oximuriate of tin, but that the converse never took place.

Farther difficulty in distinguishing tan from extract.

It may be farther observed, respecting the distinction between tan and extract, that the two reagents, which are the appropriate tests of each, jelly and the oximuriate of tin, both of them act powerfully upon the opposite substance, i. e. jelly upon extract, and the oximuriate of tin upon tan. With respect to the latter, it is known that Proust, who has exhibited so much sagacity on the subject of vegetable infusions and their action upon the metallic oxides, originally introduced the muriate of tin as a reagent for tan, and in his first experiments seems to have had no idea of its acting upon any other substance*. And with respect to the effect of jelly, whatever may be the body upon which it exerts its primary action, we find, that, when it has ceased to precipitate an infusion, what is then thrown down by the oxide of tin is at least in very small proportion to what would have been produced in the recent infusion. The facts which I have mentioned above, respecting the successive infusions and the formation of mould show an intimate connexion between the two supposed substances, and indeed scarcely permit us to draw any line of distinction. And the same idea will be still farther countenanced by con-

* Ann. de Chim. XXV, 225.

sidering

sidering what are the characteristic properties of extract, as stated at full length by Mr. Vauquelin; for we shall find, that, so far from marking any essential difference between this substance and tan, they are equally applicable to the latter, and have been pointed out as even peculiar to it. Among these we may notice its solubility in water and alcohol*, its strong taste, the effect of oximuriatic, nitric, sulphuric, and muriatic acids, of alkalis, and of metallic oxides†.

Does the infusion contain nothing but gallic acid and tan?

Are we then to conclude, that the infusion of galls does not contain any constituent, to which the title of extract ought to be applied? or that, according to the original opinion of Proust, Seguin, and others, the infusion consists merely of tan and gallic acid? Upon this point I do not feel myself qualified to give a decisive opinion. Although I think the proofs, that have been adduced in favour of the existence of extract, are very insufficient, that we are not yet in possession of any method of accurately recognizing its presence, and that we are unable to say what are its characteristic properties; yet I do not conceive, that we are warranted in denying its existence. There is indeed one fact which seems a strong presumption in its favour, viz. that, if we take two portions of the same infusion of galls, and saturate one of them with jelly, and the other with the oximuriate of tin, and let each of them remain for some time exposed to the atmosphere, until they have deposited all their precipitates, they will then each of them afford

Fact in favour of the existence of extract.

* Tan is insoluble in perfectly pure alcohol, but it is readily dissolved in the alcohol with which our experiments are usually performed.

† It is stated, that extract is insoluble in ether; but this does not apply to that part of the infusion of galls, which is acted on by the oximuriate of tin, for this reagent forms a copious precipitate with a solution of the substance which is left by evaporating ether that has been digested on galls. This substance appears indeed to act as readily upon the oximuriate of tin as upon jelly or gallic acid, and I could not perceive, that it differed in any respect from the substance procured from the aqueous infusion of galls, except in being lighter coloured. Mr. Murray observes (a), that the colouring matter of saffron, which has been regarded as a specimen of pure extract, is readily soluble in ether.

(a) Chemistry, IV, 264.

some

some precipitate to the contrary substance, the fluid which had been saturated with jelly will be precipitated by oximuriate of tin, and the fluid which had been saturated with the oximuriate of tin will be precipitated by jelly. Yet when we consider the compound nature of the fluid upon which we operate, and the variety of actions which may take place between the different reagents, we are not authorized even from this experiment to draw a decided inference in favour of the existence of two distinct substances. In entertaining doubts respecting the existence of extract as a distinct principle of vegetables, I feel happy to have my opinion supported by that of Mr. Murray *.

but this far from decisive.

(To be concluded in our next.)

X.

Question on the Preparation of Cork for Modelling. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

How may the elasticity of cork be destroyed?

I Should be obliged to you, or any of your correspondents, if they could inform me of the method of destroying elasticity in cork; or what process it undergoes, to render it fit for modelling. If you recollect its having been noticed in your Journal, by mentioning the volume you will equally oblige,

Yours, &c.

R. Z. A.

ANSWER.

Elasticity of cork owing to its texture,

I am not acquainted with the method of depriving cork of its elasticity, but do not think my correspondent will find much difficulty in discovering it by experiment.—From the texture of cork as seen under the microscope, it appears to be

* Chemistry, IV, 260.

be formed of woody fibres, with large interstices between them; and the elasticity of this substance is probably owing to the flexibility and spring of these fibres, and the difficulty with which the included air can be driven out from the cavities. It should seem, that, if the fibres could be rendered less flexible, and the spaces partly filled, the whole mass would become much less elastic. This may be tried by experiments on a small scale. A slice of cork may be immersed in any hot liquid, which becomes stiff or brittle by cold, such as melted resin, or its solution in alcohol, or glue, or gum water, or tallow, or starch, or varnish, or any other material having the property first mentioned, and of which the list is not very numerous. The cork should be repeatedly compressed under the fluid, in order that it may imbibe it, and the whole allowed to cool before the cork is suffered to rise from the surface. Many trials of this sort may be made in a short time over a candle in an iron spoon, or in the small copper or iron vessels used for pastry; and when the result is thus obtained, the operator must contrive and manage a larger apparatus according to his convenience, and the intended purpose.

and would probably be destroyed by filling its interstices with some hard substance.

W. N.

XI.

On the Dusodile, a new Species of Mineral; by Mr. L. CORDIER*.

THE new bituminous substance, which I am about to make known, was found in Sicily by Dolomieu. The specimens collected by that celebrated mineralogist arrived at Paris about ten years ago; and I then drew up a description of it under his eye, but various circumstances had prevented me from publishing it. I shall now give it, adopting the method of Haüy.

A species of bitumen found in Sicily.

* Journal de Physique, vol. LXVII, p. 277.

The

Its state. The dusodile is in the compact state, and presents itself in the form of irregular masses, which fall into very thin leaves with great facility. The following are its characteristics.

Essential character. It burns with an extremely strong and fetid bituminous smell; leaving a considerable earthy residuum.

Physical characters. Its specific gravity is 1·146.

With respect to hardness, it is easily cut, and reduced into thin and very fragile leaves.

The leaves are a little flexible.

Its colour is greenish gray.

It is opake; but the thin leaves become translucid by maceration in water.

Its smell, when breathed upon, is argillaceous.

Chemical characters. It is weakly combustible with a clear flame, and an insupportable bituminous smell, resembling that elicited by friction from the most fetid calcareous stones. This smell is so strong, that we are not very sensibly affected by it till a few instants after the combustion, that is to say, when the smoke is completely diluted with the air. The burning of a very small piece is sufficient to poison a room for more than an hour.

Combustion leaves a considerable earthy residuum, forming more than a third of the original weight.

By maceration in water its leaves separate of themselves, and become not only translucid, but perfectly flexible.

Distinguishing characters. Dusodile is distinguishable from coal, by the latter being always of a black colour, more dense, and not changed by the action of water. From bitumens, whether solid or glutinous, as these, when heated gently, or rubbed between the fingers, emit a smell resembling that of pitch; and when burned leave scarcely any earthy residuum, and give out no such smell as the dusodile. From the common elastic bitumen, as this has naturally a very perceptible bituminous smell, and is completely elastic; while the dusodile is in very fragile leaves, and emits an argillaceous smell when breathed on. The elastic bitumen too burns with leaving scarcely any residuum, and emitting a smell that is neither powerful nor diagreeable. From indurated elastic bitumen,

bitumen, as this burns in the same manner as the preceding, its fragments exhibit no appearance of flexibility, and maceration in water does not in any respect alter their consistency.

Its texture admits of no variety, being at the same time compact and foliaceous: but it has two varieties of colour, greenish gray, and yellowish gray. Varieties.

This mineral is found at Melilli, near Syracuse. It forms a stratum of no great thickness, extended between beds of secondary limestone. Where found.

It appears, that attempts have been made to work it out, but they have not been pursued. This is certain, that the combustible fossil it contains has long been known in the country. The inhabitants give it different names, some calling it the bituminous foliaceous earth of Melilli, others devil's dung. Both these names being equally improper, I have thought it necessary to frame one more suitable to mineralogical nomenclature. That of *dusodile*, which from its Greek root implies fetid, was naturally suggested by one of the most remarkable properties of this new kind of bitumen, that of diffusing a detestable smell when burned. Name.

XII.

Memoir on the triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. LE COMTE DE BOURNON, *F.R. & L.S.**

THIS memoir was written chiefly as an answer to that printed in the first part of the Philosophical Transactions for 1808 †, in which Mr. Smithson its author, criticises with as little justice as decency a former memoir of mine Former memoir of the author criticised by Mr. Smithson.

* Translated from the original, communicated by the author, and revised by him.

† See Journal, vol. XX, p. 332.

mine on the endellion, which was printed in the first part of the Transactions for 1804. It may appear strange, particularly to those who have read Mr. Smithson's sharp critique, that I have so long delayed answering it: but this delay was owing to one of those peculiar circumstances, happily not very frequent, which the mind is as unable to foresee, as prudence is to avoid. Chance made me acquainted with the criticism of Mr. Smithson, at the time of its being delivered to the secretary of the Royal Society, Dr. Wollaston, at whose house I then happened to be. He gave me permission to look it over. Its nature surprised me; and this was all the impression it would have made on me, had I not immediately felt the disagreeable necessity I should be under of answering it, if it should be admitted into the Transactions of the Royal Society; a circumstance, which I could never have supposed would take place, had I not had some particular reasons to be apprehensive of it. I requested in consequence Dr. Wollaston to favour me with a copy as soon as it should be printed; which he promised me. Some time after, being at the house of the same gentleman, whom I was frequently led to visit by the esteem and attachment I felt, I found on his table the corrected proofs of this very paper, and then reminded him of the promise he had made. In fact he sent me a copy soon after. Happily I had in readiness the materials necessary to render my answer in some degree interesting, and prevent my feeling the irksomeness commonly attendant on writings of this kind. After I had written my former paper, I had obtained a knowledge of this substance, at that time extremely scarce, and considerably so even at present, that enabled me to render my account of it far more complete: and this indeed I had for some time had an intention of doing. A pursuit however in which I was then engaged, and which I could not interrupt, did not allow me immediately to draw up the memoir I had in contemplation: and it was not till about the month of September, in the same year, 1808, that I was able to deliver it to the secretary of the Royal Society; expressing at the same time my wish, that it might be read as early as possible, in order that at least it might obtain a place in the first part of the Trasnactions

Farther knowledge of the compound sulphuret obtained by the author.

Account of it delivered to the secretary of the Royal Society.

Transactions for 1809. This I had the greater reason to hope, as the time when it was delivered was considerably before that, when the Royal Society recommenced its meetings. After these took place however, I found it impossible to get it read, notwithstanding I requested it repeatedly. The time passed on, and I could not but foresee, that it would have a powerful opposition to surmount in the committee of the Royal Society, under which it would probably sink. I could not however make any other use of the paper. The first part of the Transactions was already filled up, when at length I learned from Dr. Wollaston, that it had been read on the 4th of May. The time however still passed on, and nothing gave me reason to suppose, that the committee was taking any steps to have it printed. On this subject it preserved the profoundest silence, which I endeavoured to penetrate in vain. It was not till the 23d of June, when its vacation was nearly approaching, that I was informed of the fate, to which apparently it had been condemned from the beginning, by a letter from the committee, in which it was said, that, not deeming it expedient to print it at present, it was ordered to be deposited in the archives of the Royal Society.

Ordered to be placed in the archives of the Society:

Such are the reasons, that have hitherto prevented the publication of this paper, and at the same time have deprived it of the place it ought to have occupied. In fact it seems, that, since the critique of Mr. Smithson, as unbecoming as it was unfounded, obtained a place in the Transactions of the Royal Society, it could not without injustice refuse one of its members, whom it must have seen with regret to be the object of it, and who hitherto had been a zealous and approved coadjutor in its labours, the only indemnification he could receive, that of demonstrating the truth of his first assertions. I appeal to the Members of the Society themselves, who shall read this paper, and who know me sufficiently to do me the justice, I think, I deserve, whether I could on any account have expected this singular proceeding on the part of its committee.

but should have been published.

Endellion.

Part I.

Endellion not a simple ore of antimony,

When in the month of December, 1803, I presented to the Royal Society a paper on the triple sulphuret of lead, copper, and antimony, considered at that time as a simple ore of antimony, I thought it the more necessary, to fix the external characters of this substance, as Mr. Hatchett had just shown by his analysis of it, that, so far from being a simple ore of antimony, it was an ore composed of three sulphurets, those of lead, copper, and antimony, in which the latter was not even the principal metal.

but a triple sulphuret.

The character of the crystallizations could not at first be sufficiently established.

It was not in my power at that time however, to establish the character of the crystallizations of this triple sulphuret with all the precision, that the case required, and that I could have wished. This substance was then extremely scarce, as it is even now. The crystals I was able to procure being small, and with numerous sides, most of which were irregular, did not allow me to depend sufficiently on the measures I was able to take, to venture to fix in a peremptory manner the dimensions of its primitive crystal. All that I could then determine positively was, that this crystal was a rectangular tetraedral prism with square bases, but not a cube. In consequence I satisfied myself with establishing this truth, without settling the dimensions of the crystal. It necessarily followed, that the measures given as those of the angles of incidence between the primary and secondary faces, as they could not be the result of a calculation the base of which was not determined, must have been merely taken with the graphometer, and consequently to be considered as approximations only; yet as approximations having all the accuracy the instrument would admit, and the inaccuracy of which could not amount to one degree.

Primitive crystal.

Desirous from that period of giving a more complete account of this rare and interesting substance, and ascertaining at the same time in a more positive manner the form of its primitive crystal, I omitted no opportunity offered me of continuing to study it. For such opportunities, as I could

could not make them, I was under the necessity of waiting. At length I obtained the object I wished; I met with crystals, that I could measure with certainty, and had the satisfaction to find, not only that I could lay before the Royal Society more precise observations respecting the character of the crystallization of this substance, but besides a more complete and interesting series of its varieties of form, and a much more complete mineralogical account of every thing concerning it. I had at the same time the satisfaction to find, that the first measures I gave, which were simply taken from the crystals with the instrument, differed from those now established by calculation only in that slight degree, which may be ascribed to the unavoidable want of accuracy in the instrument; a difference amounting only to 30′ in one of the three varieties I formerly gave, to 19′ in a second, and to nothing at all in the third. This fact may serve as a proof of the near approach to accuracy obtainable by a little practice in using the instrument alone *.

More crystals of it obtained.

I shall now proceed to give a complete account of this substance, pursuing the method I adopted in my treatise on mineralogy, the first two volumes of which are just published.

By way of preliminary however I shall observe, that as all substances, beside the explanatory terms that point out their nature, and which are liable to change with the theory on which they are founded, require a proper name, invariable in itself, and fixing their existence among natural substances, I have given this the name of *endellion;* which avoids the termination in *ite*, so frequent in the nomenclature of mineral substances, and calls to mind, that the first specimens of this substance, which engaged the attention of mineralogists, came from Endellion, in Cornwall.

All substances require an appropriate name.

This named endellion by the author:

At the same time I avail myself of this opportunity, to testify

Bournonite by Jameson.

* *Additional note.* Since this paper was written, the measures obtainable by the instrument have acquired much greater precision by Dr. Wollaston's ingenious discovery of the reflective goniometer, a discovery of great importance to crystallography.

testify to Mr. Jameson how sensible I am of the flattering but unmerited honour he has paid me in his mineralogy, by giving this substance the name of bournonite.

Its characters.

Essential Characters of Endellion.

Crystallographical.

Primitive crystal. *Primitive crystal.* A rectangular tetraedral prism, the bases of which are square, and the height of which is to the side of the terminal faces in the proportion of 3 to 5.

Integrant molecule. *Integrant molecule.* I have yet met with nothing to induce me to adopt any particular opinion of the form, that belongs to the integrant molecule of this substance.

Fracture. *Fracture.* Irregular, and partially conchoidal. Made on crystals, and extending but a little way, it is perfectly conchoidal. Its lustre is very brilliant. Some of its accidental fractures exhibit more clearly than many of those made by art the direction of its laminæ parallel to the faces of its primitive tetraedral prism; but these traces are always faint, and seldom perfectly marked.

Physical.

Spec. gravity. *Specific gravity.* 5·775.

Brittleness. *Fragility.* This substance is very brittle. It is easily broken by the simple pressure of the nail.

Hardness. *Hardness.* The endellion scratches carbonate of lime with considerable facility, but not without breaking, on account of its great brittleness. Rubbed on paper it leaves a blackish brown mark. Its powder retains the metallic lustre.

Colour. *Colour.* Dark gray, and very shining, like that of polished steel, but a little more dark.

Chemical.

Very fusible. Exposed to the action of the blowpipe, it fuses the instant it is touched by the flame. It remains for some instants afterward in a fluid state; and, if it were fused in a spoon, it might be cast like melted lead, the fluidity of which in this state it even surpasses. A metallic regulus

Regulus. is easily obtained from it, of a dark gray colour, very brittle,

brittle, and the fracture is of a grain very fine and smooth.

Action of nitric acid on it.

Thrown into cold nitric acid, it dissolves pretty readily, and with effervescence. A real analysis is thus accomplished. The sulphur swims on the liquid, which holds in solution the copper and lead, and is of a green colour, and the oxide of antimony is precipitated in the form of a blue powder inclined to gray.

Component parts.

The endellion analysed by Mr. Hatchett gave for its component parts sulphur 17, antimony 24·23, lead 42·62, copper 12·8, iron 1·2: loss 2·15.

Eventual characters.

Phosphorescent.

Phosphorescence. Placed on a hot iron the moment it begins to lose its red colour, the endellion diffuses a blueish white phosphorescent light, the intensity of which appeared to me to vary in different specimens.

TABLE of the ENDELLION and its VARIETIES.

Varieties.

Species.	*Varieties.*	
Triple sulphuret of lead, copper, and antimony. Endellion.	Crystallized in a perfectly determinate manner.	Primitive crystal. Its modifications and varieties.
	In the compact state.	Pure. Mixed irregularly with sulphuret of zinc. Mixed irregularly with yellow sulphuret of copper and iron.

DESCRIPTION of the DIFFERENT VARIETIES of the ENDELLION.

Description.

Of a determined crystalline form.

Crystalline.

The perfectly determined crystalline form is that in which this substance has hitherto most commonly occurred. The surface of its crystals has a very shining lustre, which can be better compared to nothing than to the rhomboidal oxide of iron of the island of Elba, oligist iron of Haüy. This lustre however is exceeded by that of their fracture, when

when it is recent. The crystals are difficult to determine, both on account of the great number of faces they frequently exhibit, and of the irregularity of these faces. They consequently require a great deal of attention on the part of the observer, to be perfectly ascertained.

Where found. The endellion in a state of completely determined crystallization was first observed in Cornwall, in the mine of Huel-boys, in the parish of Endellion; and from this mine have been obtained the finest groupes of this substance that are seen in collections, where it is in general very rare. The endellion exists likewise in Siberia, where too it appears to be very scarce, as I know of but a single specimen, which is in my possession. I have seen in the shop of Mr. Maw several fragments of this substance, sent with other minerals from Brazil, the biggest of which was a very large single crystal, of the variety represented pl. vii,* fig. 8. Lastly I am indebted to Dr. Wollaston for some small fragments of the same substance, in which the endellion is in the compact state, mixed irregularly and very visibly to the eye with the double yellow sulphuret of copper and iron; and in which are observable little cavities, including very small but well defined crystals of the same substance, intermixed with minute specks of carbonate of lime and sulphate of barytes. These fragments came from Peru. The endellion, which I mentioned above as coming from Brasil, is in like manner mixed very perceptibly to the eye with yellow sulphuret of copper and iron.

The groupes of crystals of endellion from Cornwall are frequently accompanied with crystals of brown sulphuret of zinc; and in several sulphuret of antimony is likewise observed, commonly in fine needles, and frequently even capillary.

In the compact state.

In the compact state. The same pieces, that include crystals of this substance in Cornwall, include also parts more or less large, in which it is in the compact state. When this occurs, its fracture is thus

* The plates to this article are unavoidably deferred to our next number.

thus rendered irregular and granular, and its lustre is much inferior to that of the fracture of the crystals.

This compact variety in Cornwall is frequently mingled with sulphuret of zinc, which may easily lead to mistakes; and which Mr. Hatchett has very judiciously noticed in the analysis he made of this substance.

The compact endellion of Brasil and Peru also is intimately mixed with yellow sulphuret of copper and iron.

Description of the crystalline forms of endellion, and observations respecting them.

The primitive crystal of this substance, as I have already said, is a rectangular tetraedral prism, pl. vii, fig. 1, the height or side of which is to the sides of its terminal faces in the ratio of three to five*. I have not yet seen this crystal in its perfect state, that is to say, without the planes of any of the modifications belonging to it; and it cannot be obtained by splitting, the attraction of cohesion, that unites the integrant molecules of this substance, being too strong to be overcome. By means of some of the accidental fractures however, that occasionally exist, I have been able to discover the direction of its laminæ, and perceive that this direction, as well as that of the secondary faces, agree perfectly with the results of calculation. Primitive crystal.

I do not think however, that this prism is at the same time the form of the integrant molecule of this substance: but hitherto nothing has led me to form any particular opinion with respect to the form of this molecule. Integrant particle.

As the crystals of endellion are frequently loaded with facets, which the mineralogist may find embarrassing, I have thought it necessary, for the ease of the reader, to give separately, in fig. 4, the various retrogradations† experienced by the laminæ of the crystallization, which I have observed Various alterations of the primitive crystal.

* The method I have pursued for the determination of the primitive crystal will be seen hereafter.

† I give the name of retrogradation [*reculement*] to that act of crystallization, which has hitherto been known by the name of *decrement*, an expression that is totally false in many cases, as I have shown in the second volume of my Treatise on Mineralogy, p. 206, in the part relating to the theory of crystallization.

on

on the longitudinal edges of the primitive prism; in fig. 5, those I have observed along the edges of the terminal faces; and in fig. 6, those that I have observed at the angles of these faces. These three figures are intended for the same purpose of convenience, as those which, in my former paper on this substance*, were given solely with this view, and the exact models of which had not yet been observed in nature. My experience in crystallography has frequently led me to remark, that, when the crystals of a substance are liable to any considerable number of modifications, and at the same time actually undergo several of them, this method is extremely useful, and frees the mineralogist, who is desirous of ascertaining one of these crystals, from a task not unfrequently very troublesome. There are even substances, in which this method is very advantageous to the most expert crystallographer, and the present is one of them.

1st modification.

1st modification. The planes arising from this modification substitute for the longitudinal edges of the primitive prism a plane equally inclined to those contiguous to it. They are produced by the retrogradation of one row of the particles of the laminæ along these edges. These new planes are frequently striated, as is shown in fig. 2. Sometimes they cause the complete disappearance of the faces of the primitive crystal, giving rise to another prism, which is likewise a rectangular tetraedron with square bases, but secondary to the primitive prism; and in the crystals of this variety that I have seen the faces were constantly striated, as in fig. 3. This variety even led me into a mistake, when I wrote the first paper on this substance I presented to the Royal Society, by inducing me to consider the planes of the prism of the varieties represented at figs. 10, 11, 15, 16, and 17, as belonging to them: but a more attentive examination, elucidated by observations since made on a great number of other crystals, has taught me, that these striæ were the simple effect of aggregation, and that these same planes belonged to their primitives†.

I shall

* *Additional note*, —— and for which I was so unhandsomely reproved by Mr. Smithson, in his critique printed in the Philosophical Transactions.

† The striæ, that occur so frequently on the planes of crystals, are often

I shall not enter into any similar details concerning the other modifications, the number of each of these being affixed in each crystal to the planes belonging to it, and the table of these modifications, which will be annexed to this paper, pointing out in this respect at a single view particulars, which could scarcely be expressed by long circumlocution. In consequence I shall confine myself to the following observations.

All the varieties, from fig. 2 to fig. 20 inclusively, are met with among the fragments of this substance, that are brought from Cornwall. I have a very fine groupe of those represented at figs. 7 and 8, and a separate crystal of 8 and of 9. The variety fig. 8 is a regular aggregation, in the form of a cross, of two of the crystals fig. 7 elongated parallel to the planes of the 6th modification. It might also, and perhaps more justly, be considered as resulting from five crystals, similar to fig. 7, united by one of their planes. I have likewise crystals of the varieties 11, 12, 14, 15, and 19. With those at figs. 10, 13, 16, 17, and 18, I was furnished by two very fine groupes, and a superb single crystal, in the possession of Mr. R. Phillips. Fig. 16 answers to that numbered 17 in my former paper, which was not quite accurate. In 15 and 16 of the same paper the prism was much too thick, and they are replaced at present by 17 and 18.

Different varieties.

I have likewise the varieties from 21 to 26, in a very fine group, which, with the fragments from Peru and Brasil al-

often of very great use to indicate the direction of the laminæ of crystallization, and not seldom are they the only means, that the crystals of a substance afford. Thus the lenticular rhomboidal carbonate of lime pretty constantly indicates by its striæ the direction of the laminæ, and consequently that of the planes of the primitive crystal. In the hexaedral prism of the same substance, the same striæ point out the direction to be given to the fractures, on which Mr. Haüy has established the dimensions of its primitive rhomboid. But we must beware of the illusion, that may sometimes arise from striæ, which are indebted for their existence only to an aggregation of crystals, such frequent instances of which are exhibited by the tourmalin, thallite, sulphuret of antimony, &c.: an illusion by which I have shown I was at first misled myself with respect to the endellion, after having observed, that among its varieties there existed a rectangular tetraedral prism, the planes of which are frequently striated.

ready

ready mentioned, constitutes the only specimens I have yet seen from any place except Cornwall. The groupe, which assuredly is not English, was given to me as coming from Siberia. Its gangue is an irregularly crystallized quartz, part of which is of a dark blackish gray, in consequence of a mixture of very minute particles of sulphuret of lead, imperceptible to the naked eye. The crystals of endellion are covered by a slight stratum of green carbonate of copper; and some small crystals of common dodecaedral pyramidal carbonate of lime (*métastatique* of Haüy) are disseminated among them as well as in the quartz. Several small parcels of sulphuret of lead and blue copper are likewise observable on it.

Probably other varieties.

Such is the result, which the most careful examination, and continual attention to every thing, that could render me better acquainted with this scarce and interesting substance, enables me at present to lay before the Royal Society. I am far however from imagining, that I have seen every thing pertaining to its crystallization. Undoubtedly other varieties, and other modifications, may exist; and it is probable, that, among the small number of specimens of it in different collections, such may be found. From the numerous varieties, that exist in a single groupe of this substance, its primitive crystal appears to have a great tendency to be modified: but the modifications of this crystal, which I have given, are unquestionably sufficient, to render it easy to ascertain any new ones, if they should occur. These reflexions are not introduced here without reason. Among the different specimens of this substance examined by me, I have seen several crystals belonging to some of the varieties I have given, on which there existed likewise slight traces of planes belonging to other modifications, but which it was altogether impossible for me to determine. As an example of this I shall mention the crystal represented at Pl. VIII, fig. 27, not only because it is one of the most striking for elegance of form, but because it is in my own possession. The faces indicated by the letters x, y, and z, are certainly owing to an intermediate retrogradation at the angles of the terminal faces: but the impossibility of measuring with precision the angle of inclination between these planes and

the primitive ones in this crystal, which is very small, and partly imbedded in its gangue, completely prevents me from determining the nature of the three different retrogradations, to which they owe their existence.

(To be continued in our next.)

XIII.

On Detonating Silver. By Mr. Descotils*.

Fulminating silver.

Mr. Figuier, prof. of chemistry at the Pharmaceutical School at Montpellier, has lately written to the authors of this collection a paper on detonating silver, in which, after mentioning that Mr. Howard first formed this compound, which was afterward obtained in larger quantity by Mr. Cruickshank, he points out a process for preparing it analogous to that adopted by the latter gentleman.

Former paper on the subject.

A paper already published in this Journal † contains nearly similar results to those obtained by the professor of Montpellier, we shall therefore confine ourselves to the differences mentioned in his observations.

It explodes more readily than is usually supposed.

Mr. Figuier has seen the detonating silver explode even amid the acid solution in which it is formed, when touched by a hard body. He has likewise detonated this compound when dry by simple friction with the edge of a card. These facts indicate a much greater degree of inflammability than had been supposed, and must lead us to be more cautious in preparing this substance.

Not decomposed by weak sulphuric acid, unless in solution.

The professor has remarked, that detonating silver is not decomposed by weak sulphuric acid, unless it has been previously dissolved in water.

Nor by pure potash.

Caustic potash appeared to him merely to change its colour to a red, or a deep gray, without depriving it of its fulminating quality. This experiment, which I repeated, did not afford me precisely the same result. After remaining a considerable time in potash, the residuum gave only a slight decrepitation, arising no doubt from the portions, on which the potash had not yet acted.

* Annales de Chimie, vol LXIII, p. 104.

† Journal, vol XVIII, p. 140.

XIV.

XIV.

Process for making a fine Lake.*

Fine lake

A German chemist, whose name is not mentioned, has published the following process for making a beautiful lake.

by precipitating cochineal with solution of tin.

Take any quantity of cochineal, on which pour twice its weight of alcohol, and as much distilled water. Infuse for some days near a gentle fire, and then filter. To the filtered liquor add a few drops of solution of tin, and a fine red precipitate will be formed. Continue to add a little solution of tin every two hours, till the whole of the colouring matter is precipitated. Lastly, edulcorate the precipitate by washing it in a large quantity of distilled water, and then dry it.

XIV.

On the Blue Wolfsbane, by PHILIP ANTONY STEINACHER†.

Blue wolfsbane contains green fecula,

THE fresh leaves of blue wolfsbane, *aconitum napellus*, cultivated in a garden near Paris, being treated with a sufficient quantity of water at 45° [113° F.], green fecula was coagulated.

a gas,

The liquor separated from this fecula retained a peculiar herbaceous smell, analogous to that of the leaves of scurvy grass after the greater part of their pungency is destroyed by exposure to the open air. The progress of evaporation entirely dissipated it. Toward the end a matter of a granular form was separated. After this was washed and dried, a portion subjected to the action of the blowpipe on platina was not melted by the interior flame, but became whitish, without swelling or decrepitating.

an earthy matter, consisting of

carbonate

Another portion put into weak sulphuric acid produced a pretty long effervescence. The evaporation of the fluid afforded acidulous crystals in the form of soft needles, which were decomposed by nitrate of lead. The precipitate, heated red hot by the blowpipe on a piece of charcoal,

and phosphate of lime,

* Sonnini's Bibliothèque physico-économique, for 1808, vol. I, p. 553.

† Journal de Physique, vol LXVI, p. 234.

wat-

was reduced into little metallic globules, round which a slight aureola shone, accompanied with a very perceptible phosphoric smell. The extractive liquor boiled down contained a great deal of ammoniacal muriate. and muriate of ammonia.

As other plants gathered by the side of the wolfsbane yielded me no signs of phosphate when analysed, I conceive the organs of this plant have the faculty of assimilating phosphorus, or its elements, and converting them into an acid. No phosphate in other plants growing near it.

From my analysis it follows, that the aconitum napellus contains Summary of its contents.

Green fecula,

An oderant gaseous substance, which I suspect to be virulent,

Muriate of ammonia,

Carbonate of lime, and

Phosphate of lime.

Thus the existence of this phosphate in the blue wolfsbane, which Mr. Tutten of Wolfenbuttel announced nineteen years ago, is confirmed. The phosphate observed formerly.

SCIENTIFIC NEWS.

THE annual courses of popular lectures at the Surry Institution, Blackfriars Bridge, commenced on the 31st ult., and will be continued every succeeding Tuesday and Thursday evening, at seven o'clock, during the season. We understand, that the following gentlemen have been engaged for the respective departments, viz. Lectures at the Surry Institution.

Chemistry and Mineralogy, Mr. ACCUM.
Music, Mr. S. WESLEY.
Experimental Philosophy, Mr. JACKSON: and
Physiology (with Experiments), Dr. DAVIS.

To Correspondents.

I am not acquainted with any work on the subject after which E. H. inquires.

The papers of Mr. Barlow and Mr. Lyall will be inserted in our next number.

ERRATA.

P. 167, l. 4 from bot. *for* Pl. V, *read* Pl. VI.
168, l. 4 *for* complete *read* complex.

METEOROLOGICAL JOURNAL,

For OCTOBER, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker in the STRAND, LONDON.

SEPT. Day of	THERMOMETER.				BAROMETER,	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	9 A. M.	Day.	Night.
27	54	48	58	40	29·50	Rain	Rain
28	46	46	53	38	29·82	Ditto	Ditto
29	43	47	53	42	29·96	Fair	Fair
30	48	56	50	45	29·93	Rain	Ditto
OCT.							
1	51	52	56	49	30·14	Fair	Cloudy
2	56	58	60	55	30·28	Rain	Rain
3	56	53	61	55	30·32	Cloudy*	Cloudy
4	57	54	62	47	30·21	Rain	Fair
5	54	53	60	51	30·10	Ditto	Ditto
6	56	53	60	50	30·09	Ditto	Ditto
7	55	51	59	43	30·08	Ditto	Ditto
8	50	47	55	42	30·10	Ditto	Ditto
9	47	45	51	40	30·10	Ditto	Cloudy
10	45	45	49	41	30·04	Ditto	Ditto
11	47	45	50	41	30·02	Ditto	Fair
12	44	40	48	36	30·10	Ditto	Ditto
13	40	39	46	37	30·19	Ditto	Ditto
14	39	44	49	35	30·26	Ditto	Ditto
15	37	44	48	40	30·32	Ditto †	Cloudy
16	47	53	55	41	30·14	Cloudy	Ditto
17	54	55	58	52	30·11	Ditto	Fair
18	54	56	58	54	30·09	Rain	Cloudy
19	55	57	59	52	30·17	Ditto	Rain
20	53	53	55	51	30·18	Cloudy	Cloudy
21	52	53	55	50	30·15	Ditto	Ditto
22	52	52	55	50	30·10	Ditto	Ditto
23	53	53	56	48	30·00	Ditto	Fair
24	51	54	58	49	29·91	Rain	Ditto
25	51	53	55	50	30·17	Ditto	Ditto
26	53	56	61	48	30·28	Ditto	Ditto

* Day gloomy and close.

† Heavy fog.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1809.

ARTICLE I.

On Vegetable Astringents. By John Bostock, *M. D.* *Communicated by the Author.*

(Continued from page 222.)

AMONG the constituents of galls we always find mucilage enumerated, and Mr. Davy gives a process for obtaining it in a separate state, but I confess, that I am not altogether satisfied with the force of the arguments, by which its existence is thought to be proved. Mr. Deyeux, who I believe first distinctly mentioned the existence of mucilage in galls, founded his opinion upon an erroneous supposition, that no substance except mucus is capable of producing mould. The moulding, as has been shown above, evidently depends upon the other constituents of the infusion*. With respect to the tests for mucilages, the only one which can be considered as applying generally to them, and

Mucilage,

not the only substance that grows mouldy.

Tests not applicable here.

* As a farther proof of this position I may remark, that I have observed the process of moulding in Mr. Hatchett's artificial tan.

and which acts upon them when not in a concentrated state, is the acetate of lead; but this unfortunately cannot be applied in the present instance, because it is equally affected by tan and the gallic acid. The other tests which I found in my former experiments on this subject* to act upon particular varieties of mucilage, such as the nitrate of mercury, the oxisulphate of iron, the nitromuriate of gold, and silicated potash †, were each of them limited to those varieties, and can therefore be of no use in determining the general question, beside that some of them act upon the other constituents of galls. There is, however, one property, in which all mucilages seem to agree, i. e. their insolubility in alcohol; and it is upon this property, that Mr. Davy has founded his operation for obtaining the mucus of galls in a separate state.

All mucilages insoluble in alcohol.

There appears to be no mucilage in galls.

I endeavoured to imitate his process, but without success. A strong infusion of galls had its tan separated by jelly, the residual fluid was evaporated, and its solid contents were boiled in alcohol, in order to remove from them any extract or gallic acid. What was left was digested in warm water; a very small quantity of it seemed to be dissolved, and the fluid assumed a light green hue. The acetate of lead threw down a slight precipitate, and left the fluid colourless; it was tinged by the oxisulphate of iron; tartarised antimony and oxalic acid had no effect upon it; it was neither acid nor alkaline; being slowly evaporated, a small gray residuum was left, which did not resemble mucilage in any of its physical properties. We come to the same conclusion respecting the existence of mucus in galls by digesting a quantity of the powder in successive quantities

* Nicholson's Journal, XVIII, 28.

† I am induced to consider the precipitate which is produced by the addition of silicated potash to gum arabic, a fact which was first noticed by Dr. Thomson (a), as depending, not upon the immediate action of silex upon gum, but upon the lime which enters pretty largely into its composition, and which causes oxalic acid to throw down a copious precipitate from it. When silicated potash is added to the different vegetable infusions, the same effects seem to ensue as from the employment of the alkali without the silex.

(a) Chemistry, V, 40.

of

of alcohol or ether; in both these cases, after the action of these fluids has been carried to its fullest extent, a residuum is left, upon which water has no action, yet mucilage is insoluble both in alcohol and in ether. I feel it necessary to apologize for differing from Mr. Davy on this point of fact, but I may say in my excuse, that he relates the process for obtaining the mucus of galls rather as one calculated to answer the end in view, than as what he had really put in practice. That portion of the galls, which in his analysis he attributed to mucus, I should refer principally to the imperfect compound of tan and jelly, which I have described above.

I shall now make a few observations on the chemical properties of catechu; but it is necessary to premise, that the varieties in this substance are even greater than those in the gall nut. That which I employed was considered by a friend, on whose judgment I could rely, as a good specimen of the kind which is most esteemed by the apothecaries; yet from my experiments with it, it seemed to differ from that employed by Mr. Davy. Cold water being digested upon it for two days took up $\frac{1}{10}$ of its weight; the solution was transparent, and of a fine reddish brown colour; the portion which remained undissolved seemed like a mixture of white and red particles, in which the white considerably predominated, but when it was dried its colour became as deep as that of the catechu in its recent state. The solution slightly reddened litmus; it was rendered turbid by the oxalate of ammonia, and a small quantity of a dense precipitate subsided from it. It was also liable to the operation of moulding, although not so readily as the infusion of galls. When catechu is treated with hot water, it is partly dissolved and partly suspended. An opake infusion is formed, which contains about $\frac{1}{10}$ of its weight of solid matter. The warm infusion still continues quite opake after being passed through a paper filter, while the filter gains a great addition of weight, and is stiffened as if it had been soaked in some kind of mucilaginous matter. By standing for some days, a part of the contents is deposited, and the warm infusion becomes transparent. If the clear solution be evaporated, the residue is not capable of being

Catechu very variable in its qualities.

Treated with cold water,

and with hot.

completely redissolved, and the second infusion is rather lighter coloured than the former one. The transparent solutions of catechu, whether formed by warm or by cold water, slowly deposit a part of their contents, in the form of the whitish residuum mentioned above, while at the same time a kind of efflorescence creeps up along the sides of the glass to some distance above the surface of the fluid. This deposition proceeds the more rapidly, the stronger is the infusion; but there does not appear to be any absolute limit to its continuance. In one instance I found, that a saturated solution of catechu, after standing two months, had lost rather more than half of its solid contents, but a part of it had been expended in forming a stratum of mould. The substance that has been deposited is less soluble in water than the recent catechu, but it dissolves readily by an increase of temperature; it forms a solution of a *lighter* colour, and it has less disposition to separate from the fluid.

The transparent solutions slowly deposit part of their contents,

and grow mouldy:

Requires successive infusions.

Although catechu is so readily soluble in water, yet, as is the case with galls, it requires a number of successive infusions to separate the soluble part from the small insoluble residue. Ten grains of catechu were infused in 50 times their weight of water for 24 hours; the fluid was then drawn off, and the same quantity of water was poured upon the residue. After 9 of these successive infusions, the effect of the oximuriate of tin was no longer visible, that of jelly was barely so, but the oxisulphate of iron continued to tinge the fluid until the 15th infusion, and at this period the acetate of lead produced a very slight cloud. The insoluble residue left was not more than $\frac{1}{48}$ of the weight of the catechu employed; it seemed to be a heterogeneous mass, consisting probably of accidental impurities, and it may be expected therefore to vary in quantity. Mr. Davy found no less than $\frac{1}{14}$ of the catechu upon which he operated to consist of insoluble matter *.

Treated with alcohol.

Alcohol, at the temperature of the atmosphere, slowly dissolves catechu. By boiling the effect is much promoted, and the alcohol takes up about $\frac{1}{10}$ of its weight, which remains permanently dissolved, but the quantity varies very much

* Phil Trans. 1803, p. 259.

much in different specimens. About $\frac{1}{4}$ of the catechu seems insoluble in this menstruum. This part was readily taken up by water, except a small dark-coloured residuum; the solution produced only a slight effect upon jelly and the oximuriate of tin, but by the oxisulphate of iron the whole became as it were coagulated, and was converted into a gray mass. The acetate of lead also threw down a very copious precipitate from the fluid. These properties denote a considerable analogy between this part of the catechu, and the mucilaginous bodies, an analogy which is farther strengthened by a degree of viscidity, which may be observed in its solutions. The substance obtained by evaporating the spirituous solutions of catechu is of a deep red colour, soluble in water but less so than the whole catechu; the solution is copiously precipitated by jelly, by the oximuriate of tin, and the oxisulphate of iron. It moulded by exposure to the atmosphere, I think, rather more readily than the entire catechu.

Action of reagents on the infusion.

The infusion of catechu is very copiously precipitated by jelly, but a part of the precipitate generally remains suspended in the fluid. The oximuriate of tin also acts powerfully upon catechu, but it is not much affected by tartarized antimony, it is rendered opake, and the brown colour is changed to red, but scarcely any precipitate is formed. The acetate of lead exercises the same instantaneous action on catechu as on galls; it immediately unites with all the constituents of the infusion, and leaves the fluid perfectly transparent and colourless. The nitromuriate of gold throws down a very copious precipitate of a blackish purple colour, and the nitromuriate of platina an equally copious one of a deep reddish brown. The precipitate produced by the oxisulphate of iron is of a deep olive green, and readily subsides from the fluid. This precipitate must, I apprehend, be considered as an obvious indication of a small quantity of gallic acid; and may therefore be regarded as a proof of the variety, which exists in different species of this substance, since that which Mr. Davy employed was without this constituent*. I have always found the infu-

* Philos. Trans. 1803, p. 269.

sions

completely redissolved, and the second infusion is rather lighter coloured than the former one. The transparent solutions of catechu, whether formed by warm or by cold water, slowly deposit a part of their contents, in the form of the whitish residuum mentioned above, while at the same time a kind of efflorescence creeps up along the sides of the glass to some distance above the surface of the fluid. This deposition proceeds the more rapidly, the stronger is the infusion; but there does not appear to be any absolute limit to its continuance. In one instance I found, that a saturated solution of catechu, after standing two months, had lost rather more than half of its solid contents, but a part of it had been expended in forming a stratum of mould. The substance that has been deposited is less soluble in water than the recent catechu, but it dissolves readily by an increase of temperature; it forms a solution of a lighter colour, and it has less disposition to separate from the fluid.

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much in different specimens. About ¼ of the catechu seems insoluble in this menstruum. This part was readily taken up by water, except a small dark-coloured residuum; the solution produced only a slight effect upon jelly and the oximuriate of tin, but by the oxisulphate of iron the whole became as it were coagulated, and was converted into a gray mass. The acetate of lead also threw down a very copious precipitate from the fluid. These properties denote a considerable analogy between this part of the catechu, and the mucilaginous bodies, an analogy which is farther strengthened by a degree of viscidity, which may be observed in its solutions. The substance obtained by evaporating the spirituous solutions of catechu is of a deep red colour, soluble in water but less so than the whole catechu; the solution is copiously precipitated by jelly, by the oximuriate of tin, and the oxisulphate of iron. It moulded by exposure to the atmosphere, I think, rather more readily than the entire catechu.

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* Philos. Trans. 1803, p. 269.

sions

sions which I formed to be slightly reddened by litmus. After the infusions of catechu have undergone the operation of moulding, they are much less affected both by jelly and by the oximuriate of tin, but I never carried the process so far as to observe whether they could be entirely deprived of the capacity of being acted on by these reagents.

Unsuccessful attempts to separate the tan and extract of catechu.

According to Mr. Davy's observations the separation of the tan and extract of catchu may be accomplished with a considerable degree of accuracy, and he points out three different ways in which this may be effected. Tan, he remarks, is more soluble in water than extract, if therefore catechu be subjected for a short time to a small quantity of water, the tan alone will be dissolved, and the residue will contain a greater proportion of extract. I infused a portion of catechu for a few minutes in about ten times its weight of water, by which a part only was dissolved. The residue was afterward dissolved by the addition of more water, and when each of the infusions was become clear, by depositing a part of their contents, they were both of them submitted to the action of jelly and the oximuriate of tin; the first infusion was stronger, but I could not observe the least difference in the proportional effects of the two reagents. Mr. Davy's 2d method of separating tan from extract is founded upon the principle, that extract is more soluble in warm than in cold water, and therefore if a saturated warm infusion be formed, when it cools the tan will remain dissolved, while the greatest part of the extract will be deposited. I put this process into execution, but upon applying the two reagents they both seemed to act in an equal degree, differing only in their effects in consequence of the matter which was deposited being rather less soluble than the entire catechu. The 3d method of separating the tan from the extract is by forming a number of successive infusions, when it is said, that the tan will become first exhausted, and the extract be left in a state of almost perfect purity. I have already related the result of this operation, which was not at all conformable to the above statement. These circumstances I regard as amounting to a positive proof of an essential difference between the substances, which were employed by Mr. Davy and myself.

In

In my former paper I mentioned, that I had performed some experiments on the extract of rhatany, which led me to conclude, that it consisted principally of tan. It readily dissolves in water, and the solution is much promoted by an increase of temperature; as the water cools, a part of the rhatany separates, leaving about $\frac{1}{70}$ of the weight of the fluid in permanent solution. The fluid very slightly reddens litmus, and after some time shows a tendency to mould. The part that is deposited from the solution by cooling does not appear to be different from what is retained by the water, except that it contains a small insoluble residuum, which I am disposed to regard as an accidental impurity, and from which it requires a number of successive infusions entirely to separate the soluble part. That part which subsides from the warm infusion is also less soluble than the entire extract; but this I attribute rather to the effect of the operation, than to any original difference in its nature. Alcohol takes up about $\frac{1}{70}$ of its weight of the extract, the solution is promoted by heat, it requires several successive applications to remove all the soluble matter, and a portion is left, upon which the alcohol has no longer any effect. This part is readily dissolved by water, and forms a solution, which is of a bright red colour, which was rendered slightly turbid by jelly and the oximuriate of tin, but was very copiously precipitated by nitromuriate of gold and the acetate of lead, the former producing a reddish brown, and the latter a delicate pink precipitate. The results are very similar to what has been related above respecting the action of alcohol upon catechu, and indicates the presence of a substance, which in its chemical characters bears an analogy to mucus; at the same time it must be remarked, that the solutions of rhatany are free from any degree of viscidity.

Rhatany treated with water assisted by heat:

and with alcohol.

Part insoluble in alcohol.

Rhatany acts very powerfully upon jelly, forming with it a light red precipitate, which generally separates from the fluid. It appears, that the most perfect compound is produced by about equal parts of prepared isinglass and extract, but the substances do not unite in the same definitive proportion, the nature of the compound being much influenced

Action of reagents on it.

fluenced by the relative quantities in which its ingredients are presented to each other. When there is an excess either of jelly or of tan, the precipitate subsides more slowly, and is of a softer texture. Beside jelly, rhatany is precipitated by oximuriate of tin, oxisulphate of iron, acetate of lead, tartarized antimony, nitromuriate of gold and of platina, alum, lime, and sulphuric acid. The oximuriate of tin throws down a dense precipitate, but only in moderate quantity, much less than that produced by jelly; the oxisulphate of iron produces a black precipitate, which speedily subsides to the bottom of the vessel; acetate of lead instantly combines with all the contents of the solution, throws them down in the form of a pink mass, and leaves the fluid transparent and colourless; antimony throws down a small quantity of a reddish powder; the nitromuriate of gold produces a very copious dark purple precipitate, and the nitromuriate of platina an equally copious one of a reddish brown colour. Alum renders the solution turbid, changes its colour to a dirty brown, and throws down a small quantity of precipitate; lime water heightens the colour, and produces a red precipitate; and sulphuric acid produces a copious precipitate of a light red colour. Carbonate of potash converts the colour of the solution to a deep blood red, but produces no farther effect. After the infusion of rhatany has had jelly added until no further precipitation is produced, the oximuriate of tin renders it slightly turbid, but can scarcely be said to form a precipitate; if however, the experiment be reversed, i. e. if jelly be added to the infusion after the action of the oximuriate of tin, a copious precipitate is thrown down.

Is tan always identical?

It has been a much agitated point, whether tan be in all cases uniform in its properties, or whether there may not be substances poſſeſſed of the leading characteristics of tan, particularly its property of precipitating jelly, which yet, in some respects, may differ from each other. This latter opinion has been adopted by Proust; while Mr. Davy, on the contrary, appears inclined to attribute any diversity of operation on the different reagents, not to any difference in the tan itself, but to the peculiar substances with which it may be

be united. I confess that I am disposed to adopt the doctrine of Proust: for although Mr. Davy's remark be correct, that in all vegetables, in which tan has been discovered, it exists in a state of combination with other principles, and that its action must necessarily be modified by these combinations; yet I conceive, that, as far as we are able to judge, the nature of the combinations will not account for the difference of the effects. The extract of rhatany is copiously precipitated by jelly, and considerably so by the oximuriate of tin; but as this reagent produces scarcely any effect after the addition of jelly, we must conclude, according to the generally received opinion, that the effect of both these substances depends upon the tan which it contains, so that we are led to regard it as consisting of tan, combined with a little mucilage and a minute portion of gallic acid. Yet we find, that tartarized antimony and the carbonate of potash, which act so powerfully upon the tan of the gall-nut, scarcely produce any precipitate with the tan of rhatany. We must therefore conclude, either that the action of the oxide of antimony and the carbonate of potash depends upon the presence of some extraneous body, or that there may be a substance, which forms an insoluble compound with jelly, and which, on this account, is entitled to the appellation of tan; but which may be so modified, as in some states to unite with the above reagents, and at other times to have no effect upon them. Considering the magnitude of the effect produced, compared to the supposed nature of these extraneous bodies, I cannot but think the latter opinion the more probable. This, it is admitted, is no more than a presumptive argument; but I apprehend, that the same point is more firmly established by what I have observed respecting Mr. Hatchett's artificial tan. This substance we may regard as homogeneous, and therefore not liable to those objections, which apply to such experiments as are performed upon any of the vegetable infusions; and yet I have found it to act very differently upon other reagents, at the same time that it exercised the most powerful action upon jelly. I have now before me a solution of the artificial tan, which copiously precipitates jelly, the oximuriate

Probably not.

Artificial tan.

muriate of tin, the oxisulphate of iron, the acetate of lead, alum, lime water, and sulphuric acid; and yet it is very slightly affected by tartarized antimony, and not in the least by the carbonate of potash. Are we then to conclude, that pure tan, such as may be supposed to exist in Mr. Hatchett's preparation, has no affinity for the oxide of antimony and the carbonate of potash; and that, when the tan of the gall-nut is precipitated by these reagents, it depends upon a primary action, which they exert upon some other constituent? or that there may be substances, which have some specific differences, although, from their leading properties, they may be all of them strictly entitled to the generic name of tan? The coincidence between Mr. Hatchett's tan and rhatany, so far as the reagents are concerned, might seem to favour the former opinion; yet the latter supposition implies nothing that is improbable, and is agreeable to the analogy, which prevails in other vegetable productions.

With respect to any general conclusions, that I may draw from my experiments on these different vegetable astringents, I feel so well aware of the difficulty of obtaining unexceptionable results, and the uncertainty of the inferences that ought to be deduced from them, that I shall not venture to consider the positions which I have advanced as ascertained matters of fact, but rather as subjects for future investigation. All that I can expect from this paper is, that it will serve as an addition to the store of information which is daily accumulating, and which may assist at some future period in laying the foundation of a more matured theory, than any which could be constructed at present.

Liverpool, October 10, 1809.

II.

II.

Memoir on the triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. LE COMTE DE BOURNON, *F. R. and L. S.*

(Continued from page 237.)

Determination of the primitive crystal of endellion.

FROM the direction of the laminæ of crystallization of the endellion, with which some accidental fractures had made me perfectly acquainted, as well as from the general aspect of the crystals of this substance, I could not doubt, that the form of its primitive crystal was a rectangular tetraedral prism; and the similitude of the retrogradation of the crystalline laminæ along the edges of the terminal faces, indicated by the equality of the inclinations of the faces belonging to them, made me presume, that these faces must be squares. Accordingly it appeared to me, that the primitive crystal could only be a cube, or a rectangular tetraedral prism with square bases, the altitude of which would be greater or less than the side of the terminal faces. Primitive crystal ascertained.

Now remarking, that, with very few exceptions, in all substances, that have a perfectly symmetrical solid for their primitive crystal, as the cube, rhomboid, octaedron, regular tetraedron, &c., the secondary forms, produced by the various retrogradations to which they may be subjected, retain the same symmetry; I observe, that, in the crystals belonging to the secondary faces of the endellion, there is no symmetry between the planes that supply the place of the edges of the terminal faces, and those of the longitudinal edges of the prism, either with respect to the number of these planes, or to their inclination; whence I am naturally led to infer, that the rectangular tetraedral prism, the primitive crystal of endellion, is not a cube. Of this I was fully convinced, when I presented my first paper on this substance to the Royal Society; and it was this, that then prevented me from giving the dimensions of its rectangular tetraedral Where a primitive crystal is symmetrical, the secondary crystals are so likewise.

muriate of tin, the oxisulphate of iron, the acetate of lead, alum, lime water, and sulphuric acid; and yet it is very slightly affected by tartarized antimony, and not in the least by the carbonate of potash. Are we then to conclude, that pure tan, such as may be supposed to exist in Mr. Hatchett's preparation, has no affinity for the oxide of antimony and the carbonate of potash; and that, when the tan of the gall-nut is precipitated by these reagents, it depends upon a primary action, which they exert upon some other constituent? or that there may be substances, which have some specific differences, although, from their leading properties, they may be all of them strictly entitled to the generic name of tan? The coincidence between Mr. Hatchett's tan and rhatany, so far as the reagents are concerned, might seem to favour the former opinion; yet the latter supposition implies nothing that is improbable, and is agreeable to the analogy, which prevails in other vegetable productions.

With respect to any general conclusions, that I may draw from my experiments on these different vegetable astringents, I feel so well aware of the difficulty of obtaining unexceptionable results, and the uncertainty of the inferences that ought to be deduced from them, that I shall not venture to consider the positions which I have advanced as ascertained matters of fact, but rather as subjects for future investigation. All that I can expect from this paper is, that it will serve as an addition to the store of information which is daily accumulating, and which may assist at some future period in laying the foundation of a more matured theory, than any which could be constructed at present.

Liverpool, October 10, 1809.

II.

II.

Memoir on the triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. LE COMTE DE BOURNON, F. R. and L. S.

(Continued from page 237.)

Determination of the primitive crystal of endellion.

FROM the direction of the laminæ of crystallization of the endellion, with which some accidental fractures had made me perfectly acquainted, as well as from the general aspect of the crystals of this substance, I could not doubt, that the form of its primitive crystal was a rectangular tetraedral prism; and the similitude of the retrogradation of the crystalline laminæ along the edges of the terminal faces, indicated by the equality of the inclinations of the faces belonging to them, made me presume, that these faces must be squares. Accordingly it appeared to me, that the primitive crystal could only be a cube, or a rectangular tetraedral prism with square bases, the altitude of which would be greater or less than the side of the terminal faces. Primitive crystal ascertained.

Now remarking, that, with very few exceptions, in all substances, that have a perfectly symmetrical solid for their primitive crystal, as the cube, rhomboid, octaedron, regular tetraedron, &c., the secondary forms, produced by the various retrogradations to which they may be subjected, retain the same symmetry; I observe, that, in the crystals belonging to the secondary faces of the endellion, there is no symmetry between the planes that supply the place of the edges of the terminal faces, and those of the longitudinal edges of the prism, either with respect to the number of these planes, or to their inclination; whence I am naturally led to infer, that the rectangular tetraedral prism, the primitive crystal of endellion, is not a cube. Of this I was fully convinced, when I presented my first paper on this substance to the Royal Society; and it was this, that then prevented me from giving the dimensions of its rectangular tetraedral Where a primitive crystal is symmetrical, the secondary crystals are so likewise.

tetraedral prism, as I could not sufficiently depend on the angles of incidence, which the secondary faces, I had then seen, enabled me to take; accordingly I contented myself with giving a very near approximation to the measure of these angles, without subjecting them to the scrutiny of calculation *.

Mode in which the primitive crystal was ascertained.

Circumstances having since enabled me to acquire perfect certainty with respect to the measures of these angles, it now remains for me to determine the dimensions of the tetraedral prism. To effect this, after recognizing four different retrogradations along the edges of the terminal faces of the prism, as well as four others at the angles of the same faces, I observe, that the dimensions of the crystal may be determined either by the first of these retrogradations, or by the second: I observe too, that each will serve reciprocally as a support or confirmation of the other.

Directing my attention in the first place to the four retrogradations, that take place along the edges of the terminal faces, I begin by taking as accurately as possible the angle formed by the planes arising from each of these retrogradations with the terminal faces of the prism, and find one angle about 130°, another about 135°, a third about 149°, and the fourth between 171° and 172°. I then draw a horizontal line, A B C, fig. 28, representing one of the edges of the terminal face of a crystal perfectly resembling the primitive one, but composed of a certain number of crystalline molecules united, and the equal divisions of

* *Additional note.* It was not from omission therefore, but for valid reasons, that I did not give the cube as the primitive crystal of this substance. Mr. Smithson, to whom I am very well known, might have done me the justice to suppose, that, if the determination of this crystal, from the facts I could then observe, had been as simple as his calculation indicates, my eyes had too much experience in crystallography for it to have escaped me. If however he had entertained any doubts on this point, he was sufficiently acquainted with me, to have communicated them to me in a less hostile manner; when I would with great pleasure have submitted to him the reasons, that had determined me to act as I did. By this science would have lost nothing, and I should have gained much, in probably not experiencing the extraordinary, and I will boldly say unmerited conduct, that has been held toward me in the name of the Royal Society, of which I am proud to call myself a member, and for which I shall always feel the highest respect.

which

Mode in which the primitive crystal was ascertained.

which at the points A, T, B, C represent the extremities of the several edges of the component crystals. From the extremity C of this I let fall the perpendicular Ch, representing the direction of the side, or altitude, of the prism, and the length of which I leave undetermined. Through the extremity of this line, C, I draw the lines G C P, F C O, E C N, D C M, forming with it angles of 130°, 135°, 149°, and 171° 30′, which I produce indefinitely above the point C. From the point B, the extremity of the side of the first molecule, I erect the indeterminate perpendicular B G, cutting all the preceding lines. It is evident, that the lines C G, C F, C E, and C D, will indicate the direction of the planes derived from the four different retrogradations, that take place along the edges of the terminal faces; and that, if one of them be made by a single row, the part of the perpendicular B G, included between the line of the direction of the plane derived from this retrogradation and the line A C, will represent the height of the molecule of the last lamina placed on it, and consequently that of the primitive crystal.

It remains now to inquire, which of these retrogradations was most probably made by a single row; and whether, after having determined this, all the others will agree with it. The angle F C B, or that of inclination between F C and A C, being 45°, or the supplement of B C O, which was by construction 135°, would indicate a height equal to the edges of the terminal faces, and consequently the cube as the primitive crystal; and the observation already made militates against the choice of this, unless the farther observations, in which we are engaged, oblige us to adopt it. The angle of inclination, G C B, of the line G C, would indicate a height greater than that of the cube; and in all the crystals of this substance the longitudinal edges of the prism being constantly shorter than those of the terminal faces, I am led rather to reject than adopt this height, which would give 28·6, the edges of the terminal faces being supposed 24. The choice then remains between the two retrogradations represented by the two lines E C and D C, the first indicating E B for the height, and the second D B; and the resolution of the two rectangular triangles

E B C

Mode in which the primitive crystal was ascertained.

E B C and D B C will very readily give the value of these two lines with respect to the edge of the terminal faces represented by B C, and which we have already supposed to be 24. But as the height D B, which then would be about 3·58, would not agree with the inclination of the planes belonging to the other statements, the choice cannot remain doubtful, it falling necessarily on E B, which is of 14·4, and consequently to the edge of the terminal faces in the ratio of 14·4 to 24, or of 3 to 5. And in fact, on fixing at this the height of the primitive rectangular tetraedral prism, the determination of the other statements by calculation agrees perfectly with the inclination found by measuring the planes arising from them. Besides, the height D B of 3·58 would be much too small with respect to what all the crystals of this substance exhibit; while on the contrary that of 14·4 agrees with every thing found by observation in these crystals.

It remains now to examine, whether the retrogradations at the angles of the terminal faces agree with this height; and, if they should not agree with it, whether they do not point to one more natural, and more fit to be adopted. To proceed on this examination, I take with the instrument, as accurately as possible, the angle of incidence between the terminal faces and the four planes which take the places of their angles. Their measurement gives me 125° for one, between 134° and 135° for another, between 150° and 151° for the third, and about 172° for the fourth.

The terminal faces being a perfect square, fig. 30, and the side of the square being assumed 24, the diagonal R S is 33·94, and consequently its half is 16·97.

As every retrogradation at the angles of a polygon is made on the diagonal passing through these angles, if we suppose the primitive rectangular tetraedral prism, of which fig. 30 represents the terminal face, cut by a plane passing through the diagonal R S and that which is opposite to it in the lower face, all the diagonals of the molecules of the superficial lamina, on which the retrogradation is made, as well as of those superimposed on it, will be placed on the diagonal R S, or parallel to it. Let Q S, therefore, fig. 29, representing this diagonal, be drawn horizontally, and divided

divided at the points V W into equal parts, which shall be to those of the side A C, fig. 28, of the terminal faces, in the ratio of 33·94 to 24: these divisions representing the diagonals of the crystalline molecules placed on the whole diagonal Q S. From the point S, the extremity of the line Q S, draw the lines $g\,S\,r$, $e\,S\,a$, $q\,S\,d$, and $m\,S\,f$, so as to make with this line angles of 125°, 134° 30′, 150° 30′, and 172°, and produced indefinitely above the point S. The lines S g, S e, S q, and S m, will represent the direction of the planes produced by the four different retrogradations, that take place at the angles of the terminal faces of the primitive prism. As every retrogradation, that takes place at the angles of crystals by diagonals, is equivalent in the effect it produces to a retrogradation that takes place simply by semidiagonals; to find the height, which that of the four that takes place only by a single row would give, in order to see whether it would accord better with nature than that of 3 to 5 given by the observations that have been made on the retrogradation along the edges of the terminal faces; from the point R, half of the diagonal W S, erect the perpendicular R g, cutting the four lines S g, S e, S q, and S m, representing the directions of the substituted planes. Inquiring now whether any of these planes may be produced by the simple retrogradation of a single row, I perceive immediately, that for the same reason as was given respecting the retrogradation along the edges of the terminal faces, those planes must be excluded, the direction of which is represented by the lines e S and g S. There remain then those denoted by the lines q S and m S. For the same reason likewise as was given before, that which answers to the direction m S cannot be adopted; consequently our choice is confined to that in the direction q S. The resolution of the rectangular triangle q R S would give 9·6 for the height of the molecule, the side of the terminal faces being still supposed 24: so that this height would be to the side in the ratio of 9·6 to 24, or of 2 to 5. Observing then, that the result of the calculation made with the ratio of 3 to 5 agrees better with what the inclination of the secondary faces of the endellion exhibits in nature, than that made with the latter ratio: remarking too, that the same

Mode in which the primitive crystal was ascertained.

same ratio of 3 to 5 accords better with the customary dimensions of the crystals of this substance, almost all these crystals exhibiting this proportion between their height and breadth, while I have not yet found one in the proportion of 2 to 5: in determining the ratio of the height, or side of the primitive prism to the side of its terminal faces, I fix on the proportion of 3 to 5, to which I was before guided by the observation of the retrogradations that take place along the edges of the terminal faces. In consequence I conclude, that the primitive crystal of endellion is a rectangular tetraedral prism, the height of which is to the edges of the terminal faces in the ratio of 3 to 5.

The determination of a primitive crystal, with sufficient data, a simple process.

I have not hesitated to give with considerable minuteness the method I pursued in determining the primitive crystal of this substance; in the first place because it renders the Royal Society better acquainted with the grounds on which it is established, and shows, that this determination is by no means the result of an opinion adopted at first sight, or of a slight observation of a single crystal merely: and secondly, because these details show how simple and easy such determinations are, when nature supplies us with sufficient data, and at the same time how far the calculations they require are from being complicated*. The same may be said of the calculations for determining the planes produced in crystals by retrogradations of the crystalline laminæ: they never require any thing more than the resolution of a triangle, for which there are always sufficient data. I think I may affirm, that, by means of the method given in my Treatise on Mineralogy; and the use of the protractor with a movable radius, which I have likewise made known, and which greatly abridges the trials we are sometimes obliged to make, for determining from the angles of incidence of the secondary planes of the crystals the nature of the retrogradations calculated to give rise to them; there exists no

* *Additional note.* This method having never yet been given in any work on mineralogy in so simple and easy a manner, and besides the Philosophical Transactions hitherto containing little on the subject of crystallography were farther inducements for me to enter into these particulars. To me it appears, that they cannot but render this paper more interesting.

science,

science, the application of which is more easy, than crystallography.

From the angle of incidence of 135° between the terminal faces and one of the planes that are substituted for the edges of those faces in the rectangular tetraedral prism of the endellion; from that nearly of the same number of degrees, which one of the planes substituted for their angles makes with the same faces; and lastly, from another of 135°, which one of the planes substituted for the longitudinal edges makes with the sides of the prism; we may be very easily led, if we confine our observations to these facts, to consider the primitive crystal as a cube. These angles of incidence however, either exact, or so near it that the instrument cannot detect the difference, may be produced by retrogradations of a number of rectangular tetraedral prisms by no means of a cubical figure. I have assigned the reasons, which have appeared to me to militate against our acceding to this first attempt. I am persuaded we much too readily yield to an inclination to consider as cubical the primitive crystals of substances, the secondary forms of which indicate a rectangular tetraedral prism. There are already a sufficient number of substances, that really have the cube for their primitive crystal, though their integrant molecules are of a different form, without our enlarging it unnecessarily. By doing thus we afford an additional handle to those persons, who, seeing in every primitive crystal nothing but the form of the integrant molecules, from which however it is frequently very remote, make of the numerous repetitions of this form in several mineral substances, that are totally different, the grounds of a very unfounded objection to crystallography.

Circumstances that might lead into errour.

There is a fact relating to this substance worthy of remark, which is the equality of the number of retrogradations made both along the edges and at the angles of the terminal faces, and the great analogy between the planes owing to them.

TABLE of the MODIFICATIONS of the PRIMITIVE CRYSTAL of ENDELLION.

Primitive crystal.	A rectangular tetraedral prism with square bases, in which the side or edge of the prism is to the side of the base in the ratio of 3 to 5.

Prismatic Modifications.

Retrogradations made at the longitudinal edge of the prism.

No. of the modifications.	Form of the crystal.	Angles formed by the meeting of the planes of the crystals. Angles that the new planes form with each other when the modification is complete.		Angles that the new planes form with those of the primitive prism, as well as with those of the other prismatic modifications.			Nature of the retrogradations.
		Most obtuse.	Least obtuse.	With the primitive planes.	With the planes of the prism of the 1st modification.	With the planes of the prism of the 2d modification.	
1st,	A rectangular tetraedral prism, an inversion of the primitive.	90°	90°	135°			By 1 row along the longitudinal edges of the prism.
2d,	In the complete state, a right octaedral prism.	4 angles of 138° 52′	4 angles of 131° 8′	1 angle of 156° 26′	1 angle of 155° 54′		By 8 rows in breadth, and 3 laminæ in height, along the longitudinal edges of the prism.
3d,	In the complete state, a right octaedral prism differing from the former.	4 angles of 148° 6′	4 angles of 121° 54′	1 angle of 164° 3′	1 angle of 150° 57′	175° 23′	By 7 rows in breadth, and 2 laminæ in height, along the longitudinal edges of the prism.

Pyramidal Modifications.

Retrogradations made at the edges of the terminal faces.

No. of the modification.	Form of the crystal.	Angles that the new planes form with those of the primitive crystal		Angles that the new planes form with those of the other modifications made also along the edges of the terminal faces.			Nature of the retrogradations.
		With the terminal faces.	With the longitudinal planes.	With the planes of the 4th modification.	With the planes of the 5th modification.	With the planes of the 6th modification.	
4th,	In these 4 modifications the primitive crystal has the edges of its terminal faces supplied each by a single plane differently inclined in each modification.	129° 48′	140° 12′				By 1 row in breadth, and 2 laminæ in height, along the edges of the terminal faces.
5th,		135°	135°	174° 48′			By 3 rows in breadth, and 5 laminæ in height, along the edges of the terminal faces.
6th,		149 2′	120° 58′	160° 46′	165° 58′		By 1 row in breadth along the edges of the terminal faces.
7th,		171° 28′	98° 32′	138° 20′	143° 32′	157° 34′	By 4 rows in breadth along the edges of the terminal faces.

Retrogradations made at the angles of the terminal faces.

No. of the modification.	Form of the crystal.	Angles that the new planes form where they meet those of the primitive crystal.		Angles that the new planes form with those of the other modifications made also at the angles of the terminal faces.			Nature of the retrogradations.
		With the terminal faces.	With the longitudinal faces.	With the planes of the 8th modification.	With the planes of the 9th modification.	With the planes of the 10th modification.	
8th,	In these 4 modifications the primitive crystal has the angles of its terminal faces supplied each by a single plane, differently inclined in each modification.	125° 16′	144° 4′				By 3 rows in breadth, and 5 laminæ in height, at the angles of the terminal faces.
9th,		134° 39′	135° 31′	170° 47′			By 5 rows in breadth, and 6 laminæ in height, at the angles of the terminal faces.
10th,		150° 30′	119° 30′	154° 46′	163° 56′		By 3 rows in breadth, and 2 laminæ in height, at the angles of the terminal faces.
11th,		171° 57′	98° 3′	133° 19′	142° 32′	158° 33′	By 6 rows in breadth at the angles of the terminal faces.

Crystallization of Endellion, by the Count de Bournon.

Fig. 1.

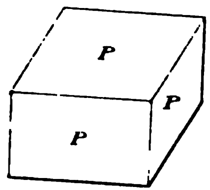


Fig. 2.

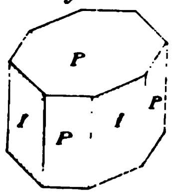


Fig. 3.

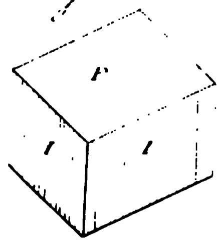


Fig. 4.

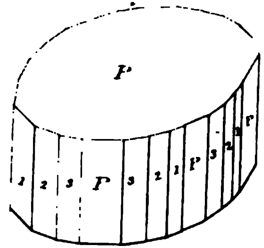


Fig. 5.

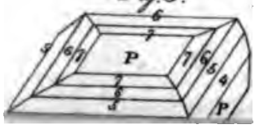


Fig. 6.

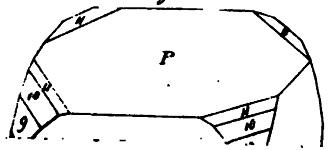
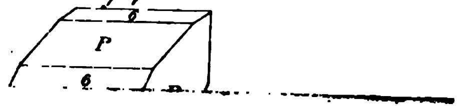


Fig. 7.



Retrogradations made at the angles of the terminal faces.

No. of the modification.	Form of the crystal.	Angles that the new planes form where they meet those of the primitive crystal.		Angles that the new planes form with those of the other modifications made also at the angles of the terminal faces.			Nature of the retrogradations.
		With the terminal faces.	With the longitudinal faces.	With the planes of the 8th modification.	With the planes of the 9th modification.	With the planes of the 10th modification.	
8th,	In these 4 modifications the primitive crystal has the angles of its terminal faces supplied each by a single plane, differently inclined in each modification.	125° 16′	144° 4′				By 3 rows in breadth, and 5 laminæ in height, at the angles of the terminal faces.
9th,		134° 39′	135° 31′	170° 47′			By 5 rows in breadth, and 6 laminæ in height, at the angles of the terminal faces.
10th,		150° 30′	119° 30′	154° 46′	163° 56′		By 3 rows in breadth, and 2 laminæ in height, at the angles of the terminal faces.
11th,		171° 57′	98° 3′	133° 19′	142° 32′	158° 33′	By 6 rows in breadth at the angles of the terminal faces.

Crystallization of Endellion. by the Count de Bournon.

Fig. 1.

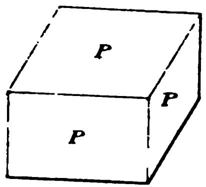


Fig. 2.

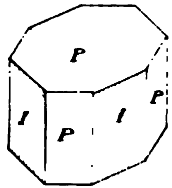


Fig. 3.

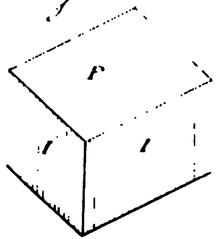


Fig. 4.

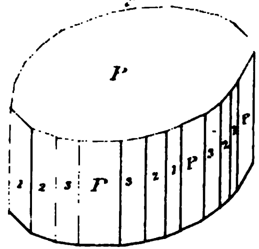


Fig. 5.

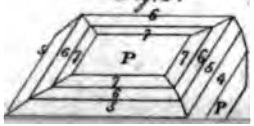


Fig. 6.

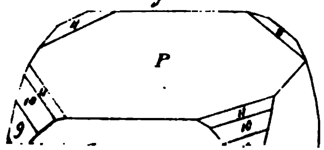
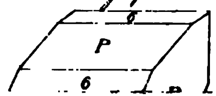


Fig. 7.



Crystallization of Endellion: by the Count de Bournon.

Fig 19.

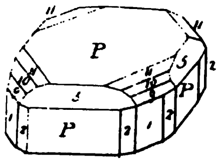


Fig 20

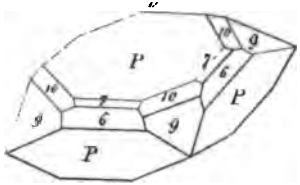


Fig 21

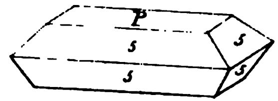


Fig. 22.

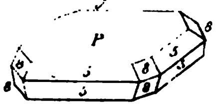


Fig. 23

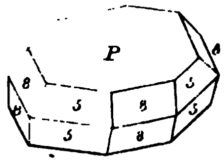


Fig. 24.

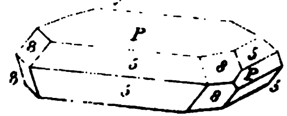


Fig. 25.

Fig. 26.

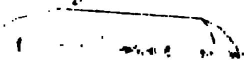
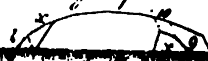


Fig. 27.



III

Of the Irritability of Vegetables. By Mr. ROBERT LYALL, *Surgeon. Read at the Literary and Philosophical Society at Manchester, Oct. the 6th,* 1809. *Communicated by the Author.*

THE irritability of some plants has attracted much attention from physiologists. Some of the most eminent men have without hesitation allowed, that vegetables possess the *faculty* of irritability; while others most strenuously have endeavoured to disprove, that any such principle exists in the vegetable kingdom. As soon as the mimosa sensitiva was discovered, without doubt the motion of its leaves when irritated by a stimulus were observed, but at what time the cause of this motion got the title of irritability is perhaps not so certain. Haller was probably among the earliest, who ascribed the motions of some plants to an irritable principle. After speaking of the comparative irritability of the heart, muscles, and intestines, with that of the ligaments, tendons, &c., he proceeds thus: Irritability of vegetables questioned.

"That this irritability exists abundantly throughout the "animal fibres, appears evidently from the example, which "we have in the polypi and other insects, which have nei- "ther brain nor nerves, but are notwithstanding extremely "impatient of all stimulus; and lastly we may take into "consideration the analogy of some plants, the flowers and "leaves of which either expand or contract by various de- "grees of heat and cold, and some even with a degree of "celerity not inferior to that of animals. This force is a "different and new principle from all other properties of "bodies hitherto known. We cannot account for it either "by gravity, attraction, or elasticity but by something "which exists in the soft fibres, and which vanishes by dry- "ing*." Haller afterward in his Elements of Physiology remarks, "It is evident, that there abounds, not only in the "animal but equally in the vegetable kingdom, a contrac- Haller's account of it.

* Primæ Lineæ Phys. p. 152. Ed. 1758.

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" tile power, by which the elementary fibres are drawn to- " ward each other*."

Many consider it as admitted.

Many authors, as Gmelin, Smith, Darwin, &c., have a long time since used the word irritability, when speaking of the motions of the parts of vegetables; and in the present time it is nearly as common to talk of the irritability of the vegetable as of that of the animal kingdom. When I began this paper, it was my intention, to have taken a compendious view of all that had been said on vegetable irritability; but the subject having now become so extensive, I found time would not permit me to make those experiments, which would have been requisite either to have proved that the vegetable kingdom in general was endowed with irritability, or to have disproved it. I have therefore contented myself at present with detailing some experiments made on particular plants: and even from these I hope to prove, that, if we do not admit that vegetables possess irritability, at least that they are possessed of something which is adequate to the muscular power in the animal body; and I am convinced, that without admitting this, many beautiful phenomena must perhaps for ever baffle the attempts of physiologists to explain them.

Sennebier unwilling to allow it.

One of the most celebrated vegetable physiologists, Sennebier has already treated extensively on this subject. He related almost every thing then known concerning the motions of vegetables, but has always been unwilling to allow, that an irritable principle had any share in these motions, and has tried to explain every phenomenon (which was before thought a proof of irritability) on mechanical principles. In the following pages I intend to quote some of his opinions, and endeavour to overturn them.

His definition of it.

As confusion might arise from the word irritability not being well defined, it becomes first necessary, to have Sennebier's opinion on this point, and then to fix what we understand by it at present. " Sennebier says: " Irritability " is that property, which forces a body to contract itself " when it is acted upon in a manner proper to produce this " effect; animals manifest this contraction in their mus- " cles in consequence of burning, pricking, or the contact

* Elem. Phys. vol. 4, p. 410.

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"of some acrid fluid, either corrosive or spirituous; then "the irritated body regains its former state, and the convul-"sions are often repeated, although the impression, which "is the cause of them, is not renewed. We can sometimes "recall these motions when they are finished, by the same "means which at first produced them. This irritability "shows itself by the action of a stimulant, which may be "of a very different nature, but always appropriated to the "muscle which it ought to move*." In another place he says: "We have pushed the analogy between animals and "plants too far. If we understand by irritability the power "of being affected by foreign bodies, it will be found in "almost all organized bodies; if we understand the volun-"tary command of a muscular force, the analogy subsists "no longer†."

Volition not necessarily connected with irritability.

I am willing to adhere to the definition, which Sennebier first states, but cannot agree with him in admitting the latter; for I do not consider that volition in every instance is connected with irritability. We know well, that the sensible iris often contracts and dilates without our knowledge of it. Here then volition, or a voluntary command of a muscular power, is out of the question; yet none will deny, but that the iris is one of the most irritable parts of the animal body. We also sometimes observe, that the motions of the iris continue, when all voluntary power is at the moment suspended, as in certain cases of concussion of the brain, &c. We know also, that the most important functions in the animal system are carried on quite independent of the will. The heart for instance is continually acting, yet we are unconscious of it. There are many other muscles &c., which also act independent of the will, as the diaphragm, the muscles of the intestines, and even at times the sphincter muscles, &c. Hence some of them have been denominated involuntary muscles. We have now seen then, that motions go on in the living animal system without the concurrence of the will, and yet that the parts are highly irritable. Bearing this idea in mind, cannot we conceive, that

* Sennebier's Physiologie Vegetale, tome V, p. 87.

† Physiol. Vegetal. tome V, page 120.

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the motions of plants may still be owing to irritability, although there is no mind to regulate them? I do not mean here to say, that plants have not a voluntary power; but merely admit that they have not in the present question. Wildenouw, when treating of the powers which vegetables possess, speaks thus on the subject. "Irritability, when "different stimuli produce a change in the parts of a body, "which without it would not have taken place*."

The author's definition of the term.

The following definition seems to me as comprehensive and accurate as any, and shows the meaning in which the word irritability is used in the following pages. By irritability then I understand that property inherent in some bodies, (or rather parts of bodies) by which, when a stimulus is applied, they are enabled to contract.

Plants experimented on by the author.

Having thus fixed the definition, let us proceed to the immediate subject of the paper. The plants on which I made my experiments were the drosera rotundifolia and longifolia. the euphorbia helioscopia, and the dionœa muscipula. The species of drosera come first under consideration; but before speaking of the moving power of their leaves, I think it necessary to give a minute description of them: and 1st of the rotundifolia.

Leaves of roundleaved sundew described.

The leaves of this plant, when properly unfolded, lie round the stem in a stellated manner. The footstalks of the leaves vary in length from half an inch to one inch and half. On the upper side they are a little roundish, and have at the same time a two edged appearance. The under surface is quite flat, and bounded by the two edges just mentioned. They are of a reddish colour, and are covered by a great number of long white hairs, spreading in different directions. They may be bent considerably without breaking, and, when the resisting force is removed, resume their former situation. At the end of the footstalks we find the leaves generally of an orbicular shape, hence the specific name of the plant. The under surface of the leaf is in the same plane with the under surface of the footstalk, (indeed it is difficult to say where the one ends and the other begins) has a somewhat membranaceous appearance, and is in general of a greenish (though sometimes purplish) colour. The

* Principl. Botany & Veg. Physiol. Wildenow (Translat. p. 219.)

upper

upper surface is covered with hairs, but when deprived of them appears nearly like the under. The leaf itself is very thin, and may be folded in different directions without breaking. The hairs, which cover the upper surface of the leaves, are of various lengths. Those on the margin are sometimes three eighths of an inch long, while those in the centre are not more than one line. In some well expanded leaves we may see nearly a regular gradation of the intermediate hairs between the two extremes. The marginal hairs are flattish at their base, and of the same colour with the leaf itself, (indeed they seem to be merely continuations of it). The other hairs are not so flat at their base as the marginal ones, but are also of the same colour as the leaf. The long hairs, except at their base, are of a red colour, each terminated by a little knob; while in the central hairs the knob is placed immediately on the white part of each. Every hair then is terminated by a little rounded body. In general each of these knobs is covered by a transparent and viscid fluid, which gives a fine appearance, and on account of which the plant was denominated ros solis or sundew. Each of these knobs appears to be a little gland, which secretes the viscid fluid* for a purpose soon to be mentioned.

The chief difference between the leaves of the longifolia and the rotundifolia is in the shape, those of the former being obovate. Longleaved.

* This fluid, which covers the glands of the hairs (of the leaves) of the indigenous species of drosera, has been differently denominated. Darwin talks of the pellucid drop of mucilage on every thread of the fringe, and in the same page speaks of the globules of mucus. Bot. Gard. Roth calls it the clammy juice, and I have here called it a transparent viscid fluid. This juice covers the glands when under the influence of the hottest sun; and also during the wettest weather. When the leaf is put under the electrical influence, each little globule of fluid spins out like a small tree, presenting a fine appearance. This fluid seems to possess the following properties, although I cannot vouch for the accuracy of the experiments. It is transparent, insipid, rather more consistent than the albumen ovi, extremely tenacious, insoluble in water, soluble in alcohol, in diluted sulphuric acid, and in solution of potash, is not very combustible, and is an electric or nonconductor. Quere. Is it a fluid sui generis? It certainly deserves the attention of the chemist.

Fluid of drosera.

Presents a beautiful appearance when electrified.

Its properties.

The

Mr. Whately noticed their contraction when irritated.

The first mention of the contraction of the leaves of the species of droseræ, at least in this kingdom, when irritated, will be found in Withering's Botany (vol. 2d p. 324) from which it would appear, that Mr. Whately discovered this curious phenomenon in August 1780. Mr. Gardom, who was with Mr. Whately at the time, gives the following account of this contraction in a letter to Dr. Withering.

This described.

"In August, "1780, examining the drosera in company with Mr. "Whately, on his inspecting some of the contracted leaves, "we observed a small insect or fly very closely imprisoned "therein, which occasioned some astonishment, to me at "least, how it happened to get into that confined situation. "Afterward, on Mr. Whately's centrically pressing with a "pin other leaves yet in their natural and expanded form, "we observed a remarkable sudden and elastic spring of the "leaves, so as to become inverted upwards, and as it were "incircling the pin, which evidently showed the method by "which the fly came into its embarrassing situation. This "experiment was renewed repeatedly, and with the same "effect, so that Mr. Whately and myself are both certain "of the fact."

Roth observed it earlier.

Roth published his work entitled Beitræge zur Botanick in 1782, from which Dr. Withering translates the following remarks.

His account of it.

"July 1779. Drosera rotundifolia and longifo- "lia. I remarked, that many leaves were folded together "from the point towards the base, and that all the hairs "were bent like a bow, but there was no apparent change "in the leaf stalk. Upon opening these leaves, I found in "each a dead insect. Hence I imagined, that this plant, "which has some resemblance to the dionœa muscipula, "might also have a similar moving power. With a pair of "pliers I placed an ant upon the middle of a leaf of the "drosera rotundifolia, but so as not to disturb the plant. "The ant endeavoured to escape, but was held fast by the "clammy juice at the points of the hairs, which was drawn "out by its feet into fine threads; in some minutes the "short hairs on the disk of the leaf began to bend, then "the long hairs, and laid themselves upon the insect. Af- "ter a while the leaf began to bend, and in some hours the "end of the leaf was so bent inwards as to touch the base.

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"The ant died in 15 minutes, which was before all the hairs "had bent themselves. On repeating this experiment, I "found the effects to follow sooner or later according to the "state of the weather. At eleven in the morning a small "fly, placed in the centre of the leaf, died sooner than the "ant had done, the hairs bent themselves as before, and at "five in the evening the leaf was bent together, and held "the fly shut up. The same experiments being made on "the drosera longifolia, the same effects followed, but more "rapidly. I observed, that in sultry weather, and hot sun-"shine, when the drops of juice upon the points of the "hairs are largest, the experiment succeeds best. If the "insect be a small one, sometimes only one edge of the leaf is "folded up; hence it should seem necessary, that the in-"sect should stir all the hairs of the leaf."

Roth also found, that the hairs bent themselves when he touched them with the point of a needle, with a hog's bristle, &c.; but that they returned to their former position after a certain time. He remarked the same contraction when he placed a piece of wood the weight of an ant upon the leaves; but that the impression made by the point of a needle remained longest. Although Withering points out the most of these circumstances particularly with a view to excite the attention of botanists to the species of drosera, yet I have not met with any account of experiments made since the time he wrote, in the year 1796.

Found to contract when touched by a needle.

For the last 5 months of the present year I have almost every day had these plants under my eye, either at home or abroad in the country. For my own part I must confess, that I have never seen that rapid contraction of the leaves of the drosera rotunda, which is mentioned by Mr. Gardom; but in all the experiments which I have made, I have observed that the contraction was gradual, though it seldom failed to happen, if the plant was in good condition. In some plants I have seen the contraction take place in nearly the time mentioned by Roth; but in most cases it has happened, that an hour was necessary for the complete bending of all the hairs, and that it required some hours more for the perfect shutting up of the leaves. In some plants I have seen the hairs and leaves nearly expanded even some hours

The contraction takes place but slowly.

hours after the stimulus was applied, and yet in the course of a few more hours both have contracted completely. The last experiments were made within doors, and probably the plants, though to appearance pretty fresh, were not in the most irritable state.

Experiment failed with Dr. Withering: probably because he did not wait long enough.

Dr. Withering mentions, that Mr. Whately's experiment failed in his hands, and from Roth's and the above observations we possibly may account for this. From what Mr. Gardom has said, he no doubt expected a sudden contraction of the leaf when irritated; but not finding this to happen, he probably concluded, that the plant was not in good condition, and, from placing implicit faith in Mr. Gardom's experiments, was not anxious to repeat them. I do not mean by what I have said, to impute to Mr. Gardom any inaccuracy in the relation of his experiments, but merely to put others on their guard, who wish to make experiments on this plant. From what I have said it is evident, that whoever has a wish to notice the motions of the leaves of the droseræ must not set out with the expectation of seeing a rapid motion (similar to what happens in the mimosæ) follow the application of a stimulus; but, to observe the ultimate effects, must watch with an attentive eye for at least, in general, 20 minutes. It is then that he will behold the bending of the hairs, which will soon be accompanied with that of the leaf.

Manner in which the contraction is accounted for

by Brousonet.

Having now considered the motions of the leaves of these plants, let us examine the manner in which they are accounted for. Broussonnet, in a memoir of the Academy of Sciences of Paris for 1784, suspects, that the disengagement of some fluids influences these motions. As Brousonnet's theory is quoted by Sennebier, I shall translate the words of the latter. After speaking of the dionœa muscipula, he says: "He (Brousconnet) remarked the same "phenomena upon two species of the drosera; their leaves "at first being folded upon themselves, their juices are not "carried immediately towards the little hairs which cover "them, but after their developement we can perceive a "drop of fluid towards the extremity of each hair; the in"sect absorbing this fluid, empties the vessels of the leaf, "which folds upon itfelf, and resumes its former position:

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"the quickness of this action is then proportional to the "number of hairs touched by the insect*."

This theory at first reading does not appear even to be plausible; for how is it possible, that an insect can absorb a thick tenacious fluid? No doubt however part of this fluid will be attached to the part of the insect which touches it, but this seems quite unconnected with the contraction of the leaf, as I shall immediately show. On the 30th of July I brought from rhe country a number of plants of the drosera rotundifolia, and on inspecting them I found many of the hairs deprived of their viscid fluid, but yet both they and the leaf remained quite expanded and in good condition. This appeared to me a favourable opportunity, to ascertain either the accuracy or inaccuracy of Brousonnet's theory. Next day in the afternoon about four o'clock, when rather cloudy and the temperature moderate, I placed a small bit of sulphate of copper in the disk of one of these expanded leaves. Now if Broussonnet's theory was accurate, I conceive, no effect should have taken place; but on the contrary by six o'clock most of the hairs on one side of the leaf, even the outermost, had bent themselves completely over the morsel of sulphate of copper. I have repeated this experiment frequently, and always with the same result. It may be well also to observe, that in other experiments the sulphate of copper rested upon some of the small hairs in the disk of the leaf without touching the leaf itself, yet the bending of the hairs and leaf was complete. In some plants also, in which every hair of the leaf has been covered with a drop of viscid fluid, I cautiously placed a small bit of bread, or wood, on three or four of the central hairs without touching the other hairs, or the viscid fluid on their ends, and in the course of a few hours I found, that all the hairs had contracted around the foreign body.

This theory not plausible, and inconsistent with facts.

We have here proof then, 1st, That the leaves do not contract when deprived of this viscid fluid; which ought to have been the case, if Broussonnet's theory had been accurate. 2dly, That the contraction takes place even when the fluid does not cover the little glands. 3dly, That the con-

Conclusions from them.

* Sennebier, Physiol. Veg. Tome V, p. 117.

traction

traction follows, although the foreign body is not brought into contact with all the hairs.

Apparent mistake of Roth.

This last conclusion is contrary to what is mentioned by Roth. He says, "if the insect be a small one, sometimes "only one edge of the leaf is folded up." *Hence it should seem necessary, that the insect should stir all the hairs of the leaf.*

Emptying the vessels not the cause of the contraction.

In the experiments mentioned no insect or any other body absorbs the fluid, and of course the vessels of the leaf cannot be emptied, which is completely in opposition to Broussonnet's theory.

Sennebier's hypothesis.

We shall next quote Sennebier's own opinion with regard to the contraction of the leaves of the droseræ. He says; "The hairs of the flowers" (he certainly means leaves) "of "the droseræ are put in motion by a hair, a needle, an ant, "or small bit of wood. It appears then, that the pressure "alone is the cause of it, and this effect permits us to "ascribe it to a cause purely mechanical *."

His facts true,

I am willing to agree with the first part of this sentence, but from the latter I must entirely dissent. Sennebier seems sensible, that the contractions of the leaves take place even when light bodies are placed upon them, which of itself would even lead us to suspect, that pressure is not alone the cause. I know, that, if we press on the centre of the leaf with a pin &c., we may cause its margin to approximate the pin; and this certainly would be owing to a mechanical cause. But suppose we see the contraction take place, as I have done, when a body specifically lighter than the leaf itself is placed in the centre, as a bit of rotten wood; should we be still inclined to ascribe it to a mechanical cause? Admit that it is the case. Suppose then we place the same bit of wood on the margin of the leaf, what effect ought to follow? If it was owing to a mechanical cause, or the weight of the forcing body, as in the last mentioned case, then we should expect, that the part of the margin of the leaf, on which the bit of wood rested, would be depressed; which undoubtedly is not the case, but on the contrary the margin rises, and then contracts toward the foreign

* Physiol. Veg. tome V, p. 104.

body,

body, or toward the footstalk of the leaf*. This would seem then to prove, that pressure alone can never be the cause of the contraction of the leaves of the droseræ, and consequently, that the action is not owing to a cause purely mechanical.

Having now seen, that the action of these leaves cannot be accounted for either by the theory of Brousonnet, or that of Sennebier, to what must we ascribe them? It appears to me, that the motions of these leaves must be owing to some other cause, and this cause a moving one†, denominate it what you will, for we must admit, that these leaves contract in consequence of the application of a stimulus; and I conceive, that this action is performed, if not by muscles, at least by *something* which is equivalent to muscles in the animal body.

The motion owing to something irritable and equivalent to muscles.

I have seen no other attempts to prove, that the contraction of the leaves of the droseræ is owing to any mechanical action; and other authors seem disposed to admit it as a proof of vegetable irritability. Among these authors we have the illustrious Dr. Smith, who seems to think, that the motions of the leaves are to be explained on the principle of irritability‡. We have also the no less celebrated Wildenouw, who, immediately after mentioning the irritability of the mimosa, dionœa, &c., says: "Less conspicuous, but "easily demonstrable, is the irritability of the indigenous "species of sundew, drosera rotundifolia and longifolia§." I will now conclude this part of the paper by quoting the words of Dr. Smith, which he uses when speaking of the mimosa, &c.: "it is vain to attempt any mechanical solu-

The principle of irritability in vegetables admitted by several.

* That this motion does not depend on pressure may be still better illustrated, by placing a fly, or some other body, on the apex of a leaf of the drosera longifolia. The hairs near the foreign body will contract around it, and then the apex of the leaf will rise upwards, and turn inwards, until it touches the base. Or if the offending body is small, the leaf will become convoluted around it.

† I mean a cause, which produces motion.

‡ Philos. Trans. abridged, vol. XXI, page 243.

§ Princip. Botany and Veg. Physiol. (Translation) page 222, edt. 1805.

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"tion of the phenomena mentioned". (Introduct. to Botany.)

What is the the final cause of this power?

Having found, that the leaves of the drosera catch flies on the principle of irritability, it may be afked, what is the intention of nature in allowing these and a few others that exclusive power? At present I am afraid this question cannot be satisfactorily answered; but in pursuing the inquiry we ought first to fix whether the particular contrivances in the dionæa, drosera, &c., are intended for offence or defence. Darwin entertained the latter idea; for, after mentioning the silene, he says: "In the dionœa muscipula "there is a still more wonderful contrivance, to prevent the "depredations of insects." Again, when speaking of the droseræ, he remarks, that, "This mucus is a secretion from "certain glands, and, like the viscous material round the "flower stalks of silene (catchfly), prevents small insects "from infesting the leaves."

Darwin supposes it intended to keep off insects.

More probably for the purpose of catching them.

I should rather be disposed to think, that the leaves of the dionœa and droseræ were intended for offence, i.e. for catching flies; for if defence was wanted, nature, ever simple in her operations, could have supplied these plants with a much simpler apparatus, as a number of spines, which would have been quite sufficient for this purpose; while we cannot conceive any contrivance, that would have answered better for catching flies, than what is seen in their leaves. Reasoning from analogy, this position will be strengthened. The sarracenia purpurea has tubular leaves beset at the margin with inverted hairs, which, like the wires of a mouse trap, render it very difficult for any unfortunate fly, that has fallen into the watery tube, to crawl out again. Now had it been the intention of nature, that this contrivance was for defence—would it not have been much easier for her to have placed the hairs on the margin of the leaf with the points upwards, instead of inverted, which would have effectually prevented the insect even from touching the inside of the leaf?

Leaves of the drosera well-adapted for this.

Regarding then these contrivances for offence, we have found the structure of the leaves of the droseræ admirably well calculated for this purpose. The glands are covered with

with a viscid fluid (probably) not only for alluring the little insects, but also for retaining them, until the contraction of the hairs (which is not immediate) shall begin. Now if the insect has been unable to overcome the tenacity of the fluid, it will soon be imprisoned by the hairs bending over it, and finally will either be killed by the contraction of the leaf, or retained in it until it dies. This contraction will continue until all is quiet, and even until the leaf becomes accustomed to its action, and of course suffers no farther stimulus; then most of the hairs and the leaf will expand and resume their former situations.

This is the manner then by which the flies &c. are imprisoned; but it may be inquired also, of what use are the insects to the plants? I think there can be little doubt, but that they are of some important use in the vegetable economy, or why should so many thousand insects be thus destroyed? Dr. Smith in his introduction to botany, after mentioning an interesting circumstance concerning the sarracenia adunca and purpurea, says: "Probably the air "evolved by these dead flies may be beneficial to vegetation." And again: "probably the leaves of the dionœa "muscipula, as well as the droseræ, catch insects for a similar "reason." On this subject I can say nothing at present, but must think Dr. Smith's explanation very ingenious, and probably just: but I cannot avoid asking one question, viz. As the flies in course of time are reduced to a pulpy state, both in the dionœa muscipula, and in the droseræ, is it not probable, that some of the pulpy mass may be absorbed, and so prove as useful to the plant as the putrid effluvia?

Of what use is the insect to the plant?

Perhaps absorbed as nutriment.

P. S. Since the above observations were written out, about five weeks ago, two papers on the motions of vegetables have appeared. The 1st in a supplement to the 23d vol. of Nicholson's Journal, and the 2d in the 107th number, or that for the present month. The author of both is Mrs. Ibbetson, whose knowledge, industry, and perseverance deserve the highest encomiums. She endeavours to explain not only the motions of plants, but also their sleep, their sensibility, and their volition, by the changes pro-

Mrs Ibbetson's theory

duced upon the spiral wires (before denominated spiral vessels), and what she calls a leatherlike substance, by the actions of heat, light, and moisture. I must confess, that Mrs. Ibbetson has brought forward some strong proofs in confirmation of her opinion; but at the same time I must acknowledge, that these proofs are not sufficient to convince me, "that all plants are merely machines governed by light and moisture, and that every idea of their sensibility or of their volition, is only a proof, that we too often let our imagination run away with our judgment:" which is the opinion of Mrs. Ibbetson. On the contrary I am still inclined to believe, that plants are both sensible and irritable. As to volition, I avoid saying any thing of this at present. In prosecuting this inquiry, it must be considered, that plants are living organized bodies; and of course, that they are at least governed by the laws of vitality, if I may so express myself. No mechanical machine is governed by such laws. Mrs. Ibbetson's opinion with regard to the motions of the mimosa sensitiva is certainly different from that which I entertain; for admitting all the mechanical structure mentioned, consisting of "different joints, pullies, knots, and bolts," to exist in the moving parts of plants, and that its spiral wires are capable of producing some of its motions; yet I cannot conceive, that either heat, light, or moisture, can possibly regulate some of the beautiful and striking experiments, which may be made either on the mimosa sensitiva, m. pudica, or others. Indeed such a mechanical structure seems to approach too near to the feeble works of men, and appears to me too complex (reasoning from analogy) to be the production of the *author* of *nature*. It is proper here to remark, that Mrs. Ibbetson's observations are mostly microscopical, and hence I am induced to suppose (though with the greatest deference to Mrs. Ibbetson's superior abilities) that possibly there may be some deception. But as I shall probably take the liberty of addressing a few observations on this important subject to Mr. Nicholson, after she has finished what she intends to write. I must for the present decline saying any thing farther; except, that, should Mrs. Ibbetson well explain these observations, I shall then be ready

ready to retract my opinion, and with the greatest pleasnre give to her the merit of having explained that which has puzzled many physiologists.

On the Irritability of the Vessels of Plants.

As almost every vegetable physiologist has treated of the ascent of the sap, and as the irritability of the vessels is intimately connected with this operation, this subject becomes extremely interesting. Van Marum, in a paper addressed to Ingenhousz which is contained in the Journal de Physique for September 1792, notices some experiments, which had been made by Coulon, and then proceeds to mention some electrical experiments, which he himself had made on plants. He first makes a few observations on the destruction of the irritability of the muscular fibres, and then reasons thus: If the contraction of the vessels of plants is the effect of their irritability, it will be destroyed in the same manner as the irritability of the muscular fibres. He adds: "I tried if this would happen in the summer of last year upon some species of euphorbia, which have the common property of giving out much milky sap from their wounds. I caused the stream of the grand Teylerian machine to pass through the branches of euphorbia lathyrus, and through the twigs of euphorbia campestris, and cyparissias, and I observed, that all the branches or twigs of these plants, which conducted the stream or the electrical torrent during twenty or thirty seconds, absolutely when they were cut did not give out any more sap from their wounds. I repeated these experiments with the branches of the fig-tree, which also gave out milk by their wounds. The effect was perfectly the same; the sap was not seen to flow out when the branches were cut, after they had conducted the electrical torrent during five seconds; but when the electrified branches were pressed between the fingers, a little sap could be perceived to flow, which rendered it evident, that the electrical torrent had not emptied the electrified vessels, by forcing the sap towards the roots,

Ascent of the sap connected with the irritability of the vessels.

This irritability, like that of muscles, destroyed by electricity.

"but

"but that the vessels had really lost the faculty of contract-"ing and of expelling the sap which they contained."

Sennebier supposes this to be owing to a destruction of the organization;

Sennebier, to account for these phenomena, observes, that "electricity stops the passage of the juices in the branches "exposed to its action by the shock which it occasions in "them, which may derange their motions, and produce "some change in the juices themselves. However strong "the sparks, which suspend the flowing of the juices, I "have suspected, that this suspension was produced by the "disorganization of the parts of plants which experienced "its action, and that the extravasated juices were diffused "into the spongy parts, which retained them. I commu-"nicated this suspicion to Van Marum, who answered me, "that an electric torrent could not destroy any thing by "passing through a less perfect conductor, such as plants "are, but especially when it was divided in such a manner "that the light of the electric fluid could not be perceived; "yet I insisted on his recalling to mind, whether he had "seen any manifest disorganization in his experiments; and "I desired him to make the experiment, and observe the "electrified parts with a magnifying glass." Physiol. Vegetal. tome V, p. 111.

but this was not the fact,

Agreeably to this request Van Marum repeated the experiment, and answered Sennebier, that he saw no apparent rupture (disorganization) in the organs of the vegetable. Sennebier then confesses, that he regards the experiments of Van Marum as the most favourable argument for the irritability of vegetables, and as the only one against which he has nothing to oppose.

The milky juice of certain plants not their sap, but a peculiar secretion.

In setting out with experiments of this kind it is necessary to know, that what has been called the sap by both the authors quoted is now thought to be a peculiar secretion. Dr. Smith, in his introduction to botany, has placed the milky juice both of the fig and spurge along with the secreted fluids of plants; and mentions, that Dr. Darwin has shown this fluid, quite distinct from the sap, to be, like animal milk, an *emulsion* or combination of a watery fluid with oil or resin.

If the vessels containing it

Of whatever nature the juice of the euphorbia is, I do not mean at present to inquire; for this fluid must be contained

ed in vessels, and if we can prove, that these vessels are irritable, then we can very easily transmit the analogy to the sap vessels. The experiments of Van Marum go far to establish the irritability of the vessels, which contain that secreted fluid; but the following, which I have made more than once, appear to me conclusive on the subject.

be irritable, we may infer, that the sap vessels are also.

Having a number of plants of the euphorbia helioscopia, I cut off the top of one, and found, that the milky juice flowed copiously. I now submitted this plant to the electric influence for some seconds by passing sparks * through it, which were so small as seldom to be visible. I then cut the stem about the middle. and but very little juice flowed. I next covered the end of the remaining half with a little moss, and placed the root in water. For some days it seemed languid, but in a few more began to recover. Soon after this I cut the stem across about two inches from the root, and the milky juice flowed abundantly.

Experiments on the subject.

In this experiment then we find, 1st, that the milky juice was expelled by the contraction of the vessels. 2dly, That the electrical fluid weakened the irritability of the vessels, but did not destroy it, or kill the remaining half of the plant: and 3dly, That after a certain time, when the plant had recovered from the shock, the milky sap was again expelled by the contraction of the vessels. This experiment also shows, that Sennebier's suspicion concerning the disorganization of the parts of the euphorbia helioscopia is groundless.

I have repeated some of Van Marum's experiments with the same results as himself. The last mentioned experiment, is rather a delicate one, for it is difficult to regulate the electric stream so as that it will only hurt the irritability of the vessels without killing the plant; but should it prove successful in the hands of others, I should then be disposed to think, that the irritability of the vessels, which contain this secreted milky fluid, will be established on a sure foundation never to be overturned.

Van Marum's repeated by the author.

* Or rather the stream.

IV.

IV.

Demonstration of the Cotesian Theorem. By Mr. P. Barlow.

To Mr. NICHOLSON.

SIR, *Oct. 13th,* 1809.

Cotesian theorem.

IF the following demonstration of the celebrated theorem of Cotes appear to you to possess a sufficient degree of originality, to entitle it to a place in your Journal, its insertion will oblige

Yours, &c.

Royal Military Academy, Woolwich. P. BARLOW.

La Grange's conjecture of what led Cotes to it, suggested the demonstration here given.

I was led to the consideration of this theorem from an observation made by La Grange, in his *Théorie des Fonctions Analytiques,* where he hazards a conjecture as to the probable circumstances that led Cotes to the discovery of this elegant property of the circle; and by pursuing the hints there given I arrived at the following demonstration, which appears to me to be, at least, as satisfactory as any one that I have at present seen; and on this I rest my apology for intruding into the pages of the Philosophical Journal a subject, that has so long, and so often, engaged the attention of many very celebrated mathematicians, without any of them having arrived at what may be considered an unobjectionable demonstration.

It will be proper, however, for the information of some of your readers, before we enter upon the demonstration, to state the theorem itself, which is as follows.

Cotes's Theorem.

Theorem.

Let A B C, &c., Pl. VIII, fig. 31, 32, be any circle, divided into any even number of equal parts, $2m$, as A B, B C, C D, &c.; also let P be any point in the diameter, either

either within the circle, or in the diameter produced, and join P B, P C, P D, &c., then will

$$P B \times P D \times P F \text{ \&c.} = A O^m + P O^m$$
$$\text{and } P A \times P C \times P E \text{ \&c.} = A O^m \backsim P O^m$$

Demonstration.

It is a well known trigonometrical property, that by making cos. $x = p$, we may derive, Demonstration.

$$\begin{aligned}
\cos.\ x &= p \\
\cos.\ 2x &= 2p^2 - 1 \\
\cos.\ 3x &= 4p^3 - 3p \\
\cos.\ 4x &= 8p^4 - 8p^2 + 1 \\
\cos.\ 5x &= 16p^5 - 20p^3 + 5p
\end{aligned}$$

&c. &c. See Bonnycastle's Trig. p. 301.

Now by substituting $2p = y + \frac{1}{y}$, and multiplying each of the above formulæ by 2, they are reduced to the following simple forms:

$$\begin{aligned}
2\cos.\ x &= y + \frac{1}{y} \\
2\cos.\ 2x &= y^2 + \frac{1}{y^2} \\
2\cos.\ 3x &= y^3 + \frac{1}{y^3} \\
2\cos.\ 4x &= y^4 + \frac{1}{y^4} \\
2\cos.\ 5x &= y^5 + \frac{1}{y^5}
\end{aligned}$$

whence we may conclude generally $\Big\}$ $2\cos.\ mx = y^m + \frac{1}{y^m}$

But as this general form is only deduced from observing the law of the leading forms, it will be more sa isfactory to see it derived in a direct manner: which may be done by means of the general formula.

$$\text{Cos. } nx = 2\cos.\ x \,.\, \cos.\ (n-1)\,x - \cos.\ (n-2)\,x.$$

Bonnycastle's Trig. p. 300.

Or

Demonstration of Cotes's theorem.

Or making $n - 1 = m$, and transposing, we have

$$2 \cos. x \times 2 \cos. m x = 2 \cos. (m - 1) x + 2 \cos. (m + 1) x.$$

When, by making $2 \cos. x = y + \frac{1}{y}$, and admitting, that in any case, $2 \cos. (m - 1) x = y^{m-1} + \frac{1}{y^{m-1}}$, and $2 \cos. m x = y^m + \frac{1}{y^m}$, (which we know to be true when $m = 2$ from the above forms), we shall find that $2 \cos. (m + 1) x = y^{m+1} + \frac{1}{y^{m+1}}$; that is, it will be of the same form with regard to the multiple $m + 1$, which may be shown as follows.

In the foregoing general formula, by substituting for $2 \cos. x$, $2 \cos. (m - 1) x$, and $2 \cos. m x$, their respective values $y + \frac{1}{y}$, $y^{m-1} + \frac{1}{y^{m-1}}$, and $y^m + \frac{1}{y^m}$; we have,

$$2 \cos. (m+1) x = \left(y^m + \frac{1}{y^m}\right) \times \left(y + \frac{1}{y}\right) - \left(y^{m-1} + \frac{1}{y^{m-1}}\right)$$

the latter side of which equation being reduced gives

$$2 \cos. (m + 1) x = y^{m+1} + \frac{1}{y^{m+1}}$$

that is, it is of the same form with regard to its multiple as the two preceding forms; and since we know that those are true when $m = 2$, or when $m = 1$, it follows, from what is said above, that it is true when $m = 3$, and consequently also when $m = 4$, and generally for any value of m. We may therefore conclude with certainty, that if

$$2 \cos. x = y + \frac{1}{y}$$

that $$2 \cos. m x = y^m + \frac{1}{y^m}$$

From which we readily deduce the following equations:

1st, $y^2 - 2 y \cos. x + 1 = 0$
2d, $y^{2m} - 2 y^m \cos. m x + 1 = 0$

Now

Demonstration of Cotes's theorem.

Now these equations must necessarily have one common root, being both obtained from the same value of y; and farther, since both equations are reciprocal ones, if y be one root, $\frac{1}{y}$ is another root, and the first equation having but two roots, and such of those being also roots of the second equation, it follows that the former of our formulæ is a divisor of the latter. And this is true, if we change y into $q\,y$, without, however, altering the value of $2\cos. x$, and $2\cos. m\,x$; for by substituting for these their true value $y+\frac{1}{y}$ and $y^m+\frac{1}{y^m}$, and $q\,y$ for y, the above formulæ are reduced to

$$\text{1st,}\quad (q\,y^2-1)\times(q-1)$$
$$\text{2d,}\quad (q^m\,y^{2m}-1)\times(q^m-1)$$

The former of which is evidently a divisor of the latter, entirely independent of the value of q, or of the measure of the angle represented by x.

We may therefore, instead of x, write $\frac{m\,x+n\,c}{m}$, then our formulæ will become

$$q^2\,y^2-2\,q\,y\,\cos.\left(\frac{m\,x+n\,c}{m}\right)+1$$
$$q^{2m}\,y^{2m}-2\,q^m\,y^m\cos.\,(m\,x+n\,c)+1$$

The former being still a divisor of the latter.

We may here also observe, that while c represents the entire circumference, the $\cos.\,(m\,x+n\,c)=\cos.\,m\,x$, whatever integral value we give to n; and hence making respectively $n=0, 1, 2, 3,$ &c, $m-1$, it follows, that the formula

$$q^{2m}\,y^{2m}-2\,q^m\,y^m\cos\,(m\,x+n\,c)+1,$$

has for its divisors the m following formulæ,

$$q^2\,y^2-2\,q\,y\,\cos.\left(\frac{m\,x+0}{m}\right)+1$$
$$q^2\,y^2-2\,q\,y\,\cos.\left(\frac{m\,x+c}{m}\right)+1$$
$$q^2\,y^2-2\,q\,y\,\cos.\left(\frac{m\,x+2\,c}{m}\right)+1$$

$q^2\,y^2$

Demonstration of Cotes's theorem.

$$q^2 y^2 - 2 q y \cos.\left(\frac{m x + 3 c}{m}\right) + 1$$

&c.

$$q^2 y^2 - 2 q y \cos.\left(\frac{m x + \overline{m - 1}. c}{m}\right) + 1$$

Since then our first formula of the $2 m$th degree has for its divisors the above m formulæ of the 2d degree; it follows, from the nature of equations, that it is equal to the product of these m formulæ.

Now by making $m x = 0$, and $m x = \frac{c}{2}$, we have $2 \cos. m x = \pm 1$, and the general form is reduced to this particular one,

$$(q^m y^m \pm 1)^2,$$

which is the Cotesian theorem.

For in the first case, making $m x = 0$, it becomes

$$(q^m y^m - 1)^2,$$

having for its divisors,

$$q^2 y^2 - 2 q y \cos.\left(\frac{0 c}{2 m}\right) + 1$$

$$q^2 y^2 - 2 q y \cos.\left(\frac{2 c}{2 m}\right) + 1$$

$$q^2 y^2 - 2 q y \cos.\left(\frac{4 c}{2 m}\right) + 1$$

&c. &c.

And secondly, making $m x = \frac{c}{2}$, we have for our formula

$$(q^m y^m + 1)^2,$$

its divisors being

$$q^2 y^2 - 2 q y \cos.\left(\frac{c}{2 m}\right) + 1$$

$$q^2 y^2 - 2 q y \cos.\left(\frac{3 c}{2 m}\right) + 1$$

$$q^2 y^2 - 2 q y \cos.\left(\frac{5 c}{2 m}\right) + 1$$

&c. &c.

Or

Or using radius r, instead of radius 1, and writing $\frac{c}{2m}$ $= x$, we have Demonstration of Cotes's theorem.

$$\text{formula } (q^m y^m - r^m)^2, \text{ and divisors} \begin{cases} q^2 y^2 - 2qyr \cos. 0x + 1 \\ q^2 y^2 - 2qyr \cos. 2x + 1 \\ q^2 y^2 - 2qyr \cos. 4x + 1 \\ \quad \&c. \quad \&c. \end{cases}$$

And also

$$\text{formula } (q^m y^m + r^m)^2, \text{ and divisors} \begin{cases} q^2 y^2 - 2qyr \cos. x + 1 \\ q^2 y^2 - 2qyr \cos. 3x + 1 \\ q^2 y^2 - 2qyr \cos. 5x + 1 \\ \quad \&c. \quad \&c. \end{cases}$$

Now the first set of these divisors agrees with PA^2, PC^2, PE^2, &c. in the above figures; and the second set with PB^2, PD^2, PF^2, &c.

For since q is undeterminate, make $qy = OP$, $r = OB$, also $r . \cos. x = OQ$; then by (Euclid 13, 2)

$$PB^2 = PO^2 - 2OQ \times OP + OB^2;$$

and it is exactly the same with all the other divisors.

Since therefore $(OB^m - PO^m)^2 = PA^2 \times PC^2 \times PE^2$ &c.
and $(OB^m + PO^m)^2 = PB^2 \times PD^2 \times PF^2$ &c.
also $OA = OB$, it follows, that

$$OA^m \backsim PO^m = PA \times PC \times PE, \ \&c.$$
$$\text{and } OA^m + PO^m = PB \times PD \times PF, \ \&c.$$

Q. E. D.

V.

On the Influence of Electricity on Flame: by Mr. LEOPOLD VACCA, *Colonel of the 32d Regiment of Light Infantry**.

FLAME has been much used in electrical experiments, but I do not know, that the effect produced by electricity on the figure of flame has ever been noticed. Effect of electricity on the shape of flame not known.

We know, that, if water dropping from a slender siphon be electrified, the dropping will be changed into a stream. Water dropping converted into a stream,

* Journal de Physique, vol. LXV, p. 224.

We

or made to spout further by it.

We know too, that if a stream of water be electrified, its velocity will be increased, and it will describe a larger parabola. The reason of these phenomena is well known: they depend on the mutual repulsion of the particles of the water, all of them being electrified with the same kind of electricity.

Flame may be supposed to be enlarged by it:

Hence it might be expected, that, flame being an assemblage of very subtile particles, if we could introduce into it electricity of one kind, they must repel each other, and consequently the flame be enlarged.

but experiment

To satisfy myself of the fact, I took a small vessel of metal filled with spirit of wine, and insulated it. By means of a metallic chain I formed a communication between the vessel and the conductor of a good electrical machine of glass. I kindled the spirit of wine without moving the machine, and observed the shape and magnitude of the flame. I turned the handle of the machine, and perceived, that its action occasioned a very considerable contraction of the flame. When I suspended the action of the machine, I found, that the flame resumed its former dimensions. This experiment, a thousand times repeated, constantly afforded me the same results.

shows the contrary.

I was at first puzzled to account for this, but I think I have at length hit upon the true cause.

Electricity issuing from points repels them by its action on the air;

We know, that electricity issuing out of a body to traverse the air repels it nearly in the same manner as powder repels the gun barrel in which it is burned. We know, that by means of points dispersing in the air the electricity accumulated in a star of metal very movable on a pivot we occasion the star to turn very rapidly in a direction contrary to that of the points. We know too, that on this principle Ferguson constructed a planetarium, which was set in motion by electricity. We know also, that no substance disperses electricity more than flame does.

and in the same manner repels the flame.

If then electricity escape from all the points that constitute the surface of flame, these points must be repelled back into the flame; consequently the flame will be compressed, and its volume diminished.

VI.

VI.

Of the Action of Phosphorus and oxigenized muriatic acid Gas on Alkalis; by Messrs. BOUILLON-LAGRANGE *and* VOGEL*.

SOME years ago we perceived, that, after having obtained a pretty large quantity of phosphuretted hidrogen gas from heating a mixture of phosphorus with pure caustic potash dissolved in water, a blackish substance remained; and toward the end of the process another gas was evolved, which no longer inflamed on contact with the air. Phenomena of phosphorus heated with potash observed long ago.

This circumstance not appearing of sufficient importance for us to pursue it, we had thought no more of the memorandum made of it, and should probably have neglected it still, had not the same phenomena occurred again lately, as we were lecturing on phosphorus. Struck at the same time with the discovery of Mr. Davy respecting the nature of alkalis, we could no longer look with indifference on the facts we perceived anew. Occurred again lately.

We do not mean to discuss before the Society the decomposition of potash by the processes, that Messrs. Thenard, Gay-Lussac, and Curaudeau have employed. We shall only observe, that we have repeated in presence of the pupils, who attend our chemical lectures, the experiment of the decomposition of soda by charcoal in a gun barrel, as we had seen it performed by Mr. Curaudau †, and succeeded completely. The experiments we are about to submit to the Society were made with phosphorus on potash and soda. At present we shall content ourselves with exhibiting the facts as we observed them, carefully abstaining from all conjectural theory, persuaded, that, to attempt explanations not grounded on incontestable facts, or requiring interpretation, must give rise to errours, which, instead of advancing science, only tend to render our notions uncertain. Soda decomposed by means of charcoal. Experiments with phosphorus and alkalis.

* Annales de Chimie, May 1808, vol. LXVI, p. 194.

† See p. 37 of our present volume.

To

Preparation of phosphorus for mixing it with alkalis.

To obtain the mixture of phosphorus with potash, which appeared to us impracticable unless by the method we shall mention, we fused phosphorus in a phial into which we had put some warm water. The phial was shaken till the water was cold; to accelerate which we immersed it in cold water, after a little while, still continuing to shake it. Thus the phosphorus was reduced to the state of powder. The supernatant water being decanted off, it was replaced by diluted oximuriatic acid. This acid, we are taught by Mr. Juch of Wurtzburg, has the property of depriving phosphorus of carbon, if it be true that it contains any. From coloured it becomes white, and in this state the acid is to be separated from it, and the moisture absorbed by blotting paper.

Purity of potash best ascertained by barytes water.

On the other hand we satisfied ourselves of the purity of the potash by treating it afresh with alcohol, and, after fusion, testing it with lime water and barytes water. We shall here observe, that lime water is not a certain test for determining whether potash retain any carbonic acid; for, if the mixture be diluted with water, a small quantity of carbonate of lime will be dissolved. This solution does not take place with barytes, and the smallest quantity of carbonate of barytes is always visible, which renders this substance preferable to lime for examining potash or soda*.

Mixture of the phosphorus with potash.

Caustic potash was reduced to powder in a glass mortar, and then an equal quantity of phosphorus, prepared as above, was added. To avoid the combustion, which took place before the temperature was lowered, we placed the mortar in a mixture of powdered ice and muriate of soda. A slight trituration was sufficient, and the mixture was immediately introduced into a coated stone retort, which was

Caustic potash precipitates from lime water carbonate of lime that is soluble.

* It has long been known, that a concentrated solution of caustic potash is precipitated by lime water, and that this precipitate is soluble in a large quantity of water; whence it has been inferred, that the potash merely took the water from the lime, and the latter fell down in the caustic state. But we have found, that this precipitate is in fact a carbonate of lime, which, thus intimately divided, is soluble in water. We have observed too, that this solution is not owing to an excess of alkali, for, after passing carbonic acid gas into lime water, the precipitate separated was equally soluble in water, yet the liquor was neutral.

placed

placed on the grate of a reverberatory furnace. A tube of safety was fitted to the beak of the retort, which communicated with a jar filled with mercury. The whole being thus arranged, a gentle heat was applied. This first degree of heat sometimes occasioned the combustion of a small portion of phosphorus; but this may be prevented by covering the mixture with a little powdered potash. It is easy to conceive, that this combustion is owing to the air contained in the retort; and that, when the caloric has occasioned a vacuum in the apparatus, no combustion can take place. Of this we satisfied ourselves by a direct experiment. We afterward increased the fire, till the retort was of a white heat. Heated in a stone retort.

During the whole course of the process, a gas was evolved, the properties of which we shall mention presently. Gas evolved.

When the retort was completely cold, we broke it, and found in it a black mass. Its inside was entirely covered with a coat shining as if metallic, and having the appearance of carburet of iron. Residuum.

The black matter has a slightly alkaline taste, and was but little soluble in cold water. By means of boiling however we dissolved it all, except a black powder, which was precipitated. Boiling nitric acid likewise dissolves it; and a black matter, which is nothing but oxide of carbon, separates in a similar way. Its properties.

Neither of these solutions contains any thing but phosphate of potash.

Among the various experiments we made there was one, in which we obtained a similar black mass, but without any perceptible taste. Water had no action on it. Nitric acid dissolved it, and separated from it oxide of carbon. The portion of the tube communicating with the retort was lined with a grayish substance, which took fire on coming into contact with water. As to the salt that remained in the retort, it was nothing but neutral phosphate of potash, which we know to be nearly insoluble in water. Residuum not alkaline.

In the course of these experiments we employed alternately potash and soda, and instead of a stone retort, a retort and tube of porcelain. The results were the same. Soda gave the same results.

The

Properties of the gas.

The properties exhibited by the gas mentioned above were: 1. It was neither acid nor alkaline. 2. It had a slight alliaceous smell. 3. It took fire at the approach of the white flame of a taper, and formed by this combustion a little phosphoric acid and oxide of phosphorus. 4. It detonated loudly, when mixed with oxigen gas, and touched by a substance in the state of ignition. 5. It did not take fire on coming into contact with the atmosphere, with oxigen gas, or with nitrous gas. 6. It was a little soluble in water; and in this solution nitrate of silver occasioned a blackish precipitate. 7. It inflamed rapidly when mixed with oximuriatic acid gas, and afterward deposited a little oxide of phosphorus on the sides of the jar.

Easy method of procuring this gas.

This elastic fluid may be procured in a simple and easy way. It is sufficient to put a little phosphorus, cut into small pieces and very dry, into a common phial; to strew over it perfectly dry caustic potash; and to adapt to it a curved tube opening under a jar filled with mercury. On heating the phial gently white vapours will form without inflammation, and the gas will be evolved. The temperature is to be raised gradually, till no more bubbles pass.

Residuum.

There will remain in the phial a black substance, slightly alkaline, containing phosphate of potash.

Difference of the results if water be present.

There is a very striking difference when a little water is added to the mixture. As long as any moisture is present, we obtain phosphuretted hidrogen gas, which inflames on the contact of air: but as soon as the matter is dry, if the action of the fire be continued, the gas evolved no longer inflames by the contact of air, and has all the properties of that above mentioned.

This difference in the results no doubt deserves examination, and perhaps may be explained without any hypothesis. The same may be said of the following experiment, which may serve to elucidate the phenomena above described.

Oximuriatic acid gas passed through potash at a white heat.

Two drachms of pure potash were introduced into a porcelain tube passing through a reverberatory furnace. Through this tube, brought to a white heat, was transmitted oximuriatic acid gas, expelled from a matrass into which the proper ingredients had been put. An intermediate phial,

phial, without water, received the gas before it reached the porcelain tube; and the other extremity of the tube communicated with a pneumato-chemical apparatus.

The moment the oximuriatic acid gas had reached the potash, a great deal of water passed into the jar in vapour not easily condensed. When condensed they left behind carbonic acid gas. Some time after oximuriatic acid gas was perceived in the jar. On examining it a copious precipitate was obtained with lime water and barytes water, but it was necessary to employ them in excess. Toward the end of the process no more oximuriatic acid gas passed over, but a mixture of oxigen and carbonic acid gas.

Aqueous vapour first passed, then oximuriatic acid gas, and lastly oxigen, each mixed with carbonic acid.

Carbonic acid gas therefore was disengaged during the whole course of the operation, taking place at its three different periods; first with the water in vapour; secondly with the oximuriatic acid gas; and thirdly with the oxigen gas. All these gasses were cloudy, and did not become transparent till the water was deposited.

The quantity of carbonic acid gas, collected and separated, appeared to us too great to be ascribed to the acid, that the potash retains. Besides, we employed an alkali we had carefully purified; and the acid it could contain, for whatever precaution is taken it cannot be perfectly freed from it, was only to be detected by barytes water, which gave only a slight cloud, scarcely perceptible.

Too much carbonic acid to have been retained in the potash.

We have not however any design, still less the presumption, to attempt to establish or determine the principles of potash: though from this experiment we might be tempted to suppose, that hidrogen and carbon exist in this alkali in certain proportions.

Probably the alkali contains carbon and hidrogen.

In the porcelain tube we found muriate of potash in thin white laminæ but slightly adherent. Some of them were tinged of a light green. The weight of this salt was much inferior to that of the potash employed.

Muriate of potash in the tube less in weight than the alkali employed.

It follows then, that all these experiments, if of little importance in themselves, may lead us to examine with more attention the changes, that take place in substances, when placed in contact with others at temperatures more or less elevated.

Potash treated with oxigen & hidrogen gas.

The action of oxigen gas and hidrogen gas on potash has likewise presented us with some phenomena, of which we shall give an account.

VII.

On the Chemical Analysis of the Onion: by Messrs. FOURCROY *and* VAUQUELIN *.

Liliaceous plants similar in structure, yet widely different in properties.

AMONG the plants that compose the very natural family of liliaceæ, and seem to have the same interior organization, there are some, which, as the onion, differ essentially in their taste, smell, and almost all their properties. The authors of the paper, of which we shall here give an abridgment, had in view to seek the causes of this difference. Beside the solution of this problem, their investigation includes several chemical facts of great importance to the progress of vegetable analysis.

Properties of the juice of the onion.

The onion, *allium cepa*, grated to a pulp, and subjected to the action of a press, gives out a white viscid juice, somewhat opake, of a strong smell, colourless the instant it is filtered, but acquiring a rosy hue from the contact of the air, on account of the oil it contains. It is perceptibly acid. It is precipitable by the acetate of lead, lime, oxalic acid, nitrate of silver, and potash. Distilled it yields a milky water, slightly acid, with a few drops of oil floating on the surface.

The water distilled from the juice contains sulphur.

The water distilled from onion juice has a strong smell, and forms a light yellow precipitate with acetate of lead. Two experiments were sufficient to prove the existence of sulphur in this liquor. 1. Oximuriatic acid makes it clear, deprives it of its smell, and gives it the property of precipitating nitrate of barytes. 2. This liquor, distilled in a copper alembic, forms on the surface of the head a black iridescent pellicle, which is sulphuret of copper. The sulphur is held in solution in the onion juice by an essen-

* Annales de Chimie, vol. LXV, p. 161. Abridged by Mr. Laugier from the original paper read to the National Institute.

tial

tial oil, which exists in it with a small quantity of acetous acid.

Examination of the juice left after distillation.

The part of the juice left in the retort deposited a fawn coloured matter, having a strong smell of onions. From this sediment alcohol separated oil and sulphur. What was not acted upon by the alcohol yielded by distillation a black, fetid oil, and carbonate of ammonia, which indicate the presence of a vegeto-animal matter, in the coagulum of the onion juice.

The fluid from which the preceding sediment was separated had a deep brown red colour and a saccharine taste. With acetate of lead it gave a yellow precipitate. This precipitate, heated before the blowpipe, grew black, emitted a smell of sulphurous acid, and left a globule of phosphate of lead. The solution of the residuum in sulphuric acid diluted in water, heated, and filtered, yielded two precipitates of phosphate of lime on the successive addition of ammonia and lime water. Hence the authors conclude, that the precipitate formed by acetate of lead in distilled onion juice is composed of oxide of lead, phosphoric acid, sulphur, and a vegeto-animal matter.

Onion juice examined by means of fermentation.

Messrs. Fourcroy and Vauquelin, having employed fermentation as a good mean of vegetable analysis on several occasions with success, tried it with onion juice. Exposed to a temperature of 15° or 20° [59° or 68° F.] in a suitable apparatus, this juice emitted no gas; but it acquired in succession a tint of rose colour and of yellow, and a fawn coloured sediment was deposited. The vessels being unluted, they were surprised to find, that the juice was converted into vinegar, but that it retained the onion smell as strong as before fermentation. This proves, that the volatile or essential oil had undergone no alteration. They afterward found, that, if the alcoholic fermentation did not take place, it must be ascribed to the absence of a suitable ferment.

Examination of the sediment formed during the acetification of the onion juice.

The sediment formed during the acetous fermentation of the juice appeared to demand particular attention. This substance has the following properties. *a.* It is in a state of minute division, forms a smooth paste, and has a strong onion smell. *b.* Alcohol takes up from it sulphur and oil,

 as

as is easy to judge by the action of oximuriatic acid, which renders the alcohol turbid, and communicates to it the property of forming a copious precipitate with nitrate of barytes. *c*. After being treated with alcohol the sediment has less smell: it scintillates on burning coals, shrivels, and then swells up, emitting the fetid smell of animal substances. *d*. Mixed with a solution of sugar, no movement was produced, and no alcohol was formed: whence we may conclude, that this substance is not of the nature of yeast, and not calculated to excite alcoholic fermentation.

Examination of the vinegar of onion juice, and the crystallizable matter it holds in solution.

The vinegar formed by onion juice had a yellowish colour, a very strong smell of onions, and an acid taste, but yet saccharine*. It marked 6° on the areometer for acids: but this density was owing to a peculiar substance, which gives it the property of crystallizing when it is sufficiently concentrated.

This substance, which particularly excited the attention of Messrs. Fourcroy and Vauquelin, is neither an acid nor a neutral salt. It presents itself in the form of fine, white, acicular crystals, disposed in diverging rays: it has a saccharine and at the same time acid taste: it is mixed with a gummy matter, and also with citric acid. Hot alcohol dissolves both the crystalline substance and the acid accompanying it, leaving the gummy matter untouched. As the alcoholic solution cools, white needly crystals separate, shining, and arranged in stars.

Properties of the crystals.

These crystals have the following properties. *a*. They are of a snowy whiteness, and of a mild, saccharine taste. *b*. They are equally soluble in water and in alcohol. *c*. They burn like common sugar. *d*. Their solution does not ferment with yeast. *e*. Nitric acid converts them into the oxalic. They afford no mucous acid, unless when they contain mucilage. Our authors satisfied themselves on this occasion, that manna, with which they compared them, is wholly converted into oxalic acid, and does not yield an atom of mucous acid, on being treated with the nitric acid, if care be taken to separate all the mucilage that accompanies

* Pickled onions, when long kept, perhaps two or three years, acquire a saccharine taste, so as at length to lose almost all their acidity. C.

nies it. From these experiments they infer, that the crystalline matter of onion juice is nothing but manna. This substance is manna.

It remained now to determine, whether the manna were ready formed in the juice, or developed by fermentation. To solve this question, they treated onion juice concentrated by evaporation in several ways, and obtained only fermentable sugar, instead of the manna which the fermented juice had furnished. It appears then, that the manna obtained from onion juice is the product of fermentation; and this opinion is the more probable, as a scrupulous examination of the fermented juice exhibited to them all the principles it contained before, except sugar*. formed by the fermentation.

From the preceding experiments Messrs. Fourcroy and Vauquelin conclude, that sugar, either when its solution is too dilute, or when it contains a different ferment from yeast, constantly undergoes a kind of alteration by acetification, which divides it into two new compounds, unequal in quantity, and differing in the proportion of their principles: one vinegar, which contains fewer radicals than sugar; the other manna, which contains more radicals than sugar. In fact, all the chemical knowledge we have of these three substances confirms this result.

Perhaps, add the authors, there is no improbability in supposing, that, in the trees which furnish manna, this substance is formed in their saccharine juice by the acetous fermentation of sugar, assisted by the glutinous matter that exists in all vegetables. It is natural to suppose, that the saccharine juice of the ash and the birch, once escaped from its vessels, runs into the acetous fermentation; and that the results are manna and vinegar, the latter of which afterward evaporates. This no doubt is the reason, why new manna is acid, and smells of vinegar. This opinion may be confirmed by examining the kind of sap or liquor, that flows from trees apt to furnish manna, when the stem is tapped. Natural formation of manna.

The examination, that Messrs. Fourcroy and Vauquelin made of manna, convinced them, that beside the crystallizable matter analogous to what they obtained from the fer- Examination of natural manna.

* The fact mentioned in the preceding note tends to confirm this. C.

mented

mented onion juice, this substance contains a small quantity of fermentable sugar, which was observed by Proust and Thenard also a small portion of yellow matter of a nauseous smell and taste, which fermentation does not destroy, and to which they think its purgative quality is to be ascribed; and lastly a little mucilage, which alone is converted into mucous acid when manna is treated with nitric acid. Melon-juice in like manner affords them manna, which they could not discover previous to fermentation.

Spirituous fermentation of onion juice.

Desirous of knowing whether onion-juice, as a saccharine liquor, be capable of affording alcohol on the addition of a suitable ferment, our authors mixed 244 gr. [3767 grs.] of this juice, reduced to the consistence of an extract, with 2 lit. [2 wine quarts] of water, and 30 gr. [463 grs] of beer yeast of the consistence of paste. The mixture, exposed to a temperature of 16° or 20° [61° to 68° F.], exhibited all the phenomena observed during alcoholic fermentation. Carbonic acid was evolved; and the distillation of the fermented liquor yielded 134 gr. [2069 grs] of brandy at 22°, equivalent to 73 gr. [1127 grs] of alcohol at 40°. This quantity of alcohol, according to Lavoisier, requires for its production 114 gr. [1760 grs] of sugar.

General results of the analysis of the onion.

From the experiments above related it follows, that the onion is composed of, 1, a white, acrid, volatile, and odorous oil: 2, sulphur combined with oil, which occasions its fetid smell: 3, a large quantity of uncrystallizable sugar: 4, a great deal of mucilage analogous to gum arabic: 5, a vegeto-animal matter coagulable by heat and analogous to gluten; 6, phosphoric acid, partly free, partly combined with lime; and acetic acid: 7, a small quantity of citrate of lime, which had never before been met with in vegetables: 8, a parenchymatous or very tender fibrous substance retaining vegeto-animal matter.

Sources of its properties.

It is to the combination of the oil of the onion, the sulphur, the saccharine substance, and the mucilage, that we must ascribe the emulsion or milk, that flows from the slices of this bulbous root, its acrimony, its property of irritating the eyes, exciting tears, blackening silver, &c. Most acrid plants, as the euphorbias, chelidonias, arums, hellebores, owe their injurious qualties to oily and resinous substances:

Acrimony of plants resides in an oil, or resin, and best

stances: and the authors recommend the oximuriatic acid as the most certain antidote, to destroy the pernicious effects of this acrid principle. destroyed by oximuriatic acid,

The presence of free phosphoric acid in plants is an interesting fact, but how is it produced there? Does it pass directly from the earth into plants? or does it come from phosphorus absorbed by the plant from the soil? Of these two questions the authors have sought the solution; and various arguments, supported by accurate observation, have led them to think, that the phosphorus existing in the animal matters employed to promote vegetation passes in combination with fats and oils into the plants, where it combines with oxigen, and produces the phosphoric acid we meet with. Free phosphoric acid, how gets it into plants?

Messrs. Fourcroy and Vauquelin terminate their paper by very judicious reflections on the advantages, that may be derived from analyses of the plants that are most common and most in use. The numerous and interesting facts contained in their paper, and the consequences deduced from them, among which we must not omit the possibility of the solution of earthy phosphoric calculi by the juice of onions, leave no doubt of the benefit that may accrue from researches of this kind, in promoting vegetable chemistry and the knowledge of vegetables in general. Analyses of common plants advantageous. Some calculi perhaps soluble by onion juice.

VIII.

Abridgment of a Paper on the Species of Carnivorous Animals, the Bones of which are found mixed with those of Bears in Caverns in Germany and Hungary. By Mr. CUVIER *.

1. ON a separate paper on the fossil hyena, I have already mentioned, that bones of this animal were found in the Baumannshoehle, and in a cavern at Gaylenreuth. Out of a quantity of bones from the latter, among which those of bears were most numerous, I procured a jawbone of a hyena, Bones of the hyena found with those of the bear.

* Journal de Physique, vol. LXV, p 282.

hyena, more complete than those I had before represented, but exhibiting precisely the same characters. The whole of the lower edge and the condyloid process are very perfect; and the four jaw teeth are seen, but a little broken. The anterior extremity and coronoid apophysis only are wanting.

The four jaw teeth occupy the length of 0·092 m. [3·6 inches] nearly the same space as in the piece from Fouvent, a place in Franche-Comté, where fossil bones of the hyena are found.

Another fragment from the same place is part of the jaw bone of an hyena, which must have been larger than the great hyena of the Levant in the proportion of 3 to 2.

Lastly Mr. Blumenbach sent me a drawing of the fourth or principal upper grinding tooth of an hyena found in the same place.

Bones of an animal of the genus felis,

2. A very large animal of the genus felis has also left numerous remains in these caverns. Proofs of this are found for those of Hungary in Vollgnard's paper in the Ephem. Nat. Cur., an. iv, dec. 1, obs. 170, p. 227. It is an unguicular phalanx, easily known by its great vertical height, little length, and different projections.

Leibnitz in his Protogea has represented part of a fossil skull of an animal of this order found in the cavern of Schartzfels. Soemmering has given a more accurate delineation of the same specimen, which is at present in the museum at Goettingen. He asserts, that this cranium perfectly resembles that of a middle sized lion, and differs from that of the bear of the caverns in thirty-six particulars, which he points out: but most of these particulars are common to all the genus felis, and not peculiar to the lion.

mentioned by Soemmering,

Esper has had engraved several teeth found in the cavern of Gaylenreuth, which closely resemble those of an animal of the felis genus, if we could depend on the accuracy of the engraving: but the differences between some of these teeth and those of the hyena depends on such slight variations, as might have escaped a common draughtsman.

Esper,

Mr. Rosenmueller promises soon to publish a work, which will contain a description of the bones of an unknown animal of

and Rosenmueller

of the lion kind, and he adds, that "these bones are not precisely similar to those of the present lion."

In the mean time he gives us, without being aware of it, three bones of this genus, which he has suffered to slip in among those of the bear: namely the semilunar scaphoid, the cuboid of the hind foot, and the first cuneiform. But if these figures be of the natural size, the animal must have been of prodigious dimensions, which the other bones that I have examined do not indicate.

Indeed I have myself some new pieces to produce both from Gaylenreuth and other places. First single teeth. A second and third upper grinder of a felis: both from Gaylenreuth. Another from the cavern of Altenstein, with the drawing of which I was furnished by the celebrated Blumenbach. These teeth differ completely from those of the hyena.

I have likewise half a lower jaw from the collection of Mr. Hadrian Camper. It is that of a felis. The posterior tooth bilobated and without a heel, the vacuity before the alveolus of the last but one, the direction of the lower edge, and the situation of the maxillary foramina, leave no room to doubt of it.

Probably belonged to the felis onca.

But when the question is, to what species of felis does this half jaw come the nearest? the answer is not so easy. I will venture to say, that it is impossible without the numerous means of comparison, which I was so fortunate as to have in my power. Now these means have demonstrated to me, as they will to any one who shall employ them, that this bone belonged neither to a lion, nor lionness, nor tiger; still less to a leopard, or the little panther of the keepers of wild beasts: but that, if we must refer it to a living species, it can only be to the jaguar, or great spotted panther of South Ameriea, which it most resembles, particularly in the curve of its lower edge.

The most accurate ideas we have hitherto of the different large animals of the genus felis will perhaps occasion a doubt of this: but the characters of these animals and their osteology will be the subject of a separate dissertation, that will remove all the difficulties.

3. The

Bones of an animal of the genus canis.

3. The bones of an animal of the wolf or dog kind are the first I have found fossil, that are no way distinguishable from those of animals now inhabiting the surface of the same country: but then it is in a genus, where the distinction of species by separate bones alone is almost impossible.

Skeletons of many of these not easily distinguishable.

Daubenton has already observed how difficult it is to distinguish the skeleton of a wolf from that of a mastiff, or shepherd's dog of the same size. More interested than he in finding out their characteristics, I have studied them longer, carefully comparing the heads of several individuals of these breeds of dogs with those of several wolves. All that I have been able to remark is, that wolves have the triangular part of the forehead behind the orbits a little narrower and flatter, the sagitto-occipital ridge longer and more elevated, and the teeth, particularly the canine, larger in proportion. But these differences are so slight, that there are frequently much greater between individuals of the same species; and we can scarcely avoid thinking with Daubenton, that the dog and the wolf are the same speecies.

These noticed by Esper,

The existence of wolf's bones in the cavern of Gaylenreuth was announced by Esper in his first work. He gives a portion of the upper jaw, pl. X, fig. a, and three canine teeth, pl. V, fig. 3 and 4, pl. XII, fig. 1. He adds in his second paper, that wolves skulls of the common size have occurred almost as frequently as those of bears, mixed with those of dogs of the same size, and with others smaller.

Rosenmueller,

Mr. Rosenmueller too observes, that bones of the wolf kind occur at Gaylenreuth in the same state as those of the bear, and that they were deposited there at the same period.

and Fischer.

Mr. Fischer has sent me the drawing of one of these wolf's heads taken from Gaylenreuth, and preserved in the collection at Darmstadt. It is more likely the head of a wolf than of a dog by the elevation of the sagitto-occipital ridge: but if we may trust to the drawing, the face is not so long in proportion to the skull as in the common wolf, and the muzzle not so slender, to speak absolutely.

I would

I would recommend therefore to those, who have at their command any of these fossil skulls of wolves, to make a comparative examination of them attentively. With accurate measurements they might perhaps find some constant specific character. I have before me only lower jaws. Our museum possesses four, all from Gaylenreuth. I have a fifth from the same place, that was in Mr. Camper's collection.

A comparison of the skulls recommended.

All these pieces so nearly resemble the analogous bones in wolves and great dogs, that the eye can scarcely perceive any difference, even individually. The ascending branch however resembles the dog more than the wolf, because it is smaller in proportion, and the condyloid process is larger. The groove for the insertion of the masseter muscle is also narrower and deeper: but I repeat, these characteristics are so slight, that I cannot venture to offer them as distinguishing, if the analogy of other fossil bones did not authorize us to believe, that there were specific differences with respect to these also.

However, if these differences be not sufficiently proved, the identity of the species is not by this resemblance of some parts. The various species of the genus canis, the different foxes, &c., resemble one another so much in shape and size, that it is very possible some of their bones may not be distinguishable.

It is proper to observe here, that these bones, whatever they are, are in the same state as those of the bear, felis, and hyena; their colour, consistence, and covering are the same. Every thing indicates, that they date from the same period, and were buried together.

I have taken myself from a block of tufa filled with bones, a tooth, and a metacarpal bone of the thumb. The latter resembles in all respects that of a wolf or a large dog.

This species of wolf is found, as well as that of the hyena, with the bones of elephants. Mr. Jaeger has sent me the drawing of his most perfect lower jaw found at Cantstadt, and Mr. Camper that of a tooth of the same kind found at Romagnano, in the place where the elephants bones described by Fortis are found. Mr. Esper says too, that he has

Found with the bones of elephants.

has some of these wolf's heads from Kuhldorf, in the county of Eichstaedt, taken from the quarry where the hyena's head decribed by Collini was found, which I have mentioned elsewhere.

Bones of an animal resembling the fox.

4. We have also the bones of an animal very like the fox, if it be not the fox itself, at Gaylenreuth. Mr. Rosenmueller thinks, that these, with the human bones, and those of the sheep and badger, are more recent than those of the bear, as they are in better preservation. It is possible, that there may be such, but to those I am going to mention this does not apply. They were embedded in the same tufa as the bones of the bear and hyena, from which I extracted them myself; and their composition is not less altered. If they be whiter, it is perhaps because, being smaller, the causes capable of depriving them of their animal matter acted upon them with more force.

Very abundant.

They must be very common there, for I took all those of which I am speaking from a block a few inches in diameter, composed in great part of bones of the bear and hyena: but they who have searched these caverns have been struck only with the large bones, and have neglected the smaller, which are neither less curious, nor of less importance toward the solution of the grand problem of fossil bones.

Enumeration of them.

My foxes bones consist of the following: 1, an outer incisive tooth: 2, a canine tooth; both of the lower jaw: 3, an ungular phalanx: 4, an intermediate phalanx: 5, a first phalanx: 6, a phalanx of the imperfect toe of the hind foot: 7, a first metatarsal bone: 8, a cuneiform bone of the carpus: 9, a first cuneiform of the tarsus: 10, a second cuneiform of the tarsus: 11, a vertebra of the middle of the tail: 12, several sesamoid bones.

To this species I also refer the canine tooth represented by Esper, Pl. X, fig. *c*.

Compared with those of the common fox.

All these bones, compared with the analogous ones in the skeleton of a full grown fox, appeared rather larger. That of the metacarpus in particular was a little longer, without being larger: but these differences were not sufficient to establish a difference of species. On the other hand the different foxes, as the corsac, the isatis, or jackal, the Cape fox, *c. mesomelas*, and the two American foxes, *c. Virginianus*,

nus, and *c. cinereo-argenteus*, resemble each other too much in size, for us to suppose that these parts of the skeleton, which in general are not very characteristic, should exhibit greater differences than those observed in the bones of the fossil fox.

I would recommend it therefore to persons, who live near these caverns, to procure other bones of this species, and particularly skulls, that they may resume the comparison. As far as I can judge from an imperfect skeleton of a jackal, which I have examined, I should not be surprised to learn, that they resemble the bones of this animal more than those of our common fox.

Farther examination necessary.

5. The same block, that furnished me with the fox's bones I have just described, supplied me with some of a much smaller carnivorous animal of the weasel genus, and resembling the European polecat, or that of the Cape. These consist of 1, a portion of the pelvis including the pubis and ischium: 2, the two outer metatarsal bones: 3, a phalanx of the second row: 4, the last but one of the dorsal vertebra: 5, two caudal vertebræ.

Bones of a species of weasel.

These are certainly bones of a weasel: and of all the skeletons of this genus I have had an opportunity of examining, there are only the polecats of Europe and of the Cape of good Hope, to which I can refer them.

Most resemble those of the European or Cape polecat.

The martin and common weasel have the metatarsal bones in particular incomparably larger. In the zorilla and polecat they are exactly similar to the fossil specimens.

The dorsal vertebra is neither so long nor so large as in the polecat: but it resembles that of the zorilla; and this resemblance struck me particularly at first, as the bones of the hyena of the caverns also greatly resemble those of the spotted hyena, which is equally an inhabitant of the Cape. But the fragment of the pelvis directed me again to the polecat of Europe, which it resembles more than it does the zorilla. Thus I could not venture to lay down the hypothesis, which at first appeared so seducing, that we must search in the neighbourhood of the Cape for the animals most resembling those of our caverns.

It is extremely desirable, that more of these small bones should be collected, and compared also with those of the

mustela

mustela Sarmatica, or polecat of Poland, of the *m. Siberica*, or yellow martin of Siberia, and of the Siberian polecat. I have never yet seen the skeletons of these three species *.

IX.

Account of some Colours for Painting, found at Pompeii: by Mr. CHAPTAL. *Communicated to the First Class of the Institute, March the 6th,* 1809 †.

Paints found in a colour shop at Pompeii.

HER majesty the empress and queen has done me the honour, to put into my hands seven specimens of paints, found in a colour-shop at Pompeii.

Terra verte.

Of these one has undergone no preparation. It is a greenish and saponaceous clay, such as nature affords in various parts of the globe, and analogous to what is known by the name of *terra di Verona*, or terra verte.

Yellow ochre.

The second is a fine yellow ochre, which has been freed by washing, as is done in the present day, from all the matter injurious to its beauty or pureness. As this substance is reddened by calcination with a very moderate heat, it affords a fresh proof, that the ashes, by which Pompeii was overwhelmed, retained but a very slight warmth.

Spanish brown.

No. 3 is a brown red, of the same nature as that at present in our shops, which is employed for the coarse reddish coat applied to casks in seaports, and to the doors and windows

All these animals found in hot climates.

* One of the things that must appear at first sight the most astonishing in the collection of the fossil bones, with which these caverns are filled, is to find there bones of animals, which we should suppose could not live in the same climate: but it is possible, that all these animals may have existed in the same country. The animals of the genus felis, whether lions or tigers, indicate, that the country at that time must have enjoyed a pretty warm climate; and we know from unquestionable testimony, that the wolf, jackal, polecat, and bear, are all found in Africa likewise. *J. C. Delamétherie.*

† Annales de Chimie, vol. LXX, p. 22.

dows of some houses. It is produced by the calcination of the yellow ochre just mentioned.

No. 4 is a pumice stone, very light, and very white. It is of a fine and close grain. Pumice stone.

The other three are compound colours, which I have been obliged to analyse, in order to know their constituent principles.

The first of these, No. 5, is a fine, deep, and mellow blue. It is in small pieces of similar form. The outside of each piece is a paler blue than the inside, the colour of which is more bright and lively than that of the finest verditer. A blue compound.

Muriatic, nitric, and sulphuric acids, produce a slight effervescence with this colour. They appear to brighten it, even with long boiling. Oximuriatic acid has no action on it. It differs therefore from ultramarine, which is destroyed by these four acids, as Clement and Desormes observed. Treated with acids,

Ammonia has no action on it. ammonia,

Exposed to the flame of the blowpipe it grows blackish, and the continued action of the flame converts it into a reddish brown frit. With borax it fuses before the blowpipe into a greenish blue glass. Treated with potash on a stand of platina it produces a greenish frit, which afterward becomes brown, and at length assumes the metallic colour of copper. This frit is partly soluble in water. Muriatic acid poured into this solution produces a copious flocculent precipitate; and the liquor decanted from this precipitate yields another in considerable quantity with oxalate of ammonia. and the blowpipe.

Nitric acid dissolves with effervescence the residuum, which the alkali could not dissolve, and the solution is green. Ammonia produces in this solution a precipitate, which it redissolves when added in excess, and then the solution becomes blue.

This colour then appears to be composed of oxide of copper, lime, and alumine. It approaches to verditer in the nature of its principles, but differs from it in its chemical properties. It appears to be the result not of a precipitation, Its composition.

tion, but of a commencement of vitrification, or rather to be a true frit.

Of very ancient use.

The process by means of which the ancients obtained this colour appears to be lost to us. All we can learn, on consulting the annals of the arts, is, that the use of this colour dates from ages long prior to the destruction of Pompeii. Mr. Descotils observed a lively, bright, and vitreous blue colour, in some hieroglyphic paintings in Egypt; and he satisfied himself, that this colour was prepared from copper.

Somewhat analogous to blue verditer.

Considering the nature of the constituent principles of this colour, we can compare it only with the verditer of the moderns; but with regard to its use in the arts we may set against it to advantage our ultramarine and smalt, particularly since Mr. Thenard has made known a preparation of the latter, which admits of being used with oil. But verditer has neither the brightness nor permanence of the ancient colour; and both ultramarine and smalt are more costly than a composition, the three ingredients of which are so cheap. It would therefore be worth while, to endeavour to discover the process for manufacturing this blue colour.

A light blue, similar to the preceding.

No. 6 is a light blue sand, mixed with a few whitish particles. Analysis shows in it the same principles as in the preceding colour; and it may be considered as a composition of the same nature, in which the lime and alumine are in larger proportion.

Rose colour.

I have only to examine No. 7. This is a fine rose colour, soft to the touch, reducible between the fingers to an impalpable powder, and giving the skin the colour of a pleasing bloom.

Action of the blowpipe on it,

This colour, exposed to the blowpipe, first blackens, and afterward becomes white. It emits no perceptible smell of ammonia.

and of acids.

Muriatic acid dissolves it with slight effervescence. From this solution ammonia throws down a flocculent precipitate, which is completely redissolved by potash.

Contains no metal.

Neither infusion of galls nor hidrosulphuret of ammonia indicates the presence of any metal in it.

This

This rose colour may be considered as a true lake, in which the colouring principle is mixed with alumine. Its properties, its tint, and the nature of its colouring principle, give it an almost perfect similarity to the madder lake, which I have mentioned in my Treatise on Dyeing Cotton. The preservation of this lake for nineteen centuries without any perceptible alteration is a phenomenon, that must astonish the chemist.

A lake probably from madder.

Such is the nature of the seven colours, which have been put into my hands by her majesty the empress. They appear to have been absolutely designed for painting: yet, if we examine the glaze or coating of the Roman pottery, vast quantities of fragments of which are found in all places, where their armies successively established themselves, we shall readily be of opinion, that most of these earths may have been employed, to form the coating of this earthenware.

Used as paints and perhaps for pottery.

In fact, most of this pottery is covered with a red coat, which is in no degree vitreous, and may have been given by the yellow ochre, or the brown red, reduced by trituration to a fine paste, incorporated with some mucilaginous, gummy, or oily substance, and laid on with a pencil. Mr. d'Arcet, who has examined the Roman pottery with great skill, has a vase, the substance of which is of a dull red, and the surface of which was coated with something of this kind. The place where the workman left off coating the vessel may be seen; and on the bottom, which is not coated, may be seen red strokes, made by the workman to try his colour or his pencil.

Roman earthenware, coated,

It is not uncommon to find other vessels, the substance of which is of a different colour from the red coating, that covers the surface.

Perhaps the Romans even employed saline fluxes, to facilitate the baking of the outer coat of their pottery.

and perhaps saline fluxes used.

Mr. d'Arcet has perfectly imitated the white covering of the Etruscan vases, by using a clay that bakes white, with which he mixed a twentieth part of borax.

White of the Etruscan vases.

It appears, that in the first century of our era the Romans were unacquainted with the use of metallic fluxes, to fix and vitrify the coating of pottery. At least the analysis of the

Metallic fluxes unknown to the Romans.

the coatings of Etruscan vases, and red, white, and brown earthenware, afforded no indication of metal either to Mr. d'Arcet or me. It was not till a later period, that sulphurets of copper and lead, and oxides of lead, were employed for this purpose. Occasionally indeed we find these metallic coatings on some vases dug up; but I conceive them to have been fabricated subsequently to the time when the Romans possessed Gaul; for all those I have examined, the origin of which evidently dates from the former period, give no trace of lead or copper when analysed.

Black glaze.

Sometimes the black colour alone exhibits marks of vitrification. I have even seen several specimens of ancient pottery, in which this character is indisputable; and I have always thought, that a vitreous lava formed the base of these coatings, the fusion of which, naturally easy, was farther promoted by a mixture of saline fluxes. I published my work on this subject five and twenty years ago; Mr. Fourcroy applied it in a very happy manner in his manufactory at Paris; and Mr. d'Arcet has confirmed my opinions by his own experience.

Their pottery baked with a low heat.

The Roman pottery however, particularly the Etruscan vases, was baked with a very slight heat compared with that we now employ. It may be estimated at 7° or 8° of Wedgwood's pyrometer; and at this degree, as Mr. d'Arcet has shown, we cannot employ the oxides of lead, which then penetrate into the substance, and leave the colour without any gloss on the surface.

Far inferior to us in this manufacture.

No doubt we are far superior to the ancients in the art of pottery. The numerous series of metallic oxides, successively discovered and applied, has furnished us with the means of enriching our pottery with a variety of colours equally brilliant and substantial; at the same time that a better chosen mixture of earths has enabled us, to obtain the greatest degree of hardness with almost absolute infusibility: but the Etruscan vases will always be prized for the beauty, elegance, and regularity of their forms; and I thought, that whatever relates to the history of the arts among the Roman people would be acceptable to those, who interest themselves in the promotion of manufactures.

X.

X.

Remarks on the Introduction of Air into the Blood through the Lungs, in Answer to Mr. ACTON. *In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

YOUR correspondent, Mr. Acton, appears rather hastily to accuse Mr. Ellis of " a most singular perversion of one of Mr. Bichat's experiments" in the last number of your Journal. It seems to be the object of Mr. E., in the first paragraph alluded to, to show, that when air is forced into the blood, through the lungs, it quickly destroys life; and in support of this position he quotes facts from the writings of Haller, Girtanner, and Bichat, which abundantly establish that point. According to Mr. Acton however, Bichat is said to consider these experiments, as " affording a proof " of the passage of the air into the blood, through the " lungs, in *addition* to that of healthy respiration." Does Mr. Ellis deny this? On the contrary, has he not brought forward these experiments expressly to prove it, with the additional circumstance, that it speedily occasions death? Vindication of Mr. Ellis.

But, by " a most singular perversion" of Mr. Ellis's meaning, Mr. A. applies these experiments to another part of that author's work, where he evidently appears to be speaking only of *natural* respiration, and makes no allusion whatever to the *forcible* injection of air into the blood, which fact he had before admitted for a very different purpose. In the language of Mr. A. I dare not say this was intended; but it is " wonderful," if the application be just, that he did not rather undertake to show, that Mr. E., in the two passages quoted, had contradicted himself, than that he had perverted the experiments of Mr. Bichat.

I am, &c.

J. F.

P. S.—With respect to the great question, whether a portion of the oxigen, consumed in respiration, be absorbd by the Oxigen gas not absorbed by the blood in respiration.

the blood as Mr. Acton supposes, or whether it be not entirely converted into carbonic gas as Mr. Ellis maintains, I do not presume to venture a decided opinion: but I must be allowed to say, that, notwithstanding the numerous experiments of Mr. Acton in behalf of the former opinion, the latter has received no inconsiderable support from the recent experiments of Dalton and Thompson*, and Messrs. Allen and Pepys†.

XI.

Letter from Mr. ROBERT BANCKS *concerning the Meteorological Journal.*

To Mr. NICHOLSON,

SIR,

Height of the thermometer.

IN consequence of the letter of your anonymous correspondent, which you had the goodness to show me agreeably to his permission, I have been endeavouring to discover the cause of my statement of the height of the thermometer not agreeing precisely with those of others, particularly on the hottest day of July 1808.

Situation of the instruments.

The account of the situation of the instruments has been given in your 21st volume, p. 79. I first placed with my thermometers two or three by other makers, the best I could procure: but could find no difference worth notice. When standing near them indeed a little while with a friend, to examine and compare them attentively, we repeatedly found, that the thermometer nearest to which we stood always rose a little the highest: no doubt owing to the heat communicated from our bodies.

The heat not communicated quickly enough to the contiguous air.

From the circumstance however, that the thermometer appeared to give generally too low a temperature for the highest, and too high for the lowest, when they deviated from the Journal of the Royal Society; I was induced to suppose, that the air contiguous to them might be too slow

* Syst. Chem. vol. V, 3d edit. † Phil. Trans. 1808.

in

in acquiring the general temperature of the atmosphere; and conceived this might be owing to their being placed in a yard of rather too confined dimensions. I therefore placed other thermometers, by way of comparison, at the height of 17 feet from the ground, in a situation where they were equally protected from reflected heat, but were of course in a less confined part of the atmosphere. My conjecture was in some degree verified; for, on a careful examination for several weeks, I have found the thermometers above apparently a little more sensible of change; though still the difference between them has never been great.

Accordingly I have since registered from the thermometers in this situation; and I am inclined to think, that few can be found superior to it in the advantage of not being affected by any reflected or adventitious heat or cold. From what I have observed it is probable, as I noticed at the time, that my statement of the heat in July 1808 was a little below the truth: but if the difficulty of finding a situation totally unaffected by reflected or communicated heat be considered, I am persuaded that much greater errours in excess were made by other observers, than mine in defect. Thermometers now placed higher.

I am, Sir,

Your humble servant,

R. BANCKS.

SCIENTIFIC NEWS.

Proceedings of the French National Institute.

THE Class of Mathematical and Physical Sciences has proposed the following prize question. French National Institute.

The first inquiries concerning sound date very high in antiquity. The proportions of the length of strings producing different notes are ascribed to Pythagoras: but this branch of science made no remarkable progress before the end of the seventeenth century. Sauveur, a member of the French Academy of Sciences, showed by very ingenious experiments, that the sounding string was divided into seve- Investigation of sound. Sauveur.

ral

ral waves, separated by nodes, or points of rest; and he determined the absolute number of vibrations that constitute each note, deduced in the first place from delicate and curious experiments, which he compared afterward with the algebraic formulæ derived from the theory of the centres of oscillation; as appears in the Memoirs of the Academy for 1713.

Taylor. Taylor, in his *Methodus Incrementorum*, published in 1717*, treated the problem more profoundly, on the hypothesis, that the forces acting on the material points of the system are proportional to their distances from a right line drawn from one fixed point to the other, so that these points all arrive at the right line at the same time. Twenty or thirty years after Daniel Bernoulli farther developed the theory of Taylor; but for the general and strict solution of the problem we are indebted to d'Alembert and Euler. These great geometricians first employed the differential equation of the motion of the sonorous chord, which is with partial differences, and of the second order. This equation was first found and summed up by d'Alembert, but Euler was more sensible of its generality.

Bernoulli.

D'Alembert & Euler.

Sonorous tubes. An equation of the same order is applicable to the oscillations of air in tubes; and does not change, when from the case of the simple line we proceed to cases of two or three dimensions.

strings, In the problems of which we are speaking the order of the differential equation of the motion is connected with the manner, in which we consider the effects of elasticity in the body moved. It has been here applied to a chord stretched between two points. If the chord be let loose at one of these points, being perfectly flexible, it is incapable of producing any acoustic phenomenon.

and springs. It is otherwise if the chord be a spring properly so called. In this case, confining it if you please to a single fixed point, the spring set to vibrate will produce a perceptible sound, if its oscillations exceed 24 per second: but the differential equation of this movement will be of the 4th order. The first problem may be considered as a particular

* Taylor's paper on the motion of tense strings was published in the Phil. Trans. for 1713.

case

case of the second, abstracting the spring: but the converse does not hold.

The essential difference between the questions of the movement, considered in each of these points of view, in the case of a simple line, leads us immediately to conceive, that we must find differences of the same kind, and in particular a great increase of difficulties, when we introduce two dimensions into the calculation. The acoustic phenomena exhibited by parchment stretched, as on a drum-head, are referrible to those of the chord; the phenomena of metallic plates, to those of the spring. Stretched parchment.

Euler, in his paper *de Motu vibratorio Tympanorum*, has considered the parchment as composed of threads crossing each other at right angles. A geometrician of the Institute has published in one of its volumes some researches on this subject, contemplating it in the same point of view. The differential equation of the motion, which is partial and of the 2d order, cannot be summed up, at least in finite terms. Drums.

In his paper *de Sono Campanarum* Euler attempts to reduce the vibrations of hard surfaces formed by revolution to those of circular elastic rings, of which he considers them as an assemblage, situate in planes perpendicular to the axis of revolution, and supposing the effect of the vibration to be a variation of the lengths of their diameters. He here arrives at an equation with partial differences of the 4th order, not summable in finite terms. Bells.

This is all that geometricians have been able to effect with regard to the problems of sonorous bodies considered in the case of two dimensions; and even introducing simplifications, which, it cannot be denied, alter the natural state of things, so that the results of analysis cannot be applicable. Hypotheses not to be admitted.

These hypothetical simplifications are particularly inadmissible in respect to vibrating surfaces of metal, or a substance naturally elastic. In the most simple case, that of a plane, it is obvious, that Euler's hypothesis of the vibration of surfaces of revolution is not applicable. We have not even the differential equations of the motion for vibrations of this kind, considering their phenomena as nature presents them; and to find these equations would be an interesting subject of meditation to geometricians, which would Euler's not applicable.

contribute

contribute equally to the advancement of natural philosophy and mathematics.

Chladni.

Happily Mr. Chladni has done for the vibrations of elastictic surfaces what Sauveur did a century ago for the stretched chord. He has discovered, and rendered perceptible in a very ingenious manner, by the arrangement dry sand takes on vibrating plates, undulations with points of rest interposed. His majesty the emperor and king, who has seen the experiments of Mr. Chladni, struck with the influence that the discovery of a strictly accurate theory, capable of explaining all the phenomena rendered sensible by these experiments, would have on the progress of natural philosophy and mathematical science, has desired the class to make it the subject of a prize, to be proposed to all the learned of Europe. The class accordingly announces it in these terms.

Prize question.

"To give the mathematical theory of the vibrations of "elastic surfaces, and compare it with experiments."

The prize will be a gold medal of the value of 3000 f. [£125], to be awarded at the public meeting on the fifth monday in january, 1812. No work will be received after the 30th of september, 1811.

Report on Mr. Chladni's Theory of Sound.

The following is an abstract of the report adopted by the class of mathematical and physical sciences, and that of the fine arts, on the 13th of february and 18th of march, 1809, on Mr. Chladni's work concerning the theory of sound.

This treatise, published in German in 1802, and about to be translated into French, contains every thing of importance in his first work, which appeared in 1787, and is enlarged by considerable additions. Under the title of acoustics, it is divided into four parts, which treat, 1, of the numerical ratios of the vibrations of sonorous bodies; 2, of the laws of the phenomena they exhibit; 3, of the laws of the propagation of sound; 4, of the physiological part of acoustics.

Number of vibrations in notes.

The first contains little but what is already known. To determine the absolute number of vibrations in a note however, Mr. Chladni does not employ a chord, but a slip of metal fixed at one extremity, and long enough to allow the oscillations it makes in a given time to be counted. Their number

number is to that of the vibrations of another slip of metal, taking place at the same time and under the same circumstances, in the inverse ratio of the squares of their lengths.

Temperaments.

In this part too Mr. Chladni treats of the temperaments proposed by different persons. He prefers that adopted by Rameau, which renders the 12 semitones included in the octave perfectly equal to each other, by making them answer to 12 geometrical mean terms between the two extremes.

Rods have three different kinds of vibration, producing different notes.

In the 2d part we find the author's discoveries. He first examines the vibrations of chords and rods, and distinguishes three sorts, the transverse, longitudinal, and those which he calls gyratory. The first take place when a chord or rod is struck in a direction perpendicular to its length. But a rod, that would produce a certain note when thus struck, would emit a very different one, if rubbed with a piece of cloth in the direction of its length. If the rod be of glass, the cloth must be wet; if of any other substance, dry. These vibrations, which he terms longitudinal, he has found subject to the same laws in a solid rod, as the longitudinal vibrations of the air in an organ-pipe; and he has given a table of these vibrations for different substances, such as glass, metal, and wood.

Notes still different from those emitted in the two preceding circumstances are produced, when a rod is rubbed in a direction very oblique to its axis. Mr. Chladni gives the epithet of gyratory to the vibrations resulting from this kind of friction, because he supposes, that the particles of the substance acquire a movement of rotation or oscillation round its longitudinal axis. He says he has found, that in these vibrations the numerical ratios are the same as those of the longitudinal vibrations, but that the tones of each rod are a fifth higher.

Experiments made with rods fixed in different ways.

Each series of inquiries abovementioned has been made with rods fixed at each end, merely supported at one or both ends, fixed at one end and supported at the other, and loose at each end. Each of these circumstances occasions a difference in the results. Mr. Chladni has likewise examined the vibrations of curved rods, forks, and rings. Euler applied the last species of vibrations to the phenomena of

of the sound of bells; but Mr. Chladni has shown very truly, that his hypotheses do not accord with nature.

Vibrations of plane & curved elastic surfaces.

The last two sections of this part are devoted to the vibrations of plates and bells, or plane and curved surfaces in general, a subject altogether new in experimental philosophy; and which, notwithstanding the striking regularity of the phenomena, has resisted the efforts of the able geometricians, who have attempted to treat on it.

Elastic plates.

Mr. Chladni has ascertained the places, which the tones we may draw from plates by giving them different forms, and by causing them to sound in different methods, occupy in the musical scale. But these inquiries are particularly interesting, when combined with those for determining the portions of each plate that have distinct and coexisting vibrations, and the remarkable curves that form their perimeters. For these experiments the plate, covered with fine dry sand, is to be held between the thumb and one finger, the ends of which press on directly opposite points of the two faces, while a bow is drawn over some point of its perimeter. Sometimes a third finger is applied at different points of one of the faces, to vary the results of the experiments. The point of support is always in one of the curves of equilibration. The figure of these curves, and their arrangement, depend on the position of the point of support, that of the point to which the bow is applied, and that of the different sounds we wish to produce by rubbing the bow in different ways on the same point. A change in either of these produces a correspondent change in the curves.

Examination of these by Paradisi.

While speaking of these curious phenomena, we cannot avoid noticing a paper inserted in the first volume of the Transactions of the Italian Institute, entitled Inquiries concerning the vibrations of elastic plates. The author, Mr. Paradisi, says in a note, that he was led to make his experiments by a passage in the Bibliotheque Britannique where Mr. Chladni's were described. Having provided an apparatus, by means of which he could keep the plates fixed at any point of their surfaces without the assistance of the fingers, he first perceived, that the curves of equilibration did not arrive at settled figures, till after a gradual and continual succession of variable figures; the generation of which, being

The vibrations go through a series of changes.

ing examined by him with great attention, led him to new inferences respecting the theory of these curves.

Examples.

Thus for example, if we take a rectangular parallelogram of glass 9 inches long and 3 broad, fix it in the line of its longer axis one sixth of its length from the end, and apply the bow to one of the longer sides of the parallelogram at one third of its length; the lines in the sand, when come to a state of rest, will divide the surface into eight equal rectangles by a right line in the direction of the great axis, and three equidistant right lines parallel to the shorter sides. But Mr. Paradisi found, that on causing the plate to vibrate by a succession of very little touches with the bow, 8 semicircles were first obtained, the centres and diameters of which were placed symmetrically on the longer sides of the parallelogram, and the point of application of the bow was one of these centres. These semicircles gradually increase: those on the same side from separate become tangents, and afterward penetrate into each other, leaving between them rectilinear lines perpendicular to the longer sides; and in proportion as these lines increase in length, the arcs flatten as they approach the greater axis of the parallelogram, with which they are at length confounded.

In other experiments Mr. Paradisi obtained whole initial circles formed on the surface of the plate, and semicircles with their diameters resting both on the longer and shorter sides of the parallelogram. The velocity of the grains of grains of sand placed in the perimeters diminished in proportion as the radii increased.

Centres of vibration and secondary centres.

Mr. Paradisi applies the term of *centre of vibration* to the centre of the circle that forms round the point to which the bow is applied, and that of *secondary centres* to those of the other circles. Supposing afterward, that when the system of curves is arrived at a fixed state, any given element of these curves is directed by the result of several forces, the actions of which emanate from these different centres of vibration, and are functions of their distances from the element of the curve in question, he arrives at a differential equation between the coordinates of this element, the summation of which would require the form of the functions, that represent the laws of the actions of the forces, to be known

known. He promises us farther inquiries on this subject in another paper.

We must refer to the memoir itself for his other experiments, among which are some interesting ones on changes in the fixed point, and in the point to which the bow is applied, without producing any in the figure or arrangement of the curves.

Vibrations of bells.

Mr. Chladni concludes his second part with reflections on the vibrations of bells, and of carved surfaces in general, and on the coexistence of vibrations in sonorous bodies. He speaks of the theory and hypothesis of Euler respecting the sound of bells; of Rameau's system of the fundamental base; of the musical system of Tartini, founded on experiments, which, according to Mr. Chladni, were known in Germany long before Tartini made use of them, and which may be considered as the inverse of Rameau's; and lastly of the combination, which takes place in certain circumstances, of the vibratory with other kinds of motion.

Propagation of sound through different substances.

In the third part the author first considers the propagation of sound as effected by the air and different aeriform fluids: he then examines the cases where it takes place through the intervention of liquid and solid substances. We here find the experiments, which the author made in concert with Prof. Jacquin of Vienna, on the vibrations of various kinds of gas; and conjectures on the cause of the difference between the observed velocity of the propagation of sound through air, &c., and that given by theory.

The committee conceive, that the two classes ought to bestow distinguished encomiums on the discoveries of Mr. Chladni respecting the philosophy of sound; and that it is an object of importance, to direct the attention and emulation of the learned to those physico-mathematical researches, to which his discoveries may give rise.

Signed, de Lacépède, Haüy, Méhul, Gossec, Gretry, Le Breton, de Prony.

Imperial

Imperial Academy at Petersburg.

The following prize subject is proposed by this academy for the year 1810. Imperial Russian Academy.

"To improve the theory of sluices, and thence to deduce rules for constructing these important works in the most advantageous manner; so that they may be used with all possible security and speed, be attended with as little expense as may be for their construction and keeping in repair, and incur no waste of the water required for the passage of loaded vessels more than is absolutely necessary." Prize question for 1810,

And for 1811. "To give a complete comparative chronology, and, if possible, corrected and verified, of the Byzantine authors, from the foundation of the city of Constantinople till its conquest by the Turks." and for 1811.

The prize for each is 100 Holland ducats [£46 5*s.*], and the answers must be sent before the 1st of July in each year.

Mr. Peter Alemani, of Milan, has analysed a new species of urinary calculus. In 100 parts he found pure magnesia 51, silex 20, phosphate of iron 11·84, carbonate of magnesia 4. The volatile substances and loss amounted to 3·16. [One of these numbers has evidently a deficiency of 10.] Analysis of a urinary stone.

Dr. G. Melandri, of the same place, is examining the artificial tannin of Mr. Hatchet, but in another point of view. His researches are on the tannin of different plants and vegetable products. He thinks, that it is not an oxide of carbon: but an oxide with a binary, or more probably ternary radical. The nitrogen of the nitric acid must enter into its composition; as must the nitrogen of the animal charcoal, since this succeeds better than vegetable charcoal. He believes too, that hidrogen enters into it, though in small quantity. Artificial tannin.

On analysing deadly nightshade, *atropa belladonna*, he discovered in the leaves a salt never before observed in vegetables, the oxalate of magnesia, joined with free oxalic acid. The other substances in them were oxalate of lime, Analysis of deadly nightshade.

muriate

muriate of potash, a soft green resin, an animal extract, mucilage, and oxigenizable extract. In the berries he found as sensible a test of acids and alkalis as the infusion of mallow flowers. By pouring alcohol on the expressed juice of the ripe berry, the purple fluid will be coagulated by the precipitation of the mucilage. This coagulum is to be well washed with the same alcohol, and the tincture filtered off. If this tincture be diluted with water till it has no longer any perceptible colour, it will become green with alkalis and red with acids. The purplish colour of this tincture changes to a yellow in time, but it still retains its property of detecting the smallest portion of acid or alkali in water.

Sensible test of acids and alkalis.

Potassium obtained in various ways.

Mr. Ritter has obtained the metallic product of potash with almost all the metallic substances yet known, when they are employed as the extremity of the negative conductor, and always fine and perfect. Arsenic alone produces it of a shining black or blackish colour. He has obtained it also by employing charcoal and plumbago as conductors: but not with the gray crystallized oxide of manganese, which is merely deprived of its oxigen in the process. When tellurium was placed in potash as the extremity of the negative wire, it did not produce bright metal of potash, but a brown dirty substance. Mr. Ritter then took tellurium for a negative wire, and immersed it in pure water in which was likewise the positive wire, and immediately streaks of a brown black were produced, which, separating from the tellurium, fell to the bottom of the water, and from the manner in which they were produced, and the place of their origin, they must have been hidruret of tellurium. Thus tellurium produced no metal of potash because it absorbs all the hidrogen itself. The button of tellurium, purified afresh, was employed as a positive wire in pure water; and, what must excite more astonishment, it remained brilliant, formed no oxide, and gave out a great deal of gas. Thus of eighteen metals subjected to Mr. Ritter's experiments, tellurium is the only one, that produced a hidruret at the negative pole; and the fourth, that with gold, platina, and palladium, gives out gas at the positive pole. Does tellurium then commence a new series of

Tellurium differs in some respects from other metals.

of metals, which comport themselves with respect to the hidrogen of water as others toward the oxigen of this fluid?

Dr. Seebeck, of Jena, has obtained indications of an amalgama with magnesia and alumine. Magnesia and alumine perhaps metallic.

Mr. Trommsdorff has prepared an artificial succinic acid. For this purpose he employs the saccholactic acid of Scheele, which he introduces into a retort and subjects to dry distillation. The products of this distillation deserve farther inquiry. Artificial succinic acid.

He has likewise examined the sulphuretted alcohol of Lampadius, and found in it several new properties. As it contains no carbon, he thinks it may be called oleous hidroguretted sulphuret. It readily dissolves phosphorus, and in large quantity; one part dissolving eight of phosphorus and still remaining liquid. This solution of phosphorus readily takes fire in the open air. In close vessels it may be decomposed by heat. The sulphuretted alcohol first passes over, though not quite free from phosphorus. Sulphuretted alcohol of Lampadius contains no carbon.

Fecula dissolved in boiling water undergoes a remarkable change, when evaporated over a moderate fire. It becomes a semitransparent horny mass perfectly insoluble in hot water. Wetted, and kept five months in a pretty warm place, Mr. Trommsdorff could not find it exhibit any signs of fermentation. Fecula changeable by heat.

Mr. Trommsdorff repeated Mr. Cadet's experiments on the solution of camphor in distilled water*. He found them accurate; but he also found, that the camphorated water is rendered turbid by pure soda, and consequently will not serve as a test to distinguish this from potash. Mr. Vogel has made some trials, that confirm this: but soda combined with a certain portion of carbonic acid does not precipitate the camphor. Camphorated water not a test of soda.

* See Journal, vol. XIX, p. 26.

METEOROLOGICAL

METEOROLOGICAL JOURNAL,

For NOVEMBER, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

OCT. Day of	BAROMETER, 9 A. M.	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	WEATHER. Day.	Night.
27	30·28	50°	50°	52°	47°	Fog	Foggy
28	30·28	48·5	50 5	53	46	Ditto	Heavy fog
29	30·26	47	49	51	46	Ditto	Rain
30	30·22	48	49·5	52	46	Ditto	Cloudy
31	30·11	48	46·5	50	41	Fair	Foggy*
NOV.							
1	30·10	49	44·5	52	42	Cloudy	Rain
2	30·20	45·5	47	49·5	39	Fair	Cloudy
3	30·14	44	45	47·5	40	Rain	Rain
4	29·93	44·5	45	46	40	Ditto	Ditto
5	29·84	42·5	42	45	37	Ditto	Cloudy†
6	29·92	41·5	43	46	37	Ditto	Rain
7	30·06	42	45	47	40	Cloudy	Fair
8	30·39	42·5	43	46	40	Fair	Ditto
9	30·42	43	46·5	47·5	40	Cloudy	Cloudy
10	30·29	47	47·5	50·5	43	Ditto	Ditto
11	30·15	44·5	43	50	49	Ditto	Ditto
12	29·91	44	44	46	41	Ditto	Ditto‡
13	29·74	43·5	44	46	36	Ditto	Ditto
14	29·72	38	46·5	47·5	34	Fair	Rain
15	29·70	36	35	39·5	29	Ditto	Fair
16	29·71	31·5	34	37	32	Ditto	Ditto§
17	29·48	39	36·5	45	30	Rain	Ditto
18	29·73	34	36	38	30	Snow	Ditto
19	30·23	33	30	37·5	26	Fair	Ditto
20	30·43	28·5	34·5	37	36	Ditto	Ditto
21	30·28	38	35·5	41	36	Ditto	Ditto
22	30·16	39	45·5	49·5	41	Ditto	Ditto
23	30·03	43·5	47	49	40	Rain	Rain
24	29 39	42	43	47	39	Ditto	Cloudy
25	29·78	38	40	42	37	Fair	Ditto
26	29·10	42	38	43·5	32	Ditto	Fair

* At 6 P. M. stars visible; at 9, heavy fog; at 11, starlight.
† At 11, starlight and clear.
‡ Commencing rain at 10 P. M.
§ Snow in the night, the morning milder.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXIV.

ARTICLE I.

Memoir on the Triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. LE COMTE DE BOURNON, *F. R. and L. S.*

(Concluded from Page 260.)

PART II.

Observations on Endellion, as being the Result of a triple Combination, and on the different Sulphurets of Copper.

THE Royal Society has printed in the first part of the Philosophical Transactions for 1808 a paper*, in which the constituent principles of endellion, as well as the manner in which they combine, are discussed †. The author

Paper on endellion in the Phil. Trans.

* See Journal, vol. xx, p. 332.

† *Additional note.* I mean Mr. Smithson's paper already mentioned. The Royal Society having printed a critique on the crystallographical part of my first memoir on endellion, however I might feel hurt by the style of that critique, I thought it better not to notice it, than to expose the transactions of that illustrious and respectable body to be made the scene of a dispute, which certainly could not be more misplaced. I therefore presented my new memoir on endellion to the Royal Society, merely as the result of continued and extended observations,

Doubts of the existence of higher than binary combinations.

author there professes his doubts of the existence of triple, quadruple, and greater combinations; and his opinion, that all combinations are binary. In consequence he endeavours to refer to one of the latter the nature of the compound, that gives rise to endellion; considering it as formed by the intimate combination of sulphuret of lead, or galena, and that kind of copper ore, which the Germans call fahlertz. I cannot conceive on what reasons the author grounds his opinion, that there can be no triple, quadruple, or greater combination. On the contrary the possibility of these combinations seems to me demonstrated by the simple facts, that I have already brought forward in the second volume of my mineralogy, p. 390, in order to show, that the molecules of bodies, considered as principles of minerals, possess, as well as the integrant molecules which result from the combination of these, the property of having a regular figure. The act of combination of these molecules, in

Ultimate particles of bodies have a regular figure.

observations, which had enabled me to make this substance, which is peculiar to England, more thoroughly known, and to render my account of it more complete.

Combinations more than binary may exist.

The second part of my paper was intended to include some reflections on a fact highly interesting both to the mineralogist and the chemist, which is the possibility or impossibility of the existence of triple, quadruple, and other combinations in the mineral kingdom. Mr. Smithson, in one part of his paper, sought to establish the principle, that all combinations could only be binary, and adduced endellion in confirmation of his opinion. After having laid down the reasons, that seem to me to preclude all doubt of the possibility of more than binary combinations, it was necessary for me to show the weakness of the argument deduced from endellion; which could not be made to answer this purpose without giving an arbitrary proportion of the component parts of the two sulphurets, the binary combination of which produced it, or at least a proportion different from that usually admitted by chemists. I confess, however, that, had I not found occasion to answer the critique included in the same paper, it would not probably have been in the Philosophical Transactions, that I should have pointed out this obvious error. However, if the committee of the Royal Society had requested me, to suppress this part of my paper, I should not have hesitated a single moment to comply with its wishes, however interesting I conceived it to be.

forming

forming the integrant molecule, which is the immediate result of this combination, differs then in no respect from that, which afterward unites the integrant molecules. Now it is very easy to conceive, and even to adduce a number of instances of the formation of a crystal of any determinate figure (representing the integrant molecule of a compound substance) that shall be composed of the intimate union of three, four, or even more crystals of different forms, which in this case would represent the molecules of the substances that compose it; and which would enter into the composition of the crystal in equal numbers, or, which is more commonly the case, in unequal numbers. It is indeed to the property, which the molecules of minerals have, of uniting intimately with each other, so as to produce a new molecule of a determinate figure, that I attribute in part the formation of those minerals, which are commonly said to be the effect of chemical combination. Every combination of the substances of which a mineral is composed seems to me to require, that the form of the molecules of each shall bear such a relation to those of the rest, that their faces may respectively coincide, so as to produce collectively a molecule, the form of which shall be at once determinate and invariable. It is this relation between the several component molecules, which in all probability determines their action upon each other, known by the name of "attraction of composition;" or which is at least a principal cause of this. Upon the same principle we may account for the proportion of the several substances, which must necessarily vary, according to the number of these molecules, the forms of which are different, and the mode in which they arrange themselves, so as to produce a new molecule, the form of which shall be determinate. When the molecules are wholly dissimilar, and there is no relation whatever between their faces, it is not possible for them to combine, so as to generate a new substance properly so called and capable of crystallization.

Particles unite to form secondary particles of a different figure in chemical combinations.

The real existence of these triple and greater combinations is farther demonstrated by facts. We know, for instance, that there exists a substance which differs from gypsum, or the combination of lime with sulphuric acid and water,

Gypsum the result of a triple combination.

water, only in not containing of the last of these three constituent principles. Yet this simple privation, by producing a substance of a different form, harder, heavier, and possessing different chemical properties likewise, shows, that water, which combines as a principle with lime and sulphuric acid in the formation of gypsum, is necessary to its formation, and that gypsum is consequently the result of a real triple combination.

Objection. It may be objected indeed, that in gypsum there exists only a double combination between the molecules of sulphate of lime on the one hand, and those of water on the other.

Answered. But, if this were the case, when the water had been expelled from the gypsum by calcination, the sulphuric acid being left, there should still be an intimate combination between the molecules of the sulphuric acid and those of the lime: and the plaster, which results from this calcination, should exhibit a substance precisely of the same nature as that known by the name of anhydrous gypsum, or bardiglion (as I call it,) whereas in fact there is no similitude whatever. The bardiglion reduced to powder, either before or after calcination, possesses none of the properties of gypsum, and does not absorb any water whatever, so as to combine with it, and thus acquire the solid form. By the great avidity with which calcined gypsum seizes on water, the moment it comes into contact with it, we discover that calcination, by taking from each of the integrant molecules of gypsum (composed of those of sulphuric acid, lime, and water) the molecules of water, has changed the integrant molecules of this substance into new molecules, consisting simply of those of lime and acid, but having only an incomplete form: or, if I may be allowed so to express myself, we perceive that this calcination has carried away a part of each of the integrant molecules of gypsum, and left in each one or more cavities, the sides of which, having a very powerful affinity for the corresponding sides of the molecules of water, seize on them as soon as they have an opportunity of so doing, and fix them again in the places to which they belong. Gypsum is then in fact the result of a triple combination.

But

Endellion more probably a quaternary than a ternary combination.

But is it certain, that the integrant molecule of endellion is simply the result of a triple combination of the integrant molecules of the three sulphurets of lead, copper, and antimony? and is it not more natural to consider it as the result of a quadruple combination of the molecules of sulphur, lead, copper, and antimony? I confess, I am much inclined to consider endellion in the latter point of view, particularly when I observe, that the sulphur, which from the proportion in which it exists in the three sulphurets should amount to 20·03, forms only 17 hundredths of this substance.

Composition of endellion, as given by Mr. Smithson.

In referring endellion to the binary combination of galena and fahlertz, the paper above quoted gives the following proportions.

$$\text{Endellion}\begin{cases}\tfrac{1}{2}\text{ galena}\begin{cases}\tfrac{1}{6}\text{ sulphur}\\ \tfrac{5}{6}\text{ lead}\end{cases}\\ \tfrac{1}{2}\text{ fahlertz}\begin{cases}\tfrac{1}{3}\text{ sulphuret of antimony}\begin{cases}\tfrac{1}{6}\text{sulphur}\\ \tfrac{5}{6}\text{antimony}\end{cases}\\ \tfrac{2}{3}\text{ sulphuret of copper}\begin{cases}\tfrac{1}{3}\text{sulphur}\\ \tfrac{2}{3}\text{copper}\end{cases}\end{cases}\end{cases}$$

From these proportions it would follow, that the sulphuret of lead contains $83\tfrac{1}{3}$ of lead, and $16\tfrac{2}{3}$ of sulphur: that of antimony $83\tfrac{1}{3}$ of antimony, and $16\tfrac{2}{3}$ of sulphur: and that of copper $66\tfrac{2}{3}$ of copper, and $33\tfrac{1}{3}$ of sulphur.

Those of the sulphurets of antimony and copper differ from other authors.

The proportion in which the sulphur is said to combine with the lead in galena, or the sulphuret of this metal, is the same as is given by Mr. Kirwan. As to the two sulphurets of antimony and copper, the proportions between the sulphur and the metal result probably from the author's own observations, which it is much to be regretted that he has not given.

Proportions in sulphuret of antimony.

According to Proust and Bergman, the only two authors, as far as I know, that have established the proportions of sulphur and antimony in the sulphuret of this metal, the sulphur is to the antimony in the proportion of 26 to 74; and this is the proportion hitherto followed by all writers.

Proportions in sulphuret of copper.

They have in like manner followed the proportion affixed by Klaproth to the sulphur and metal in the sulphuret of copper, which is that of 18·5 to 78·5; and with respect to this sulphuret I will add, that, about the time when I laid before

before the Royal Society my first paper on endellion, being desirous of obtaining some additional data with respect to the proportion in which sulphur enters into the sulphuret of copper, I requested the favour of Mr. Chenevix to assist me in my researches, by analysing different varieties of the sulphuret of this metal, with specimens of which I furnished him. This able chemist found in one perfectly pure Cornish specimen, which was regularly crystallized, 19 of sulphur and 81 of copper; a proportion exactly similar to that given by Klaproth, if we consider that the sulphuret of copper analysed by him contained 2·25 of iron, and 0·75 of silex, which did not exist in that analysed by Mr. Chenevix.

Analysed by Mr. Chenevix.

This gentleman at the same time, at my request, took the trouble to analyse seven other varieties of sulphuret of copper; I having supplied him with the specimens, duplicates of which I preserved. One of them only was from Bohemia, and the rest from Cornwall. All of them contained from 0·03 to 0·08 of iron, but no other extraneous substance. They all gave similar results as to the proportions of sulphur and copper, except that the quantity of sulphur was a little greater where the quantity of iron was greater.

Other specimens analysed by him.

Thus it appears to me incontrovertible, that the proportion of sulphur, admitted in the paper in question as essential to the composition of sulphuret of copper, is much too great; and that the proportion of sulphur, there said to enter into the composition of sulphuret of antimony, is too small; which would totally overturn the proportions, by which the author of that paper endeavours to prove endellion to be produced by a binary combination between sulphuret of lead, and the gray sulphuret of copper named fahlertz. It is possible, for this sometimes happens in metallic sulphurets, that Messrs. Klaproth and Chenevix, in the specimens they analysed, may accidentally have met with varieties of sulphuret of copper, in which sulphur was superabundant, or simply interposed; and in this case their analyses will give too large a proportion: but that two chemists, so eminent for their talents as the gentlemen just mentioned, should constantly find the proportion of sulphur in this sulphuret much less than is assigned in that paper,

Mr. Smithson's proportions erroneous.

per, agreeing at the same time with respect to the proportion in which it enters into this compound, is I believe as complete a demonstration, as chemistry can furnish: at least if there be any errour in it, the errour must be proved.

The majority of ores in the sulphuretted state, of which copper constitutes a part, being of a gray colour; these ores being very numerous; in some of them the copper being merely interposed; and in the greater number of those of which it is a component part, being commonly intermingled with different metals, most of them sulphuretted likewise: nothing is more difficult, than to distinguish these ores, so as to refer each to the principal and particular type, to which it belongs.

Ores of copper easily confounded.

Among the different species, to which these ores may be referred, it has uniformly appeared to me, that what the Germans call fahlertz belongs to the gray species that crystallizes in regular tetraedra. But this species, in which the copper in the sulphuretted state is in pretty large quantity, is at the same time one of those most subject to admit foreign substances by the interposition or juxtaposition of their molecules. As I have already said in my first paper on endellion, presented to the Royal Socisty, it appears to me unquestionable, that the essential component parts of the gray tetraedral sulphuret of copper are copper, iron, and sulphur; and the analysis, which I have there said was made by Mr. Chenevix of a variety from Cornwall in well defined crystals, in which he found nothing but these, in the proportions of copper 0·52, iron 0·33, and sulphur 0·14, seems sufficient to prove that these substances are thus proportioned in this ore.

Fahlertz.

Very liable to extraneous mixture.

The author of the paper, when he gives fahlertz as the second component part of endellion, considered as the product of a binary combination, says that it is composed of $23\frac{1}{3}$ sulphur, 50 antimony, and $26\frac{2}{3}$ copper; a composition that would constitute a variety among the antimonial ores, and has nothing to do with that I have just given. But as it differs materially from all the very numerous analyses, that have been made of different varieties of gray copper containing antimony, it must be the result of the author's own observations, and a particular series of experiments, which

Mr. Smithson's fahlertz a new variety among the antimonial ores.

which it would have been of great importance to make known.

Examination of fahlertz.

The attention which the different sulphurets of copper appear to me to deserve, as opinions respecting them are not yet settled, induces me to add to what I have said on the fahlertz, or tetraedral sulphuret of copper and iron, the following reflections. Part of them have already appeared in my first paper on endellion; but the observations which I have subsequently had an opportunity of making on the sulphuret of copper enable me to present these reflections to the Royal Society again on a larger scale, and in a manner better adapted to illustrate this interesting subject, on which so much uncertainty prevails.

Extraneous substances mixed with it.

The substances foreign to fahlertz, or tetraedral gray copper, which observation has hitherto shown to be interposed in it, are silver, lead, antimony, and arsenic; and very frequently these substances appear to be sulphuretted, as well as the copper. An analysis by Klaproth of a variety from Kapnick, in Hungary, has even indicated 0·06 of zinc; and another of a variety from Poratsh, in Upper Hungary, 0·0625 of mercury. These substances, several of which are sometimes found in the variety, that possesses the property of crystallizing, are no obstacle to crystallization: but there are many other varieties, that appear to be destitute of this property. In spite of all the researches I have been able to make on this subject, I cannot establish in a satisfactory manner the characters, that might serve to make them known: in general the gray colour of the crystallizable variety is I think less deep, and its lustre more brilliant, but to this there are many exceptions.

All the gray sulphurets of copper liable to foreign mixture.

The fahlertz is not the only sulphuret of copper, that is subject to this interposition of foreign substances; it is the same with all the gray sulphurets of this metal. The various mixtures they contain has even occasioned different names to be given them, and different opinions to be entertained respecting their nature. From the silver, which some of its varieties contain, fahlertz had long the name of *gray silver ore*. Afterward, when it was found, that a great number of other varieties were totally devoid of silver, it was called *gray copper ore*. An analysis, which Klaproth made of a variety

variety from Andreasberg in the Hartz, having afforded him 0·34 of lead, occasioned its removal from the copper to the lead ores; among which many German mineralogists continue to class it, expressing doubts however, respecting the nature of its real component parts. Nothing is more variable than the analyses, that have been made of different gray sulphuretted copper ores, taken even among those that are crystallized; and it is absolutely impossible to found upon these any determinate classification of the species and varieties of this ore, unless we previously establish certain fixed points, to which we may refer them.

Division of the sulphuretted copper ores.

In this state of things I conceive we ought to consider the sulphuretted copper ores in two different points of view; the first regarding those that admit a determinate form; the second, such as have not hitherto appeared to admit any, as the weissgueltigertz and graugueltigertz of the Germans, &c.

Distinction of the species.

Among such of these ores as take a determinate form we ought, I think, to consider as belonging to one species those, that constantly take the same primitive form, or some one of its modifications. At the head of this we should place, first, the simple sulphuret of copper, composed of 0·81 copper, and 0·19 sulphur. Its primitive form is a right hexaedral prism, the terminal faces of which are regular hexagons *, and the specific gravity of which is 5.643.

Simple sulphuret of copper.

* My intention is soon to lay before the Royal Society a more complete detail, relating directly to these copper ores, in which the form and dimensions of these crystals will be established.

Addition. When I entered into this engagement, I was not aware of the fate, that awaited the paper in which it was inserted.

Primitive crystal of sulphuret of copper.

The height of the regular hexaedral prism, forming the primitive crystal of the simple sulphuret of copper, is to the apothema § of the regular hexagon, that serves as its base, in the ratio of 2 to 3, a ratio determined from three different substitutions for the edges of the same hexagons, the planes of which make with the terminal faces for the first an angle of 146° 19′, for the second 138° 22′, and for the third 116° 32′. Frequently all the planes owing to these three modifications terminate the same prism. Often too they reach each others limits, and then give rise to as many hexaedral pyramids, either terminating

§ *The apothema is a perpendicular line let fall from the centre of the circle in which the hexagon is inscribed, and bisecting any one of its sides.*

5·643 *. This species, when it is perfectly pure, and without any mixture of iron, may be cut nearly with as much ease as lead, of which it has almost the colour. It cuts perfectly smooth, and with a metallic lustre. This sulphuret alters spontaneously to a deep black by the oxidation of its surface.

Variegated copper ore.

2d, The double combination of copper and iron with sulphur, known by the name of *bunt kupferertz*, which was given it by the Germans, and the composition of which appears to be from 0·60 to 0·65 copper, 0·18 to 0·15 iron, and 0·22 to 0·25 sulphur; proportions deduced from the analyses of ten different specimens, made by Mr. Chenevix at my request. Its primitive form is a cube †; and its specific gravity

terminating in a point, or very nearly so. Frequently these pyramids touch each other at their bases, when, the prism disappearing, they give rise to three different dodecaedra with isosceles triangular faces. In one of these dodecaedra, the planes meet at the summit at an angle of 112° 38′; in another at one of 96° 52′; and in the third at one of 53° 8′. These crystals are almost peculiar to Cornwall; every where else the crystals of sulphuret of copper are very rare.

* This specific gravity was given by two very perfect crystals united together, weighing about 258 grains, and perfectly pure. Authors that have mentioned this substance give its specific gravity from 4·810 to 5·338. Beside that the specific gravity of no substance can vary to such a degree, the greatest is certainly below the truth; it was probably taken from some of its amorphous varieties; and a great number of trials with these has taught me, that their specific gravity varies considerably; and never equals that of the crystals. No doubt this is owing to cavities in their interior, which in fact may be frequently seen on breaking pieces of this sulphuret. In weighing this sulphuret of copper too, we should not take crystals that are grown black or oxided on their surface; the black oxide of copper not being easily permeable to water, there always remains in this case a great deal of air between the surface of the piece of sulphuret and the water in which it is weighed; and as we cannot entirely free it from this, the specific gravity obtained is always much less than it ought to be.

Crystals rare.

† *Additional note.* The crystals of bunt kupferertz are very rare. Cornwall, which has furnished mineralogists with so many scarce species of this metal, has produced some very fine groupes, though but few. The cube, which is the primitive crystal of this substance, is the form

gravity is 5·033. It is not so easily cut as the preceding; and the cut, though smooth, has not the same lustre. Its most common colour has the redness of nickel, which is deeper where cut; but if this sulphuret be ever so little decomposed, it acquires a blueish tint, and afterward assumes the finest colours.

3d, The double combination of copper and iron with sulphur, but with a larger proportion of iron than the preceding species. It is the fahlertz, or gray sulphuret of copper and iron; the composition of which is copper 0·52, iron 0·33, and sulphur 0·14; and the specific gravity 4·558. This species is much harder than the preceding; but its hardness varies according to the nature of the different substances frequently interposed in it. It may be scratched, but not cut; and the place scratched has neither the smoothness nor the lustre of the two foregoing species. Its powder varies from a full black to a black with a reddish cast, or a reddish brown. The latter colour, as far as my observations go, always indicates the presence of silver, which is commonly in the state of red antimoniated silver. It is much Fahlertz.

form its crystals most commonly assume. Sometimes the places of its solid angles are occupied by equal sided triangular planes. Very commonly these cubes have their faces slightly curvilinear. At other times they are merely an aggregation, frequently irregular, of other small cubes, which renders their figure very difficult to discriminate. Chiefly cubical.

To the bunt kupferertz no doubt should be referred the cube, which Werner, Estner, and several other German mineralogists, give as one of the forms of the simple sulphuret of copper, to which it appears to me incapable of belonging. As to the octaedron, given likewise by the same authors to the simple sulphuret of copper, to which it is equally far from belonging, I presume, that some octaedra of red oxided copper, oxigenized to a maximum at the surface, and turned black to a less or greater depth, may easily have led to the mistake. Formerly there occurred in Cornwall a variety of bunt kupferertz in thin laminæ superimposed on one another, and frequently of a fine blue colour at their surface. This contained iron in smaller proportion than the bunt kupferertz, but sufficient to render the sulphuret of copper incapable of being cut with the knife, and when cut exhibiting the metallic lustre as the simple sulphuret of copper. The fracture presents a coppery red colour. Variety.

more

more liable to decomposition than either of the preceding, particularly when crystallized.

Copper pyrites. 4th, The double combination of copper and iron with sulphur, which is shining and of a deep yellow colour. We have no analysis of this species, except that of Lampadius, who gives for its component parts 41 copper, 17 iron, and 45 sulphur: but it is very probable, that the specimen analysed by him contained a superabundance of sulphur interposed in its substance; and besides, the proportion of iron given by this analysis is certainly too small*.

From several assays of this copper ore made with Mr. Chenevix, it always appeared to us, that it differed very little, either in its component parts, or in their proportions to each other, from the gray species, which I have said should bear the name of gray sulphuret of copper and iron. The form of its primitive crystal is a regular tetraedron, modifications of which it sometimes admits, though much fewer than the gray sulphuret of copper and iron; and among which we chiefly find the regular octaedron, and the dodecaedron with rhombic faces; but the latter variety, which occurs in Cornwall, is very rare. The specific gravity of this sulphuret is 4·058 ‡. It is not so hard as the fahlertz. Its fracture is very brilliant, ragged, and as if it were composed of small laminæ intersecting each other in various directions. In decomposition it assumes the most lively

* *Additional note.* I have lately seen in the Journal des Mines, No. 122, that Mr. Gueniveau, engineer of mines in France, has analysed two varieties of yellow sulphuret of copper and iron: One, from St. Bel near Lyons, afforded him metallic copper 30, metallic iron 33, and sulphur 36. The other, from Baigorry, yielded metallic copper 27·5, metallic iron 29·5, sulphur 31·5 §.

‡ This specific gravity is a mean of those of four tetraedral crystals, either perfect, or with their solid angles truncated. Authors have hitherto carried this specific gravity to 4·315: but I presume, that it was not taken from crystals, and that the pieces weighed were mingled with sulphuret of iron, which frequently happens. I have found yellow sulphurets of copper and iron, thus mingled, weighing as high as 4·6.

§ See Journal, vol. xxi, p. 148.

lively colours, till at last it loses great part of the copper it contained; and which very frequently in this case, combining with carbonic acid, passes to the state of green copper, leaving a residuum of oxide of iron, which however is still sometimes pretty rich in copper, and is then known by the name of hepatic copper ore.

Care must be taken not to confound this double sulphuret of copper and iron, as is frequently done, with the martial pyrites that contains copper intermingled with its substance, commonly in small quantity, though it is sometimes pretty rich in this metal. From this the double sulphuret is totally different. The form of the martial pyrites containing copper is either a cube, and this commonly striated, or a regular octaedron. The martial pyrites is likewise much harder than the yellow sulphuret of copper and iron, and it is heavier, its mean specific gravity being 4·944*. It must appear very strange, that this sulphuret, having great analogy in its component parts, as well as in its form, with the gray sulphuret, or fahlertz, should have a colour so very different from it, as well as from all the other sulphurets of copper. Endeavouring to account for this, I have always been led to think, that this difference of colour might arise from the iron's being in the perfectly metallic state in the yellow sulphuret of copper and iron, as it is in the martial pyrites, while in the gray sulphuret it is oxided. This opinion however I only mention as a great probability †.

This not to be confounded with the iron pyrites in which copper is intermixed.

Cause of the difference of colour.

In

* This specific gravity is a mean of those taken from crystals all of different forms. Authors give for it from 4·100 to 4·749. Certainly however they have not taken it in the same manner from crystals, but from amorphous masses; or at least it must have been from very impure crystals, otherwise they would not have varied from 4·1 to more than 4·7; and it would even have been found superior to this maximum.

† I have observed with the greatest satisfaction, that the opinion I had long embraced respecting the cause of the difference of colour between the yellow sulphuret of copper and iron and the gray, and which was inserted in my first paper on endellion presented to the Royal Society, has been verified by the analyses made by Mr. Gueniveau of two varieties of yellow sulphuret of copper and iron

from

Another species.

In this case perhaps it would be necessary to make a 5th species among the sulphurets of copper of an ore, which was formerly very plentiful in Cornwall, but is now become rather scarce, and which probably differs from the preceding species by a more or less considerable degree of oxidation in the iron. This ore is of a dull yellow colour, inclining a little green. Its fracture is smooth and dull, and sometimes a little conchoidal. Its grain is extremely fine, and frequently even imperceptible to the eye. Its texture consists of parallel layers, very thin, and distinguishable only by the assistance of a lens, but easily separated by a stroke of the hammer. This ore has never exhibited to me any crystalline form; but it is frequently mamillary, much like the martial hematites. Its surface is commonly smooth, a good deal like that of a metal which has lost its polish. Its specific gravity is 4·157: consequently a little greater than that of the preceding yellow sulphuret of copper and iron. Its hardness is nearly the same. If scratched with a knife, the part scratched appears smooth, and acquires a metallic lustre. On decomposition the surface frequently assumes various colours, but less lively and brilliant than those of the lamellar yellow sulphuret. At other times its surface grows black from the oxidation of the copper, having a good deal the look of an antique bronze, and the more so as it is often partially covered with malachite. This species is frequently mixed with simple sulphuret of copper, a phænomenon by no means so common in the yellow species which I have just mentioned above.

This division proposed as a standard.

This division of the sulphurets of copper, being once adopted, might be considered as a standard, to which we might refer all the numerous varieties, that exhibit no marks of crystallization; arranging them under one or other of these species, according to the manner in which their essential component parts are proportioned. We might then

from St. Bel and Baigorry, which I have noticed. In these analyses given in the Journal des Mines, No. 122, the author says expressly, that the iron in these yellow sulphurets was in the metallic state; while in two other analyses made of varieties of the simple sulphuret of copper from Siberia, which were probably in amorphous masses and contained iron, he says the iron was in the state of oxide.

place

place in a second division such as appear not to agree with any of those already known and classed among the species properly so called; and this division may be subdivided at pleasure, as may appear necessary for the establishment of order and perspicuity.

Extraneous mixtures do not alter their nature, but may make subdivisions of varieties.

It is obvious, that in fact, the species existing among the sulphuretted ores of copper being perfectly known from the sum of the characters essentially necessary to ascertain them, the silver, lead, antimony, arsenic, &c., which happen to be intermingled with them, are perfectly extraneous, and do not in the least alter their essential nature. These intermingled substances once known, they may give rise to subdivisions of varieties; but these subdivisions themselves would become very numerous, if proper limits were not assigned to them. In the fahlertz, for example, from the great tendency it has to receive into its substance an intermixture of a great number of others, I am persuaded, that we should be obliged to make almost as many subdivisions as we analysed specimens.

New variety.

A collection of minerals I lately received from Russia convinces me, that we are yet far from knowing all the gray sulphuretted ores, of which copper forms a component part, or in which it is simply interposed or accidental. Among the specimens in it was one bearing the name of a substance, which certainly did not belong to it; and the appearance of which, differing from that of every analogous substance that I recollected, particularly caught my attention. As this specimen affords a new and interesting variety of the simple sulphurets of copper; and affords me an opportunity of showing how we may sometimes discover, that a substance is simply intermingled with another, and not combined with it, a point frequently difficult to determine; I will enlarge upon it for a few moments.

Described.

This substance, which is in small separate pieces interspersed in a quartz, partly compact and partly lamellar, is of a fine, close, compact grain, and of a hardness nearly equal to that of fahlertz, or gray sulphuret of copper and iron. Its colour is a duller gray, and its fracture is more smooth. Its specific gravity is 4·554. Well assured that this substance was not nickel, under the name of which it had

had been sent me, I requested Dr. Wollaston, to have the goodness to ascertain its nature. His examination informed him, that it contained nothing but sulphur, copper, and antimony. Desirous of ascertaining if possible, whether the antimony were combined with the copper in it, or simply intermingled with the sulphuret of this metal; and this ore being soluble, though very slowly, in cold nitric acid; I first of all dissolved it in this acid. A part only of the sulphur rose to the surface of the solution; and it is probable, that the rest was converted into sulphuric acid. The copper dissolved entirely, and the antimony was precipitated in the state of oxide. The latter, to judge from the size of the specimen I had set to dissolve, was evidently in smaller proportion than the copper. As this same substance is extremely fusible, I brought a thin piece, about four lines long, to the state of fusion, and kept it so for a short time. Great part of the antimony sublimed, covering the surface of the body on which it rested with a white powder. The fragment when cooled retained its form, and even its bulk. On breaking it afterward, its fracture exhibited an aspect exactly resembling that of the sulphuretted copper which is produced by the last fusion, it could be cut with the same facility, and the cut had a metallic lustre. Having afterward placed this fragment, which had been fused, in cold nitric acid, and a fragment of simple sulphuret of copper along with it by way of comparison, they both dissolved slowly, comporting themselves exactly in the same manner, and the solution contained nothing but copper. The solution of each of these fragments produced the same black, flocculent, and very light precipitate, which was nothing but sulphur still united with a small portion of copper, which, no doubt, was the cause of its black colour. From these details it appears to me there can be no doubt, that the ore was a simple sulphuret of copper, with which antimony, probably in the state of a sulphuret likewise, was intermingled. This substance came from Bojojawlensk, near Catharinenbourg, in Siberia.

Analysed.

II. On

II.

On the Effects produced by the grafting and budding of Trees. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

WHEN I first began the study of grafting and budding by dissection, in order to judge of the effect produced in trees by such an operation, it was my design to collect all the knowledge disseminated in every author on the subject, and by joining it with what I should attain by dissection myself, offer to the public a treatise, that might at least serve as a sort of standard of our knowledge in this art. After dissecting therefore an innumerable number of grafts and buds of every different tree, commonly grafted and budded, and at different distances of time, making drawings as exact as possible, and committing my own observations to paper; I was anxious to see what other botanists had said on the subject. But great was my astonishment to find, that scarce any author had really investigated the matter. Even Miller gives only a few common rules for practice, without observations. The scientific Mirbel (in whose work I hoped to find important information) gives only a short note, and a reference to Duhamel; Malpighi and Grew are totally silent; and Dr. Smith and Willdenouw are equally neglectful of the subject: yet it appeared to me to be in every respect that which promised the most important instruction with regard to the manner in which trees are formed, to show the process of each different part, and produce evidence which no other situation of the vegetable world is capable of giving. Nor was I deceived, I think; for by this sort of dissection I have learnt more of the real nature of the different parts, than any other investigation of plants ever taught me; since they are brought forward in a state, that obliges them to exert their powers, and much may be drawn from the curious struggle for life, which points out to notice every important part.

Design of the author.

The subject not investigated by writers.

Yet highly important.

Finding that from Duhamel alone I was to expect any information, that was not merely practical: I with great trouble

Duhamel's book.

trouble at last procured his work. It is indeed a book full of most admirable instruction, and though it has but little beyond practical observations on this subject, yet, as far as he proceeds in investigating causes, it is excellent: but here again the want of the opake solar microscope has prevented the possibility of his proceeding farther, and knowing whether the parts did, or did not unite. This being the case, I must trust wholly to myself for the anatomical part, and the consequences arising from them; but as I shall give a sketch of the exact change made by the uniting of the two branches. I hope my figures will prove the truth of my assertions.

Object of grafting and budding.

The use of grafting and budding is to propagate any particular tree: the wood or fruit of which pleases the eye or palate; as the only means we possess of procuring a perfect imitation. It is a well known fact, that seeds seldom produce the exact prototype of the plant from which they spring; and the reason is plain; the tree is only the mother of the blossom, but to complete the resemblance, it must be impregnated by the stamen of the same plant. Now if the wind blows the ripened dust of the stamens from a neighbouring tree of the same order, when the blossoms of the first are covered with the juice of the pistil, the blossoms thus prepared will receive the powder, and the consequence will probably be a new variety of fruit, if the seed is planted; and not the same fruit which the original tree gave. Thus are produced more than half our sorts of apples, peaches, and innumerable flowers; for we neglect to examine the origin of the varieties that take place in our gardens, or we should continually be able to trace them to this source.

Why seedling trees are commonly varieties.

Budding and grafting merely continues the same tree.

But in grafting and budding, this cannot be the case, no resemblance can be so exact; it is indeed merely an increase of the tree, from which the scion is taken. Every one knows, that grafting is taking a shoot from one tree and inserting it into another, in such a manner, that both may unite closely, and become one tree. Mr. Bradley (from some observations of Agricola) suggests what anatomising the parts first proves, that the stock serves merely as pipes to convey the medicated nourishment to the scion; that the scion

scion preserves its natural purity unmixed or uncontaminated with any other juice: and, as we have since discovered, that all sap is nearly the same, being merely the juices of the earth; and that it is the blood which runs in the bark alone which gives taste and variety to the tree; so the scion can in no way be altered by the stock, without the juice of the bark runs into it. Now it is most certain, that this is not the case; for the barks never join so as to communicate juice, as I shall show when I describe the alteration made in the parts in contact. The scion or bud (for it is the same in both) is placed on the graft or stock as in the earth; but, instead of having to prepare its own sap (which the infant plant is obliged to do before its root grows) it finds this ready medicated for it, and fitted for its more advanced state, to push it forth with a vigour truly wonderful. To graft is merely therefore to form a tree in a far quicker manner than a seed could; and in very delicate plants to take away from them all the dangers attending their infant state; and at once place them in maturity. This is the real object of grafting and budding.

No union of the vessels of the bark in graftings.

I cannot agree with Mr. Foresyth, that the juices, when they arrive at the scion, must have a more easy, plentiful, and perfect assimilation, than if they were its own; for the scion can only do well, when the vessels of the stock agree perfectly with its vessels. Its own cylinders must therefore more perfectly assimilate than any stranger tree can. Still, if the wood vessels suit each other, we may be satisfied that the plant will do well. This is indeed of such extreme consequence, that of the number I have examined which I had budded and grafted for trials of various trees, more than thirty have died from the difference of the size of the two woods; for does it not stand to reason, that, if you try to force a quantity of water from a large vessel into a small one, it will burst the small one: or, if it is the small one, that pours its contents into the large one, it will not half fill it, and the vessel will be pressed with too much air, that will pour in to supply the place of water; and the vessel will equally burst. This is literally the case in the two woods that are to be joined together, whether in plethory or emptiness of sap, they equally burst, but in the

Juices of the stock not more agreeable to the scion.

first,

first, the whole is wet and full of juice; and in the latter, the parts are shrunk and dried up.

Grafting and budding similar in effect, but budding preferable.

I must now mention, that, though grafting and budding are so very differently performed, yet in their effect they are perfectly the same; the first being the planting several buds, the latter only one: but there are some reasons, that make the last infinitely to be preferred, where it can be done: and I doubt not it succeeds much oftener, as I shall show at the conclusion of this letter. I shall now proceed to the alteration effected in the bud and graft by the operation.

Effects of grafting.

The first observation on cutting a graft, after it has been done about two or three months, is, that all the part between the two plants is filled with a moist substance; which upon magnifying you perceive to be the same wood as the scion, only loose, and incomplete. Next a white line is seen struggling through this loose wood, and soon reaching in a very undulating manner from the stock to the scion. It always begins at the stock. To perfect this line, which is the circle of life, six weeks are required. I never saw it perfected in less.

Junction of the barks.

The next is the formation of what Duhamel calls the bourrelet, that is a species of bolster of new bark, formed from the old juice of the stock, which, being prevented from continuing its course to form new bark, runs down the division of the two branches, and joins them with a new piece: for let the barks be laid ever so close, or even wrapped one on the other, the old barks will never join; and it is necessary, that a piece of new bark should cement the two edges. But the juice stops at the end of the join, and it is perceptible by the extreme dryness of the two edges, (so different from the rest,) that no juice passes from one to the other.

Grafting cannot improve fruit.

There is no communication therefore between the barks of the two branches, of course the bark of the scion is pure and unalloyed. How then is grafting or budding to meliorate a fruit? I believe, that there is not any thing more certain, than that it makes no change whatever; and that there is no practise that repays so ill as that of repeatedly grafting, and cutting down plants. It must exhaust, and I have heard an excellent old gardener say, who has prac-

Repeated grafting is a bad practice.

tised

tised this art for thirty years; that he, after years of repeated trial, was perfectly convinced of this truth.

New wood.

The next thing to be observed in a graft or bud is the row of new wood round the division between the bark and wood. In the common beech for stock, the scion being the copper beech, the new wood is always of a pink colour, by which means it displays the mixture formed with the wood of the stock, which is perfectly white. See (Pl. IX, Fig 1,) a graft of one year, the scion not only increased at, p. p, the usual way, but continued rounds are added till they meet; and the whole of the stock is eradicated. There is a very peculiar appearance in the wood of the scion and graft, which well proves that they can in some measure alter the direction of their vessels, even after the regular formation of the wood; it is the undulating form, sometimes absolute twisting and turning of the vessels, which Duhamel notices, and which, he adds, strongly resembles that in glands in animal bodies after a great incision. And thence he infers, that a new sort of viscus takes place, where the two branches join; which most probably must greatly meliorate the fruit of the scion. Unless Duhamel had tasted human flesh before and after amputation; I know not how he could draw such an inference on the melioration of flavour. If he found it corrected the taste of the former, he might indeed draw the same inference in favour of the fruit. Or I should suppose it was much more natural to infer, that this undulation was caused by nature being disturbed in her office, and by the struggle the circle of life makes, to pass to the new branch, which soon however subsides, a few inches higher: and as to the new viscus, when placed in the solar microscope, it proved exactly the same wood as the scion.

Formation of the bark and wood.

There has long existed a dispute with respect to the manner in which the bark and wood are formed, which, it appears to me, dissecting grafts is the true way of elucidating and deciding. It is most plain, that the bark and wood have not the smallest connection, but that which the attaching of the flower bud to the wood occasions. I have been long of this opinion, and my present occupation has confirmed the idea. I think indeed, I have a specimen, that

that would convince the most unbelieving, and prove, that the process of the formation of the wood proceeds in this manner. When the sap begins to rise, it detaches the rind, the bark, and inner bark, in one close layer, from the wood; and they, being disposed to grow faster than the inner part of the stem, increase as much as the fastening of the flower buds to the wood will permit. The sap then forms the new wood in the intervening space, and a band is completed each year.

Lusus naturæ. I found some time ago a lusus naturæ, which teaches more than all I can say on the subject; and I have given as exact a drawing of it as I could make. *See pl.* x. *fig.* 1. On looking at some plants, I observed a Portugal laurel appeared in a very strange state; and on examining it, I perceived some accident had separated the rind, bark, and inner bark, in two regular bands, from the stem of the tree. Still, however, the ends were attached, but the loose part, from being at liberty, had so wonderfully increased in *length*, that it was more than double the measure of the piece of stem it originally covered. It had broken the trifling hold of the nourishing vessels, and that of the flower buds, which, when I found it, were perfectly dead, but still it had thrown out its leaves, and was forming fresh ones. On dissecting the leaves, there appeared no nourishing vessels, or their emptiness prevented their being distinguished; and the spiral wire was only to be found now and then, and in a broken and dilapidated state. I deeply regretted the having separated the branch from the stem, before I knew what it was. But it is still a very great curiosity, and explains well the powers of the different parts. It plainly marks, that the rind, bark, and inner bark, are the formers of the leaves; and that though they receive most part of their nourishment from the nourishing vessels which spring from the wood, yet they can expand and form without them. They did not indeed appear in perfect health, nor could it be expected, as their whole nourishment came from the dew they received, and the carbonic acid gas they inhaled. The inner bark vessels were full of the blood of the plant, and did not appear to evaporate in any manner, though one side was exposed; which shows how very complete must be the separation between the blood of the plant and its sap.

It

Difficult to attain a knowledge of the different juices of trees.

It strongly indicates too, how impossible it is to gain a thorough idea of the juices of trees, since to procure them they must be mixed; and no person can, I think, dissect trees without perceiving the astonishing pains nature takes to prevent this mixture, which would probably render futile all the intentions of nature. How then are we to judge of them, when we get them only by wounding the tree? in barking we get two juices of a very different nature, for that in the rind is purer than the sap in general, and often very bitter; then there is the juice of the circle of life, which is clammy, and approaching to syrup; and an almost plain water, that is often to be found concealed between the folds of the pith. All these should be procured singly, to be able to understand them.

Wood of grafts requires a new piece to join them.

I shall now return to the grafts: having described as minutely as possible the manner in which the two branches of the grafts join, I shall mention also, that the woods as well as the bark must have a new piece to join the wood of the stock and scion together; as will be seen in the plate. It often happens that the white undulating line before mentioned, which is the line of life, a little intercepts their meeting, but this is soon conquered; the line of life is always to be traced from the pith of the stock to the pith of the scion, as if to establish the communication of life, which adds another proof to those before adduced in my 4th letter, (*See vol.* xxiii, *p.* 334,) that it is the most important part of the plant, and truly what I have named it, the circle of life, or propagation. In looking over Duhamel, I was not a little pleased to see he had marked its consequence, but was uncertain what to call it.

Heterogeneous mixtures in grafting or budding cannot succeed.

I shall now mention the folly of expecting heterogeneous mixtures in grafting or budding to succeed, black noses, &c. That a plant should be capable of receiving its nourishment through the cylinders of another plant, is astonishing; but it must at once appear how much this miracle must be increased, if two plants are taken, which in their nature are wholly different. That such a mixture may be made by applying the powder of the stamen of one plant to the pistil of another, I know, but not in the way of graft; that

that new grafting an old tree, and cutting it down, may make it bear fruit, when it would not before, I believe; because to cut and pare a tree always infuses fresh vigour into it; for the momentary flow is more hasty, it has the power therefore of clearing away all intervening objects, that might prevent the more perfect flow of the sap; but all this is very different from allying two objects of a distinct nature. Resolved, however, not to trust any thing to reasoning alone, I grafted many of these trees myself, and got an excellent gardener to graft and bud a number. Most of them died before the year came round to cut them; but a chesnut on a palm lived much longer, though it was decaying. Some died because the juices of the circle of life coagulated with the juices of the pith on meeting; at least there was a strange substance, that had much this appearance: others died from the irregular size of the wood. But I have a number now, that I mean to give a more exact account of than I am at present prepared to do; which will illustrate this subject in a manner to leave, I hope, no doubts concerning the impossibity of joining these heterogeneous mixtures, which nature never intended should meet.

Wood.

I shall now close my letter with some observations concerning the wood. The newly inflating the dead wood is not peculiar to grafts; for it is seen in the spring in many plants, as was exemplified in one of my letters, in which I showed how the graft was revived. The dissecting of grafts has at least this advantage, it adduces fresh proof of the simplicity of the formation of the wood, and showing it to consist entirely of cylindrical passages for the current of sap. These can not only be inflated with other juices as well as their own, but the wood will remain for many months in perfect form, without decay, though they are empty.

Hydrangia.

The hydrangia exemplifies this each year in a way so curious, that it is worth laying before the public. The whole of the stalk dies away, except the cylinders of wood, the rows of pith, and the rind, so that there remains a total vacancy between the rind and wood. The bark and inner bark decay, and fall away to powder. The life dies down to the earth, and there remains in a torpid state, till a few fine days

in

in the spring revive it, and give it strength to shoot forth, and run in its few simple vessels up to the top of the dead wood. Warmer weather throws up the sap to the top of the old wood vessels, by the pressure of air below; but the vacuum existing between the rind and bark prevents the juice filling the last row or two of the wood, and gives it the appearance of a fresh rind, which I at first took it for; but on placing it in the solar microscope, I found it was the last row of the wood. If you take off the rind at this time, it will be found standing hollow, with an apparent rind skin on the wood. After a time the blood of the plant begins to form; though in what manner I have not the most distant idea, for it does not appear to me to have any connection with the root. I do not however say it has not: but I am now deeply studying this part, which is that I know the least of. I hope however to make it my winter study, as I have been collecting specimens for more than two years for the purpose, and have now a large collection of drawings from which I have not yet been able to take the results, for want of time, and the interference of grafting and budding. As soon as the blood is formed, the bark and inner bark begin to grow, and I was surprised to see, that they grew exactly in the same manner as the bourrelets in the graft (see *Pl.* IX, fig. 6 and 10.) At first I could not reconcile this form to the usual form of the growth of the bark, which is much the same in most plants; but it is its inflated appearance, that disguises its natural shape, and hides part of the lines as is seen at *d d.* It appears very evidently to prove, that there is no return of the blood of the plant: it will not then be called a circulation. I cannot therefore agree with the gentlemen, who did me the honour to notice my letters in the Panorama I think, but I have not seen it, a friend having copied this remark to send me.) "That the holes in the cylinders of the blood vessels were intended to prevent the return of the blood, or vegetable juice." Now there being so little motion in a plant is the very reason, I should suppose, why it will be the less likely to run contrary to the manner in which it was intended; and I do not believe, that there is any circulation in this part.

But

Plants and animals should not be compared.

But ingenious as the idea is, there is not any thing, I confess, that appears to me more fallacious, than the comparison between the animal and plant; or that causes more mistakes. The one has action, the other only motion; the one has volition, the other is a mere machine. It would at first sight be more to the purpose therefore, to compare it to any of our works of this kind, if the plant had not life, which must render every comparison futile. In the infant plant there is some resemblance to an animal; but it has caused more errours, and more delayed the progress of phytology, than any fashionable idea I am acquainted with. Where find a comparison for that, which is evidently a machine, but one that has life without volition; that is formed with such ingenuity as to combine its powers, extract its juices, decompose, and recompose its gasses, and deduce from a combination of the whole new life, and new beings? Mirbel alone has classed it in a manner worthy of itself. "The vegetable world," (says he,) "*is the division* between the organised and unorganised parts of creation: it gives life to the unorganised substances of the Earth, changing them into living vegetables, for the support of animal life."

I am, Sir,

Your obliged Servant,

AGNES IBBETSON.

Cowley Cot, 22d Oct.

III.

On the Defects of grafting and budding. *By* Mrs. Agnes Ibbetson.

To Mr. NICHOLSON,

SIR,

Important in grafting, that the wood of the stock and graft should be similar.

I SHALL now continue my subject. The most important part toward making grafting and budding perfectly succeed is, that the woods of the stock and scion should exactly assimilate, and be enabled intimately to connect with each other. To prove how very necessary this is to the joining of

of the two parts, I have given a sketch of woods of different patterns, to show the impossibility of their meeting and carrying on the circulation, unless they agree in every respect; and that each contains a pretty equal quantity of sap. (See *Pl.* X, *Fig.* 2, 3, 4, 5.) Conceive each of these circular apertures to be cylinders, and supposing a quantity of similar vessels laid close to each other in rows, and that for the use of some machine a double quantity of vessels was wanted, to make it double the height, to convey the liquid on through this whole series; would it not be most apparent, that the vessels so applied for the continuation of the pipes should be formed of the same size, and of the same shape, as the first? and that, if they were either larger, or smaller, the liquid would escape in pouring from one set to the other? Just so it is with the cylinders in the wood vessels of the stock and scion: except that, having no apertures from which the sap can escape, it bursts the under vessels, that should contain it.

The next thing necessary is, that the time for the flowing of the sap should be nearly the same both in the stock and scion; for every tree has its own peculiar period for this operation, and the energy with which it acts at that time is to be traced to the most distant leaf. The current is then very great, and the effort it makes is infinitely superior to its motion on any other occasion. If any obstruction has taken place before in the inner part of the tree, either by the introduction of a worm, or the piercing through of any cryptogamia (no uncommon accident) this is the clearing time; as the tree has then strength enough to force away all heterogeneous matter, that hurts or impedes its growth. If therefore the scion should advance to this period before the stock, it will want the sap to act with, and be debilitated and starved, till the time for the stock arrives, when it will have lost its energy: and if the flow of the sap takes place in the stock first, the vessels of the scion will not be ready for its reception, and it will probably burst them. I have often found it in this state. In the one case the scion appears shrivelled in its upper leaves, in the other it leaks at the graft, and decays often by slow degrees. And also the time of the flow of the sap.

The

Rain and strong sunshine to be avoided.

The third point is, that there should be no grafting or budding in rainy weather, or in very strong sunshine. The first is apt to swell the graft and buds, and overload them with moisture, so that they are killed with plethora; and too much sun dries up the bark and rind, and draws off the moisture, so that the bark draws itself up from the bark of the stock, and they are forced asunder. The gardener imagines it is owing to the thickness of the rind, whereas it is generally his own fault: many disappoint themselves, because they cannot bear to leave off, when they have once begun.

Nurseries of stocks should not be over crowded.

Great care should also be taken, that the nursery, from which the stocks are brought, is not over crowded, and yet this appears to me to be little thought of. No one, who does not dissect vegetables, or trees in particular, can have an idea how much they are the children of habit. One vessel by the accidental pressure of another, or some trifle, gets a twist, which is followed by the next, and so on by a third; the daily impression continues; the plant grows stiffer, and therefore more incapable of being righted; till the whole tree takes a spurious shape, from so trifling and minute a cause, that no one would credit it, if remembered. There is not any thing more beautiful than a well grown tree, if proper pains were taken to make it straight, and well shaped. To graft it, is certainly to give it a defect; but if well grafted, the injury is small, as it should be little increased in size at the grafting place. A grafting or budding, that can be easily pointed out six or eight years after, is badly performed; and one that enlarges the tree where it is budded is badly done. If gardeners would encourage their grafters to be double the time about it, they would find their account in it. The smallest quantity of air introduced increases instead of banishing the rot already there. In cutting 120 grafts, and 90 buds, more than 70 of the grafts had so much rot in them as could not be banished without great care; for when there is rot, if a severe frost comes, and this part is not guarded, the frost attacks it, and the decay increases. The first proof of this is the shrinking of the most distant branches.

There

There are very few grafts entirely without rot. The example I have given at *Fig.* 1, *Pl.* IX, has less defect than is usually found, the parts *f f* are trifling degrees of decay, and would soon have been banished, if kept from the air. I know but one way of doing this, which is by placing a composition on the place, when the clay or bass mat is taken off. I know none better than Foresyth's. Any composition that will keep out the air, and not crack, will do: but his having been tried, and warranted by such competent judges, must have the necessary qualities one would suppose. I knew a gentleman, who always covered his trees, whether grafted or wounded, with a plaster of this kind; and his grafts and buds were truly a picture. He did it for two or three years, according to the appearance: a bud but half the time. Within this space all danger is over. It is a method that would save thousands of trees, if adopted. If the number of grafts, that die between the 3rd and 6th year, were counted, they would perhaps be found nearly one eighth of those that survive the first operation. I have a collection of grafts between those ages, that well show the danger arising from this constant increase of rot: which will almost inevitably take place in every graft, when first performed, if not well guarded from the air: but if, when the clay is taken off, the plaster is put on, and renewed every six months for two years; or, if delicate, three; all danger would be at an end. The joining of the bark would have been renewed by that time, and taking off the plaster by degrees, the rind would be fresh and hardened.

Few grafts without more or less rot.

Remedy.

On the contrary the method of performing the operation is this. A certain time is marked out for it at most nurseries. The weather is little attended to, either in grafting or budding; because the hurry of business will not admit of such nicety. It is supposed, that, if the shoot takes, all is well: but not one in a thousand is really joined when the clay is taken off. The operator should have a common little microscope, which would show him, that they are so open as to admit air enough to destroy half, though to common observation they appear closed. In this state they are prepared for selling. Some will live two, three, or four years;

Common mode of grafting.

years; but if cut they would show, that they carry their certain death with them. The gardeners indeed may say, to manage them in this careful manner would take so much time, as would ruin us; for the price is not adequate. Certainly not; but it would better answer to a gentleman, to give half a guinea for a good tree, that would live; than buy ten for a shilling a piece, that will die just when they should bear well; and many, I dare say, would prefer it.

Decay of the apricot.

I have this year cut two to endeavour to discover the cause of the decay of the apricot, particularly the Anson, which loses a limb each year. In vain I searched every part for the defect, till I came to the graft, and then it was visible enough: for the separate parts of the wood had decayed just as it led to each of the large branches, till there were but two left. The canker very usually begins there.

Canker.

Grafted beech.

I was examining a small copper beech tree, grafted on the common, six years old. I thought it appeared sickly from the uncleanly appearance of its leaves; for it is certain, that when a tree grows unhealthy its juices grow sweeter; and the insects therefore seize it with double avidity. This had its beautiful leaves much disguised with the filth of the vermin that swarmed on it. I examined every part, till I observed a great enlargement about the graft. The rind was loose, and on making an incision, I found the bark all decayed to powder, half way up the tree; and I took from it above a pint of woodlice. On examining it had certainly arisen from the rot in the graft: which had never been well joined; and which had soon allowed these creatures to form themselves a habitation between the two barks. This spread by degrees; and had I not discovered it, the tree would soon have died; but by a proper application of the composition, and cutting away all the decayed parts, I doubt not it may do well. I was not a little surprised to see Mr. Forésyth advise the cutting three inches above the bud, which is certainly leaving great room for rot to accumulate.

Budding.

I shall now mention the difference between budding and grafting, and the reason for giving a preference to the former. There is in my opinion no comparison between them, so infinitely superior is the practice of budding. The

The chance of escaping the rot is much greater. As I prefer experience to reasoning, of 90 buds that I cut, only 30 had any rot within them; but in 120 grafts near 60 were so bad, as to require care to banish the evil from them. Then the bud stands a far greater chance of taking, for it lies much closer; and there is almost always (especially in peaches, nectarines, apricots, &c.) a couple of concealed buds, which are sure to succeed, if the middle one fails. Beside this, the line of life, which ensures their joining, is more quickly completed in the bud than in the grafts. In the former three weeks or a month will perfect it; but in the latter six weeks are the earliest time, and I have known it three months. A bad bud can hardly be so ill done as a graft. Its advantages.

It is a great pity the country practitioners will not bud their apple trees, for it is really mortifying to see what mischief they do their trees in grafting them. I have many examples by different bunglers around me, that make me deeply regret this, on account of the cider: and much of the yearly ill bearing may come from this cause: for it is making the tree so delicate, as to have its sap checked by every change of weather. People who have not studied this subject may think I exaggerate the evils, that may arise from it; but the custom that I have followed for a few years of dissecting every old fruit tree I could get, and of seeking in every dead bough for the cause of its death, has laid open to me many evils of this kind little thought of; and I hope shortly to give a letter on the decay of trees in general, that will prove I have not advanced more than experience warrants. What I have now written is not as advice to well established nurserymen; they, I doubt not, manage in a far better manner; I have not the vanity to suppose I can give counsel, where experience must be so good a master. But in this remote country with common practitioners we are certainly not so clever; and in many of the smaller nurseries about London I saw miserable examples of ill management in this art: and in orchards in general, if people would adopt budding, or, if they must graft, make use of the plaster for a year or two to their grafts; and throw the soap and water, with which they Management of orchards.

Trees should be cleaned, wash in their families, on their apple trees to cleanse them from vermin. If they would get a man once a year to scrub them with a hard brush and urine a little diluted, as one man could do a middle sized orchard in a day, the expense could not amount to 6*d* a pipe, and the quantity of cider would infinitely repay the trouble. I am acquainted with a gentleman, who tried it with one tree; and it bore every year in a surprising manner. But the quantity of cryptogamian plants, that is allowed to draw off the nourishment from our apple trees (if calculated) would scarcely be believed. All these parasite plants throw their roots into the trees, as the trees do theirs into the earth; and equally draw forth their support from them. There are many thousands on each tree; they flower and fruit in a short time; and all the juice for these purposes must be drawn forth from the tree. Does it not therefore stand to reason, that the tree will be weakened, and the fruit lessened by this draining? Proceed in your examination of the orchard: and view the lumps and excrescences to be found around the grafts. If cut, they will prove full of vermin. They also assist in draining the tree of its nourishment. We constantly manure, when we place in the ground seed, from which we expect some return. This is done in every case, except in orchards. Why should they alone give all, and receive nothing? The consequence is plain. They have what they call a good year but once in three. I once knew a butcher, who had an orchard that yielded many pipes every year. He was said by his neighbours to be a lucky man, and envied accordingly. I inquired into his manner of managing. A few times in the year he diluted some fresh blood (bullock's I believe) and poured it on the roots of his trees; covering them with a fresh layer of earth. This might take him probably four or five days in the year to accomplish; and his orchard yielded him double the quantity it was in the custom of doing before this practice. There is perhaps no gain so sure as that we give to the earth: it always returns fourfold.

Juncture of the graft and stock. I shall now endeavour to explain the sort of bolster, or bourrelet, which joins the stock and scion. It is of the greatest consequence to the tree, and influences, I doubt not,

not, the sort of tree produced; as I have before endeavoured to prove. For if the resins of the two trees mix, I have, I think, given the most convincing proof, we should experience this in the taste and appearance of the fruit. I know no two barks, that differ more than those of the plum and peach in taste and appearance. I took a fresh graft before the rind had covered it, and cutting off the bolster, I formed a very thin slice, and placed it in my microscope. The whole part represented at *n n Pl.* IX, *Fig.* 7 and 8, proved plum bark: but the under piece (*m m*) was certainly peach bark; and though I have examined it completely, as long as the rind is covering the whole, they never join. One edge lies over the other; but no vessels pass between the two, and there is no communication whatever. The fruit of the scion therefore must be pure, and wholly the produce of the peach tree, and the fruit can in no way acquire melioration from the mixture with the stock. In budding the juncture is made exactly in the same manner as in grafts. But as, in spite of appearance, the bark really never joins, it may easily be conceived how very necessary a plaster must be, till the new rind has grown over the whole; and this depends upon the thickness and height of the bolster, and therefore of course on the grafting or budding being well or ill done. If the air is allowed to enter the smallest pin hole, and the budding or grafting has left the smallest rot, the aperture will make it a serious evil.

Curious circumstance in budding.

I have observed a curious circumstance which frequently occurs respecting the side buds in budding, though never in grafting. Growing fast, they will often take refuge with their new made wood under the cover of the bark of the stock, and make the bolster between the buds: still they never join.

Fatal defect in grafting.

There is a defect in grafting, which is sure to cause death. This is when the stock and graft differ in size; and the graft is not placed even, so that the ends of the bark vessels of one come into contact with the wood vessels of the other, permitting the juice of the bark to run into the sap. Here the sap retires, and a black rottenness takes place; which proves how necessary it is, that the juices should be divided; and shows why nature has taken such

pains, to carry up all the juices separate to the flower and fruit. *(See my former letters.)*

Juncture of the wood.

As to the grain of the wood, from all the specimens I have cut it appears, that the bastard grain must be joined; and that the twisting and turning of this, when it could not join, (see *Fig.* 4, *G a*) was the cause or rather perhaps the effect, of the death of the graft. This has always been the appearance when the graft and stock did not agree. But as to the silver grain, it appears of little importance whether it joins or not: for I have seen it not meet in very perfect buds, that completely succeeded.

Shoulder grafting.

I shall now mention the different sorts of grafting. When old trees are grafted we must do the best we can: I should think shoulder grafting preferable. It would be in vain to notice the defects of that which is to make a thing worth something, that was worth nothing; though it can never make perfect trees of them. If however the plaster is put on, when the clay is taken off, and the graft is well staked; it will succeed, and make tolerably good trees.

Grafting by approach.

As to the grafting by approach, it would certainly be the best kind of grafting, but for our climate. I know a gentleman, who had practised this in India with the greatest success, and he had so completely the perfection that constant practice gives, that I was very anxious to watch the effect of his trial in this country, as he did not seem to imagine it would not equally succeed. He began; but soon found his success very different. The sap, that should have formed his grafts by making new wood for the purpose, was stopped by a few cold days. This introduced the rot into his scion. To prevent this, he next began his operation in very warm weather; but this had a bad effect on the barks, and made them recede from one another, which was sure to destroy them. In short after two years trial he was convinced, that, except for a very few plants, the sap of which was very difficult to be backened, this was not a climate for the purpose. I am not quite satisfied however to leave the result in this state. Resolved to be assured of the reason of its frequent failure, and, if it does succeed, of the poorness of the tree that proceeds from it, I have got several done by a person who in general succeeds unusually

usually well. It may be, that both bringing sap will not do so well, as it does not so completely make the stock a passage for the sap. Nothing but experiment however can prove the truth of these conjectures, which I hope to verify.

I fear the Chinese method would not do better though something different from this. (*See Trans. Soc. of Arts*, vol. xxv, p. 14, or *Journal*, vol. xxii, p. 321.) It would I think want more than two months, or even four, to strike a root fit to nourish such a branch. We know how large a root is necessary to support an infant shoot: and I much doubt whether such a branch would not bleed to death, especially in the spring, and unprepared. This time of the year, October, seems much more fitted for the trial. But I must think in one respect the gentleman must have been mistaken: that it fruited the better for the cutting. In our climate the flower bud is formed the autumn preceding its fruiting. I doubt not, that vegetation is much quicker in China; but not sufficiently hasty to produce bud, flower, and fruit, in two months; which must be the case, if the operation of abscission causes more fruit to grow on the branch, than was there when chosen. As to the fruit being larger and better for taking off the leaves of a plant, it has been too often tried, to allow of our being again deceived. We know, that the fruit decays on the tree being wholly robbed of the leaves; and how should it be otherwise? it must at once lose all the carbonic acid gas, that passes into its circulation; and all the dew, that is taken in by its leaves, is decomposed and produces the oxigen for us, and the hidrogen for the seeds. Is it therefore to be credited, that in any climate plants would be the better for such a change in their formation? I beg Marsden's pardon for believing he has made some mistake.

Chinese method.

Fruit trees not to be robbed of their leaves.

The most common way of grafting is whip grafting, or tongue grafting, which is certainly the best that can be followed in this climate. But there is in the practice of this a custom, which would be better exploded; the giving the notch. It is said to hook them to each other; but this is a great mistake; for that part is soon in a manner reduced, and I have known it frequently introduce the rot into the graft.

Whip or tongue grafting.

graft. Indeed as to fastening it, it cannot do so; for if it does not decay, it is so steeped in sap, and so watery in the new made wood, that it is impossible it can have any strength. It is very certain, that, where the knife is not absolutely necessary, it does the greatest mischief. I have seen gardeners also go on with the same knife from graft to graft, and do great damage in this way, by the decomposition of the iron, and the dirt the inside of the graft contracts. It is impossible to conceive how many of these trifles act to the serious detriment of the plant. All these things, the last excepted, budding escapes. I have seen awkward grafters put the scion smaller than the stock. This is a serious evil indeed; for the very way, that nature takes to counteract a plethory in the scion, introduces the rot into the stock. It is a very curious provision of nature; or rather perhaps the consequence of the smallness of the scion; but such a quantity of decay takes place, as will leave perfect the size of the scion (See *Pl.* IX, *Fig.* 3.) The rules of grafting may be found in any common gardening book, I mean not therefore to trouble you with them, but only with the remedies, that, if applied, might cure the defects which the cutting so many grafts has made me perceive. The stock should be cut sloping from the scion, and as close as possible, and the plaster should cover the cut as well as the graft. The buds excepted of course.

I am, Sir,

Yours &c.

AGNES IBBETSON.

Cowley Cot, 20*d October*, 1809.

EXPLANATION OF THE PLATES.

Plate IX, *fig.* 1. Section of a graft of the copper beech of the second year on a common beech of the third. *a a a a*, the circle of life leading from the pith of the stock to the pith of the scion. *b*, the pith of the stock. *c*, the pith of the scion. *d d*, the projecting pieces of the bark and rind. *f f f*, trifling degrees of decay where the wood has not united. *p p p*, the line of new wood, which is first formed by the copper beech, colouring the sap of the stock, and making it rather pink: which proves, that the new wood gains ground

Pl.9. p.ES

Fig. 1.

Fig. 4.

Fig. 5

Fig. 1.

Fig. 4.

Fig. 2.

Fig. 5.

Fig. 3.


Fig. 1.
b
b
b
b
a
c
b
a
b
Fig. 4.
2.
e
o
o
3.
o
e
Fig. 5.

ground not only from the centre to the circumference, or from *a a* to *p p*, but that it works the contrary way, or from *p* to *a*; thus exploding the stock both ways, at least with respect to the wood.

Fig. 2. A very well joined bud. *b* the pith. *a p*, *a p*, the new made wood. *c c c c*, the line of life.

Fig. 3. The manner in which the graft is cut in this country, if the scion is larger than the stock. *c* or *c*, is the manner in which the stock is left; and *m* is generally the part, that becomes rotten, to decrease the quantity of sap by degrees.

Fig. 4. A graft that disagreed with its stock. The twisting of the bastard grain and vessels in the wood of the scion from this circumstance is shown at *Gn*. They ought to have appeared as in *fig.* 5.

Fig. 6. Appearance of the bourrelet, or bolster, that joins the stock and scion, during its formation. *d d d*, the inflated parts, that tend to conceal the fibres.

Fig. 7. The manner in which the stock and scion, whether graft or bud, are joined together. *n n*, the part which is always formed by the stock. *m*, the under part formed by the scion. The part *n n* folds over this, but never joins it.

Fig. 8. A very bad graft. The letters of reference as in *fig.* 7.

Figs. 9, and 10. Oblique section of a graft. *Fig.* 9, the stock. *Fig.* 10, the scion. *p p p*, new wood. *a a*, the new circle of life. *c c*, the old, which dies away. *d d*, the beginning of the new bark, formed in the manner shown at *fig.* 6.

Pl. X. *Fig.* 1. A lusus naturæ, *c c c*, the buds. *b b b b b b*, the leaf stalks. The leaves were on it, but I was fearful of crowding the drawing. There were many stalks also full of young leaves, growing from the buds. All the flower buds were dead; but the leaf buds were increasing, and bursting out into leaf. There was every reason to presume, on dissecting the leaves, that the bark had been torn from the stems ever since the commencement of spring.

Figs. 2, 3, 4, and 5 exhibit the difference of structure in several kinds of wood, to show the necessity of the stock being of the same nature with the graft.

IV. *Experiments*

IV.

Experiments on Ammonia, and an Account of a new Method of analyzing it, by Combustion with Oxigen, and other Gasses; in a Letter to HUMPHRY DAVY, *Esq. Sec. R. S. &c., from* WILLIAM HENRY, *M. D. F. R. S., V. P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester* *.

MY DEAR SIR,

I Should sooner have communicated the account, which you are so good as to request, of my further experiments on the decomposition of ammonia, if I had not been anxious to obtain, by frequent and careful repetition of them, results not affected by any of those numerous causes of errour, which easily insinuate themselves into processes of so much delicacy. You have already been informed, that the fact, which I lately mentioned to you, (tending to prove the existence of oxigen as an element of the volatile alkali, by the discovery of oxigen gas in the products of its analysis) is not entitled to confidence, owing to the admission of a small quantity of atmospherical air, in a way which was not at all suspected. Frequent repetitions of the same process, under circumstances wholly unobjectionable, have fully satisfied me, that no portion whatsoever of oxigen gas is evolved by electricity from ammonia, even when, by means of an apparatus constructed for the purpose, the only metallic surface, exposed to the gas, consists of the sections of two platina wires, each $\frac{1}{30}$ of an inch in diameter, the wires themselves being enclosed in glass tubes which are sealed hermetically round them, and then ground away, so as to expose only the points. Nor does any difference in the nature of the products arise from electrifying the gas either under increased or diminished pressure, the latter of which, it appeared

Supposed evolution of oxigen gas from ammonia.

* Philos. Trans. for 1809, *p.* 130. This letter, in its original form, was read to the Society, May the 18th, 1809, some new observations were added, and some corrections furnished by the author, in consequence of subsequent experiments made in June; it was transmitted to the Secretary for publication, July the 10th.

to me probable, from the known influence of elasticity in impeding the combination of gaseous bases, might prevent the oxigen of the alkali from uniting with hidrogen to form water, and occasion the expansion of both into the state of gas.

Having failed, therefore, to acquire, in this way, proof of the existence of oxigen in the volatile alkali, I was next led to seek for some unequivocal mode of evincing the production of water by the same operation; a fact, which would be scarcely less satisfactory in establishing oxigen to be one of its constituents, than the actual separation of oxigen gas. The most careful observation of ammonia, during and after the agency of electricity, does not discover the smallest perceptible quantity of moisture. In order, therefore to subject the gas to a satisfactory test, I had recourse to the following contrivance. Ammoniacal gas, I had previously found, may be so far desiccated by exposure to caustic potash, as to show no traces of condensed moisture on the inner surface of a thin glass vessel containing it, when exposed to a cold of 0° Fahrenheit; though the recent gas, by the same treatment, is made to depofit water in the state of a thin film of ice. A glass globe, of the capacity of between two and three cubical inches, was filled with gaseous ammonia, which was then dried by sticks of pure potash, fastened to pieces of steel wire, so that they could be withdrawn, after having exerted their full action. This point of dryness was ascertained by applying æther, or a mixture of snow and salt, to the outside of the globe. By means of a peculiar apparatus, the gas was next strongly electrified, and the cooling power was again applied to the outer surface of the globe.

Attempt to produce water from it.

Ammoniacal gas dried by caustic potash,

and then electrified.

In the first trials, that were made with this apparatus, water certainly seemed to have been formed by the electrization of the alkaline gas; for the same portion of gas, which was not affected by a freezing mixture before the process, gave evident signs of condensed moisture, when the cooling power was applied after long continued electrization. The appearance was not only quite satisfactory to myself, but to Mr. Dalton, and several other chemical friends, to whom I showed the experiment. Finding, however,

Moisture appeared,

but varying in degree.

The experiment repeated with great precaution.

Little or no moisture.

ever, that the appearance varied as to its degree, I was induced to repeat the process with redoubled precaution; filling the globe, previously heated with hot mercury, and drying not only the quicksilver, but the iron cistern which contained it, by exposure to long continued heat. The electrified gas now betrayed no signs of moisture on the application of a temperature 20° of Fahrenheit; and gave only the smallest perceptible traces, by a cold of 0° or a few degrees below. I cannot help suspecting, therefore, that the moisture, manifested in the earlier experiments, was derived from the mercury or from some extraneous source, and was not generated by the action of electricity *.

Avidity of ammonia for moisture.

The avidity with which ammonia retains moisture, and again absorbs it when artificially dried, is very remarkable. A confined quantity of common air may be completely desiccated, in the space of a few minutes, by pure potash, or by muriate of lime; so that no ice shall appear in the inner surface of the containing vessel, when exposed to a cold of—26° of Fahrenheit. But ammonia requires exposure during some hours to potash, to stand the test even of 0° Fahrenheit; and a single transfer of the dried gas, through the mercury of a trough in ordinary use, again communicates moisture to it. Muriatic acid gas, freed merely from visible moisture, deposits no water at the temperature of 26° Fahrenheit. This is probably owing to its strong affinity for water; for electricity, after the full action of muriate of lime, evolves, as I have lately ascertained, about $\frac{1}{75}$ its bulk of hidrogen gas, the recent muriatic acid gas giving about $\frac{1}{14}$th after the same treatment †.

From

* It may be objected, I am aware, that as the gasses produced from ammonia are nearly double its original bulk, they may hold in combination any water that may have been generated by electricity. But though this supposition may explain the nonappearance of *visible moisture*, it does not account for the inefficiency of a powerful cooling cause to discover traces of watery vapour: for this is a test which renders apparent very minute quantities of water in gasses.

† In a course of experiments, which I have described in the Philosophical Transactions for 1800, it appeared that muriatic acid gas, after being dried by muriate of lime, gave nearly as much

hidrogen

From the average of a great number of experiments on the decomposition of ammonia by electricity, I was for some time led to believe, that you had rather understated the proportion of permanent gasses obtainable from it by this process, (viz. 108 measures of permanent gas, from 60 of ammonia, or 180 from 100). For the most part, I had found the bulk of ammonia to be doubled by decomposition, even when the gas was previously dried with extreme care. In one instance, a small bit of dried potash was left in the tube, along with the ammonia, during electrization, with the view of its absorbing water, which I supposed, at that time, to be generated by the process. In this case, 59 measures, (each = 10 grains of mercury) became 115. The following table shows the expansion of various quantities of ammonia.

Proportion of permanent gas obtained from ammonia,

Exp.			
1.	60	measures of ammonia, gave permanent gas	112
2.	60	- - - - -	120
3.	59	(potash being left in the tube) - -	115
4.	55	- - - - -	115
5.	75	(under the pressure of half an atmosphere)	150
5.	65	- - - - -	130
7.	65	- - - - -	130
8.	53	(one of the conductors being of steel wire)	106
	492		978

nearly double

hidrogen by electrization, as gas which had not been thus exposed. I was not however aware, at that time, of the extreme caution necessary in experiments of this kind; and was satisfied with transferring the acid gas from a large vessel, in which it had been dried, into the electrizing tube, a mode of proceeding which I now find to be quite inadmissible. The action of muriate of lime, which has undergone fusion, on muriatic acid gas, is rendered very sensible, when considerable quantities are used, by the evolution of much heat, and by a diminution of the volume of the gas. Ammonia, also, is contracted in bulk by dry caustic potafh. Muriate of lime cannot be employed for its desiccation, since this substance rapidly absorbs the alkaline gas, even when the gas has been previously exposed to quick-lime. In this case, the ammonia attracts a portion of muriatic acid from the earthy salt, agreeably to the law of affinity, which has been so ably illustrated by Berthollet.

and

and 492 : 978 : : 100 : 198·78. These proportions, you will find, correspond very nearly with those long ago stated by Berthollet *, who converted 17 measures of ammonia, by electrization, into 33 measuses of permanent gas, which is at the rate of 191 from 100 †. Having lately, however, carried on the process with the observance of additional precaution, (the mercury being first boiled in the tube, before admitting the ammonia, and still remaining hot when the gas was passed up), I have obtained from the alkali *less* than double its volume of permanent gas, viz. 280 measures from 155, or at the rate of 180·6 from 100. The variableness of the first set of results arises, I believe, from the uncertainty of the quantity of ammonia decomposed. For if the smallest portion of moisture remain in the tube, a little ammoniacal gas will be absorbed, and will be slowly given out again as the electrization goes on, thus rendering the actual quantity submitted to experiment *greater than* appears. It is probable, also, from a fact which I shall afterward state, that mercury itself, unless when heated, may absorb a small portion of alkaline gas.

But this probably above the truth.

Proportions of hidrogen and nitrogen.

The proportion of the hidrogen and nitrogen gasses to each other in the products of ammonia decomposed by electricity, I am satisfied, by recent experiments (June, 1809) is as nearly as possible what you have determined, viz. 74 measures of hidrogen gas to 26 of nitrogen. The nearest approximation I have made to these numbers is 73·75 to 26·25. Our only methods of analyzing mixtures of these two gasses, (viz. by combustion with a redundancy of oxigen) is not, I believe, sufficiently perfect to afford a nearer coincidence.

Attempt at a shorter method of analysis.

The extreme labour and tediousness of the decomposition of ammonia by electricity influenced me, to attempt the discovery of a shorter and more summary method of analysis. The most obvious one was its decompositihn by oximuriatic acid gas; but this plan was abandoned, from the impossibility of confining both the gasses by any one fluid; since water acts powerfully on the one, and mercury on the other

* Journal de Physique, 1786, ii, 176.

† Berthollet, jun. lately found the mean of a number of experiments to be 204 from 100. See the following article. C.

other. But a mixture of oxigen and ammoniacal gasses more than answered my expectations When mingled in proper proportions, these gasses, I have ascertained, may be detonated over mercury by an electric spark; exactly like a mixture of vital and inflammable air; and the results of the process, with due attention to the circumstances, which will soon be stated, afford an easy and precise method of analyzing, in the space of a few minutes, considerable quantities of the volatile alkali. With a greater proportion of pure oxigen gas* to ammonia than that of three to one, or of ammonia to oxigen than that of three to 1·4, the mixture ceases to be combustible. When the proportions best adapted to inflammation are used, oxigen gas may be diluted with six times its bulk of atmospherical air, without losing its property of burning ammonia.

A mixture of ammoniacal and oxigen gas detonated over mercury.

Atmospherical air alone does not, however, inflame with ammonia, in any proportion that I have yet tried; though, by long continued electrization with air, ammonia is at length decomposed; its hidrogen uniting with the oxigen of the air and forming water, while the nitrogen of both composes a permanent residuum. Forty-five measures of ammonia being electrified with eighty-six of common air, the total 131 became 136, and 132 after being washed with water. Of 17·2 measures of oxigen, contained in the 86 measures of air at the outset, only 2·9 were left; and these also would probably have disappeared by continuing the operation. If a mixture of ammonia and atmospheric air, each previously dried by caustic potash, and then electrified, be examined, the production of water is made sufficiently apparent on applying ether to the containing vessel. In subjecting ammonia, therefore, to this test of the generation of water by electricity, the purity of the gas from atmospheric air should be carefully determined †.

Atmospherical air does not detonate with ammonia, but decomposes it by long electrification.

* Containing only three or four per cent nitrogen gas.

† The result of this experiment shows, moreover, that, even supposing oxigen to be a constituent of ammonia, we are not to expect its evolution, in a separate form, by electricity; since, when electrified with ammoniacal gas, oxigen gas is deprived of its elastic form, and its base is condensed into water, by unic nascent hidrogen evolved from the alkali.

Products of the combustion of ammonia with oxigen vary according to the proportion of the gasses.

The products of the combustion of ammonia with oxigen vary essentially, according to the proportion of the gasses which are employed. If the oxigen gas exceed considerably the ammonia (that is, if its volume be double or upwards) the ammonia entirely disappears; and no gasses remain, but a mixture of nitrogen with the redundant oxigen. The moment the detonation is completed, a dense cloud appears *, and soon afterward settles into a white incrustation on the inner surface of the tube. The quantity of this substance, which is produced, is too minute for analysis; but its characters resemble those of nitrate of ammonia, the acid ingredient of which is probably generated by the action of oxigen on the nitrogen of one part of the volatile alkali. Accordingly, when the excess of oxigen is removed by sulphuret of lime, the nitrogen generally falls short of the proportion, which ought to accrue from a given weight of ammonia; and hence it is scarcely possible to attain, when a considerable excess of oxigen is used, an accurate analysis of the volatile alkali.

When, on the contrary, the ammonia exceeds considerably the oxigen gas, no production of nitrous acid appears to take place; for the residue, after detonation, is quite free from cloudiness. It is remarkable, however, that ammonia, when fired, in certain proportions, with less oxigen than is required to saturate its combustible ingredient, is nevertheless completely decomposed. Part of its hidrogen is sufficient for the saturation of the oxigen; and the remaining hidrogen, and the whole nitrogen of the ammonia, together with that existing as an impurity in the oxigen employed, remain in a gaseous state, and compose a mixture, which may be inflamed by adding a second quantity of oxigen gas, and passing an electric

* In some cases I have observed, that, when the cloud does not occur immediately, it may be made to appear by agitating the quicksilver contained in the detonating tube. This is probably owing to the disengagement of some ammonia, which had lodged with the mercury. The fact confirms what I have already suggested respecting the cause of the variable proportion of gasses, evolved from ammonia by electricity.

spark,

spark*. In this way all the hidrogen of the volatile alkali may be saturated with oxigen, and condensed into water; and the whole of the nitrogen may be obtained as a final result of the process. After determining the amount of the oxigen, consumed both in the first and second combustions, it is easy to calculate the quantity of hidrogen, in the saturation of which it has been employed; for when no nitrous acid has been formed, the hidrogen will be, pretty exactly, double in volume the oxigen which has been expended.

These general observations will tend to render the following experiments more intelligible. They may be divided into two classes, 1st, those in which ammonia was fired with an excessive proportion of oxigen; and 2dly, those in which the oxigen, used in the first combustion, was insufficient, or barely adequate, to saturate the whole hidrogen of the alkali.

I. *Decomposition of Ammonia by an Excess of oxigen Gas.*

Twenty-two measures and a third of ammonia were mixed with $44\frac{3}{7}$ oxigen containing 43 of pure gas. The total 67 became 34 when exploded. Water did not produce any farther diminution, but sulphuret of lime left only 8 measures. Now $34 - 8 = 26$ show the quantity of oxigen gas, which escaped condensation; and this, deducted from the original quantity (43) gives 17 measures for the amount of the oxigen expended. The last number 17, being multiplied by 2, gives 34 for the hidrogen apparently consumed. The final residue $8 - 1\cdot66$ (the nitrogen introduced by the oxigen gas) $= 6\cdot34$ is the nitrogen obtained from $22\frac{1}{7}$ of ammonia; and if to this the hidrogen be added, 40·34 measures of permanent gas will be the total result. Hence 100 measures of the gas producible from ammonia should contain 84·29 hidrogen and 15·71 nitrogen; numbers

Ammonia decomposed by excess of oxigen.

* This is analogous to what happens, when ether, alcohol, or any of the aëriform compounds of carbon and hidrogen, are exploded with a deficient proportion of oxigen; for much of the hidrogen is found in the residuum in the state of gas, and again becomes susceptible of combustion after the addition of a second quantity of oxigen. (*See Mr. Cruikshank's excellent papers in the 5th Volume of Nicholson's Journal, 4to.*)

bers too remote from those, which have been already assigned, to be considered even as approximations to the truth. The errour arises from the combination of oxigen, during combustion, not only with the hidrogen, but with the nitrogen of the alkali, the latter of which consequently appears deficient, and the former proportionably in excess.

Proportion of nitrogen defective.

Frequent repetitions of this combustion, with a considerable excess of oxigen gas, continued to give a deficient proportion of nitrogen; and as no accurate conclusions can be drawn from experiments of this kind, I shall proceed to those of the second class.

II. *Experiments, in which Ammonia was fired with a deficient Proportion of oxigen Gas.*

Ammonia decomposed with a deficiency of oxigen.

Sixty-three measures of ammonia were exploded over mercury with 33 of oxigen gas containing one of nitrogen. The total 96, when fired by an electric spark, were diminished to 57 measures, which were not contracted any farther by successive agitation with water, and with sulphuret of lime. The whole of the ammonia, therefore, was decomposed; and all the oxigen had entered into combination with the hidrogen of the alkali. The residuary 57 measures were mingled with 40 measures of the same oxigen gas, and detonated by an electric spark; after which the total, 97, were reduced to 60. The diminution, therefore, was 37 measures; and as two thirds of this number may be ascribed to the condensation of hidrogen gas, the residuary 57 must have been composed of 24·66 hidrogen, and 32·34 nitrogen. The oxigen expended, also, was 32 in the first combustion, +12·33 in the second =44·33; and this number, being doubled, gives 88·66 for the whole hidrogen saturated, supposing it to be in the state of hidrogen gas. But from the above quantity of nitrogen (32·34 measures) we are to deduct one measure, with which the 33 measures of oxigen were contaminated; and the remainder 31·34 shows the number of measures of nitrogen, resulting from 63 measures of ammonia. The total amount of gasses obtained is 31·34 + 88·66 = 120; and the proportion of the hidrogen by volume to that of the nitrogen, as 73·88 to 26·12.

To

To avoid the tediousness of similar details, I shall state, in the form of a table, the results of a few experiments out of a number of others, all of which had, as nearly as could be expected, the same tendency. The sixth experiment in the table is the one which has been just described.

Tabulated results.

No. of Ex.	Meas. of Ammon. decomposed.	Meas. of Oxigen saturated.	Meas. of Hidrogen estimated.	Meas. of Nitrogen obtained.	Hence 100 Meas. of Ammonia Take Oxigen.	Give Gas.	Permanent Gas contains in 100 Measures. Hidr.	Nitr.
1	72	47·5	95	37	66	183	72	28
2	95	64	128	46	67·5	183·3	73·5	26·5
3	100	72·2	144·4	54	72·2	198·5	72·8	28·2
4	74	51·7	103·4	37	69·8	189	73·6	27·4
5	49	33·7	67·4	25·3	68·7	180·2	72·7	27·3
6	63	44·3	86·6	31·3	70·3	193·5	73·9	26·1

Remarks.

From an attentive examination of the foregoing table, it will appear, that the results are not perfectly uniform, though perhaps as much as can be expected from the nature of the experiments. Thus the proportion of permanent gasses to the ammonia decomposed (the nitrogen being actually measured, and the hidrogen estimated by doubling the oxigen expended) may be observed to differ considerably

ably; the highest product being 198½, and the lowest 180·2, from 100 of ammonia. There can scarcely be a doubt, however, that this want of coincidence is owing to the same cause, as that which I have already assigned for the variable proportions of permanent gas, which are obtained from equal quantities of ammonia by electrization. And, accordingly, I have found, that the evolved gasses, as ascertained by combustion, bear the smallest proportion to the ammonia, when most pains have been taken to obviate the presence of moisture. The lowest number, therefore, is to be assumed as most correct; but other circumstances being considered, I believe the second experiment furnishes the most accurate data for determining the composition of ammonia. The same explanation will apply to the different proportions of oxigen gas required for the saturation of 100 measures of ammonia, the variation no doubt arising from the uncertainty of the quantity of alkaline gas which is actually burned. The proportion of *oxigen to* ammonia, which I believe to be nearest the truth, and most precisely necessary for mutual saturation, is that resulting from the second experiment, *viz.* 67½ measures of oxigen gas to 100 of ammonia, or 100 of the former to 148 of the latter.

Proportions of hidrogen and nitrogen.

It may be observed, also, by comparing the numbers in the last two columns of the table, that the hidrogen and nitrogen gasses do not uniformly bear the same proportions to each other. Notwithstanding all the labour I have bestowed on the subject, I have not been able to obtain a nearer correspondence, owing most probably to the imperfection of the mode of analysing a *mixture of hidrogen* and nitrogen gasses. In the mixture of permanent gasses, determined in this way, the hidrogen, it may be remarked, bears generally rather a less ratio than that of 74 to 26. I do not, however, consider this fact as contradicting the accuracy of the proportions which you have assigned; and it appears to me, that a sufficient reason may be given for the want of a more perfect coincidence between results, obtained by such different methods of investigation. In the products of the electrization of ammonia, the hidrogen composes nearly three fourths of the mixture: and hence its

its combustion by oxigen gas is likely to be completely effected, and the whole of the hidrogen condensed into water. But after the *partial* combustion of ammonia by oxigen gas, a residuum is left of hidrogen and nitrogen gasses, of which the hidrogen usually composes less, and sometimes considerably less, than one half the bulk. In this case it may be suspected, that a small quantity of hidrogen occasionally escapes being burned; and whenever this happens, its proportion to the nitrogen will appear to be less than the true one*.

Ammonia susceptible of slow combustion with oxigen.

From the inflammability of a mixture of ammonia with oxigen gas, it was natural to expect, that this alkali would prove susceptible of *slow* combustion. By means of a peculiar apparatus (on a plan which I have described in the Philosophical Transactions for 1808, part II †, but on a smaller scale, and with the substitution of mercury for water), I have found that ammonia, expelled from the orifice of a small steel burner, may be kindled by electricity in a vessel of oxigen gas; and that it is slowly consumed with a pale yellow flame. The combustion, however, is not sufficiently vivid to render the process of any use in the analysis of ammonia.

Mixture of ammonia and nitrous oxide extremely combustible.

With nitrous oxide (containing only 5 per cent impurity) ammonia forms a mixture which is extremely combustible. If the nitrous oxide be in excess, the proportions have a considerable range; for any mixture may be fired by electricity, of which the ammonia is not less than one sixth of the whole. The combustion is followed by a dense cloud, sometimes of an orange colour. When the nitrous oxide greatly exceeds the ammonia, (as in the proportion, for example, of 100 to 30) there is little or no diminution after firing: and the residuum is composed of a small por-

* This consideration suggests the propriety of using no more oxigen in the first combustion of ammonia, than is barely sufficient to inflame it; or if a larger quantity has been used than is required for this purpose, and a residue consequently obtained, of which the hidrogen forms only a small proportion, it is proper to add a farther quantity of hidrogen, before the second combustion. An allowance may afterward be made for this addition.

† See Journal, vol. xxii, p. 83.

tion of undecomposed oxide, some oxigen gas, and a considerable quantity of nitrogen, the last of which, however, is not in its full proportion. When the nitrous oxide is farther increased, still mose oxigen is found in the residuum.

When, on the contrary, the alkaline gas is redundant, combustion does not take place, unless the nitrous oxide forms one third of the mixture. A little diminution takes place on firing, but no cloudiness is observed; and the residue is composed of hidrogen and nitrogen gasses, with occasionally a small portion of undecomposed ammonia. As an example of what takes place, I select the following experiment from several others.

Results of an experiment.

A mixture of 41 measures of ammonia with 40 of nitrous oxide (= 38 pure), in all 81 measures, were reduced by combustion to 75, which were found to consist of 16 hidrogen and 59 nitrogen gasses.

Explanation of it.

To explain this experiment, we may assume (as is consistent with your own analysis*) that 100 measures of nitrous oxide are equivalent to 52 measures of oxigen gas and 103 of nitrogen. The oxigen in 38 measures of nitrous oxide will, therefore be 19·7, to which, when the oxigen spent in burning the residuum (viz. 8 m.) is added, we obtain 27·7 for the total oxigen consumed; and multiplying by 2, we have 55·4 for the hidrogen saturated. From the residuary nitrogen (59) deduct 39 measures arising from the decomposition of the nitrous oxide + 2 m. mingled with it as an impurity = 41, and the remainder, 18 measures, is the nitrogen resulting from the volatile alkali; and as 41 measures of ammonia give 55·4 + 18 = 73·4 measures of permanent gas, 100 would give 179 measures, in which the hidrogen and nitrogen would exist in the proportion of 75·4 to 24·6. From the same facts it may be deduced, that 100 measures of ammonia require for saturation 130 of nitrous oxide = 67½ oxigen gas.

Confirms the former analysis.

The coincidence then, between the results of the combustion of ammonia with nitrous oxide, and those with oxigen gas, confirms the accuracy of both methods of analysis.

* Researches, Res. ii, Div. 1, or Thomson's System of Chemistry, 3d. edit. ii, 143.

Nitrous

Nitrous gas, which, it appears from your testimony*, does not compose an inflammable mixture with hidrogen, (nor, as I am assured by Mr Dalton, with any of the varieties of carburetted hidrogen) may be employed, I find, for the combustion of ammonia. The proportions required for mutual saturation are about 120 measures of nitrous gas to 100 of ammonia. An excess of the former gas does not give accurate results; since not only the hidrogen of the ammonia, but some of its nitrogen is also condensed; and the mixture, after being fired, exhibits the cloudy appearance usual in that case. Proportions for mutual saturation.

Forty-eight measures of ammonia, being fired with 60 nitrous gas, (= 53 pure) both gasses were completely decomposed; and a residue left consisting of 61 nitrogen and 9 hidrogen. Sixty measures of ammonia and 41 nitrous gas (= 36·1 pure) gave, after firing, a mixture composed of 10 ammonia, $53\frac{1}{2}$ nitrogen, and $30\frac{1}{2}$ hidrogen. But taking for granted that 100 measures of nitrous gas, according to your analysis, hold in combination a quantity of oxigen equal to $57\frac{1}{2}$ measures of oxigen gas, and of nitrogen equal to $48\frac{1}{2}$ measures; and assuming the proportions of the nitrogen and hidrogen in ammonia, to be those established by your experiments and my own: it will appear from an easy calculation, that the proportion of nitrogen, in the above residua, a little exceeds, and that of the hidrogen rather falls short of what might have been expected. I have not yet been able to reconcile these differences by the numerous trials required in a process of so much delicacy; and I reserve the inquiry for a season of more leisure. The foregoing statement I wish to be considered as merely announcing the general fact of the combustibility of a mixture of ammonia and nitrous gas, a property which chiefly derives importance from its being capable of application to a new method of analysing the latter. Results of different mixtures.

Before concluding this letter, I shall briefly state the results of some experiments, which I have lately made in conjunction with Mr. Dalton, on a subject that formerly oc- Effects of electricity on aeriform compounds of car-

* Researches, p. 136.

 cupied

bon and hidrogen. cupied much of my attention; viz. the effect of electricity on the aëriform compounds of carbon and hidrogen. Subsequent reflection, as well as the candid and judicious criticisms of various writers*, have influenced me to doubt of the accuracy of a few of the conclusions drawn from my former inquiries †. The knowledge of this class of bodies has, also, been so materially advanced during the last twelve years, that the examination of their properties may now be undertaken with much greater confidence of success than formerly. It is to be lamented, indeed, that experimentalists do not oftener retrace their labours, with the combined advantages of acquired skill, and of a more improved state of the science which they investigate.

Gasses experimented on.

The gasses, submitted by Mr. Dalton and myself to the action of long continued electrization, were carburetted hidrogen from pit-coal of the specific gravity of about 650 (air being 1000); olefiant gas, and carbonic oxide. Each gas was used in as pure a state as possible; muriate of lime being first introduced into the same tubes in which the gasses were electrified, and being withdrawn when it had exerted its full action. Platina wires were used to convey the electric discharges.

Carburetted hidrogen and olefiant gas.

When the electrization of carburetted hidrogen or olefiant gas was continued sufficiently long, they were each found to expand, notwithstanding their extreme dryness. No carbonic acid could be discovered in the electrified gas by the nicest tests. When fired with oxigen, it gave less carbonic acid than the unexpanded gas, and required less oxigen for saturation. Calculating, from the diminished product of carbonic acid, how much gas had been decomposed by electrization, it appeared that the decomposed part, in all cases, was about doubled. The smaller product of carbonic acid from the electrified gas was sufficiently explained by a deposition

* See Berthollet's Chemical Statics, Eng. trans., Vol. II, p. 454; Murray's Elements of Chemistry, Vol. II, Note G; a letter from an anonymous correspondent in Nicholson's Journal, 8vo, II, 185; and Aikin's Dictionary of Chemistry, I, 251.

† "Experiments on Carbonated Hidrogen Gas, with a View to determine whether Carbon be a simple or a compound body." Phil. Trans. Vol. LXXVII.

sition of charcoal on the inner surface of the glass tube, too distinct to be at all equivocal, and most abundant from the olefiant gas. No addition whatsoever of nitrogen was made by the electrization. It appears, therefore, that the hidrocarburetted gasses, like ammonia, are separated by electrization into their element, the carbon being precipitated, and the hidrogen evolved in a separate form, and acquiring a state of greater expansion. This change, however, is effected much more slowly, than the disunion of the elements of ammonia.

From a portion of carbonic acid gas, carefully dried by muriate of lime, and electrized with platina conductors, we obtained, after removing the undecomposed gas by caustic potash, a residuum equal to about one twentieth of the whole gas which had been employed. It was found on analysis to consist of oxigen and carbonic oxide gasses, in such proportions as to inflame on passing an electric spark through it without any addition, and to be thus convertible again into carbonic acid. In the experiments of Mr. Saussure, jun.*, that ingenious philosopher obtained only carbonic oxide by the same operation, owing doubtless to the electricity having been conveyed by conductors of copper, which would become oxidized, and prevent the oxigen from being evolved in a separate form. Carbonic acid gas.

Carbonic oxide, electrified with similar precautions, did not appear to undergo any change. Eleven hundred discharges from a Leyden jar had no effect on a quantity of the gas, equal to about one tenth of a cubic inch. Its bulk, after this process, was unaltered; no carbonic acid could be discovered in it; and there was no decided trace of oxigen gas in the residuum. The carbon, it appears, therefore, which exists in carbonic oxide, must be held combined by an extremely strong affinity. Carbonic oxide not decomposed.

With sincere esteem and respect, I am,

Dear Sir,

Your faithful and obliged friend,

WM. HENRY.

* Journal de Physique, Tom. LIV, p. 450.

VI. *Observations*

Observations on the Com
MR. BERTHO

Oxigen sought for in ammonia.

THE object of Mr. Bertholl gen, which, according to Mr the proportion of 20 per cent

Expansion of ammoniacal gas by electrification.

He repeated by more direct Mr. Davy. He ascertained t gas, when, from the effect of long time, its elements have city. The analysis of the ga this operation afterward show of the substances composing i ber of experiments indicates, posed by the electric fluid, its

Consists of hidrogen and nitrogen only,

100 to 204‡; and that the ga hidrogen, and 245 azot. Ca power of the gasses, Mr. Bert of ammonia by weight, prod and that their proportions by azot 81·13.

and no oxigen, unless contained in these.

The following is his infe Ammonia is composed of hidr can be found in it, unless by should be able to extract oxig ways been considered as pure

Ammonia decomposed by heat in a porcelain tube.

The gas collected by decom tube of porcelain contains t

* Annales de Chimie. Augus

† " Seven or eight, at least; *for* 1808, p. 40; *or Journal,* vol

‡ This exceeds all Dr. Henry' one. Mr. B. too makes the prop either Dr. Henry, or Mr. Davy.

§ It would appear by Mr. I vol. xxiii, p. 242. &c. that nitro likewise; and these will be fart appendix to his former paper, wh

gen and azot as the preceding. In an experiment of this kind, when twenty quarts of ammoniacal gas were decomposed with every precaution for condensing the water, that should have been formed if ammonia contained ⅕ of oxigen, none was obtained.

No oxigen in the decomposition by electricity.

The decomposition by the electric spark shows no trace of humidity, or of oxidation, when an iron wire is employed; yet one or other of these effects must inevitably take place, if there were any oxigen in ammonia.

VI.

Analysis of the Chinese Rice-Stone, with some Observations on the Yu; by MR. KLAPROTH *.

Rice-stone an artificial product,

THE rice-stone, of which the Chinese make cups, and other vessels, which are occasionally brought to Europe, is an artificial production, the component parts of which are yet unknown. Authors are by no means agreed on the origin of its name. Storr informs us, that many collectors of curiosities in Holland assured him it was actually prepared from rice, which was hardened by the addition of other substances. Bruckmann on the other hand supposes, that it received its name from its resemblance to transparent rice. It has also been considered by some as alabaster: by others as chalcedony, or one of its varieties, cacholong; and lastly as the problematic stone, or yu, which will be mentioned hereafter. Mr. Kratzenstein, of Copenhagen, has at length ascertained what this substance is, and gives the following description of a cup.

said to be prepared from rice.

A fusible glass.

This substance is a fusible glass, resembling in colour a white jelly. It is formed in a mould of two pieces, while it is soft. It is ornamented with figures and handles in relief. The sharp edge occasioned by the mould was still observable. It is so hard as to scratch glass, and is much more difficult to cut than marble. Its fracture has a dull lustre, like that of starch jelly dried. Its colour and transparency much resemble alabaster.

* *Annales de Chimie*, vol. lxix, 302. From *Gehlen's Journal.*

Crell

Examined by Crell. Crell subjected the rice-stone to several chemical experiments in 1781, to find whether it contained rice or its mucilage. He exposed some pieces to a strong red heat in a small crucible, but found no indication of any volatile matter, animal or vegetable. The pieces were agglutinated together, and adhered to the bottom of the retort: and the substance still retained its colour and semitransparency, as before the experiment, and had undergone no loss.

Analysed. As no farther experiments on the component parts of the rice-stone have been published, its nature has still remained unknown. I have attempted an analysis of it on a very small quantity; and though this analysis is not very exact, it is sufficient to throw some light on the composition of the stone. The portion analysed was taken from a vessel with two handles, weighing 12 ounces. From its external appearance it might be taken for a greenish gray chalcedony, as well on account of its polish and transparency as of its colour: but the sound it emitted when struck, and still more its specific gravity, which was above double that of chalcedony, since it was 5·3936, showed unquestionably, that it was not this stone.

Specific gravity of a specimen.

Hardness. It is easily attacked by the file. It breaks easily, with a conchoid fracture, and glassy lustre. In a small spoon before the blowpipe it readily fuses into a small bead; but on a piece of charcoal this bead is covered with a leaden gray pellicle. Borax and the phosphoric salts difficultly combine with it: but if it be fused in the small platina spoon with carbonate of soda, immediately small globules of metallic lead appear. Acids do not act on this stone; accordingly I attacked it by alkalis in the following manner

Treated before the blow-pipe.

Silex. *a.* One hundred grains of this stone reduced to an impalpable powder were heated red hot with potash. The mixture became hard, and acquired an ashen gray colour. On supersaturating it with nitric acid silex was separated to the weight of 39 grains.

Oxide of lead. *b.* Sulphate of soda being added to the solution, sulphate of lead was precipitated, weighing 55 grains, which indicate 41 grains of oxide of lead,

c. The

c. The liquor separated from the sulphate of lead, being mixed with ammonia, yielded 7 grains of alumine. Alumine.

An addition of carbonate of ammonia produced no farther alteration.

Thus 100 parts of the rice-stone gave Its component parts.

Oxide of lead - - - - - -	41
Silex - - - - - - - -	39
Alumine - - - - - - -	7
	87

It may fairly be presumed, that the 13 parts deficient were some vitrifying principle, either borax, soda, or potash: but the small quantity sacrificed for this analysis did not allow a repetition of experiments. Some saline flux.

From the results of this analysis it appears, that the pretended stone or paste of rice is nothing but a siliceous glass of lead, to which a resemblance of chalcedony is given by means of alumine. But it is not necessary, to employ alumine purified by art for the preparation of this glass. It is even very probable, that the Chinese employ feldspar, or petuntze, with the properties of which they are well acquainted, since it is with this substance and kaolin, that they make their porcelain. Its fabrication.

Preliminary experiments have shown me, that a substance similar to the rice-stone may be prepared, by fusing together 8 parts of oxide of lead, 7 of feldspar, 4 of common white glass, and 1 of borax: or by employing 8 parts of oxide of lead, 6 of feldspar, 3 of silex, and 3 of borax, potash, or soda. Confirmed by experiments.

It appears, however, that no determinate proportions of oxide of lead are observed in the preparation of the rice-stone. Hence the specific gravity of this stone varies considerably, so that several other stones which I tried, or which have been examined by others, were near a third lighter than the stone I analysed. The specific gravity of one small cup resembling the former, but ornamented with antique Chinese figures, I found to be 3·68; that of several fragments of a thin vase, 3·635; that of a drop for the ear, in the shape of a long pearl, and labelled "oriental nephritic stone", 3·58. Crell found the specific gravity of a rice- Variable in the proportion of ingredients. Different specific gravities.

a rice-stone vase, preserved in the collection of natural history at Brunswick, 3·768; that of another small cup, 3·5; and that of the pieces he made his experiments on, 3·75.

The yu perhaps artificial.

As to the *yu* it is known only from the memoirs of the *Pekin Missionaries*. It is surprising, that a stone so much vaunted on account of its beauty, hardness, and the sound it emits when struck, should be unknown in Europe. Mr. Hager has given a description of a vase preserved at Paris, which he supposes to be made of this stone: but its being so is very doubtful; and we may even presume from his description, that it is merely an artificial production analogous to the rice-stone. The missionaries indeed speak of it as a natural production: but the sonorous property of the stone leads to the conjecture, that it is nothing but a vitreous composition. Though we know several sonorous stones, as the *klingstein, or porphir-schiefer*, and the sonorous quartz crystals of Pricborn, the sounds they emit are not comparable to those of the *yu;* nor can instruments of music be made of them, as of this stone. We cannot however absolutely deny, that sonorous stones are found in China, of which instruments of music are made: for a proof of this is found in a Chinese *king*, in the collection of Mr. Bertin, at Paris, an analysis of which was published by the duke de Chaulnes, who found it to be a black bituminous marble.

But this by no means certain.

Sonorous stones,

known to the ancients.

Pliny, lib. 37, *cap.* 10, mentions a black stone, as sonorous as brass, by the name of *chalcophonos*, which was given it because it sounded like brass when struck.

VII. *On*

VII.

On the Effect of westerly Winds in raising the Level of the British Channel. In a Letter to the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. P. R. S. By* JAMES RENNELL, *Esq. F. R. S.**

DEAR SIR,

IN the "*Observations on a Current that often prevails to the Westward of Scilly*," which I had the honour to lay before the Royal Society many years ago, I slightly mentioned, as connected with the same subject, the effect of strong westerly winds in *raising* the level of the British Channel; and the escape of the superincumbent waters through the Strait of Dover, into the *then* lower level of the North Sea. Strong westerly winds raise the level of the Channel.

The recent loss of the Britannia East India ship, Captain Birch, on the Goodwin Sands, has impressed this fact more strongly on my mind; as I have no doubt that her loss was occasioned by a current, produced by the running off of the accumulated waters; a violent gale from the westward then prevailing. The circumstances under which she was lost, were generally these: Loss of the Britannia,

In January last she sailed from her anchorage between Dover and the South Foreland (on her way to Portsmouth), and was soon after assailed by a violent gale between the west and south-west. The thick weather preventing a view of the *lights*, the pilot was left entirely to the reckoning and the lead; and when it was concluded, that the ship was quite clear of the Goodwin, she struck on the north-eastern extremity of the southernmost of those sands. And this difference between the reckoning (after due allowance being made for the tides) and the actual position, I conclude was owing to the northerly stream of current, which caught the ship when she *drifted* to the *back*, or *eastern side* of the Goodwin. owing to a northerly current from the Strait of Dover.

The fact of the high level of the Channel, during strong winds between the W. and SW., cannot be doubted: be-

* Philos. Trans. for 1809, p. 400.

cause

cause the increased height of the tides in the southern ports, at such times, is obvious to every discerning eye. Indeed, the form of the upper part of the Channel, in particular, is such as to receive and retain, for a time, the principal part of the water forced in; and as a part of this water is continually escaping by the Strait of Dover, it will produce a current; which must greatly disturb the reckonings of such ships as navigate the Strait, when thick weather prevents the land or the lights of the Forelands, and the North Goodwin, from being seen.

Effects of SW. winds on the tides in the Channel.

I observe in a new publication of Messrs. Lawrie and Whittle, entitled "*Sailing Directions &c. for the British* "*Channel*, 1808," that throughout the Channel, it is admitted by the experienced persons whom he quotes, that strong SW. winds "cause the flood tide to run an hour, or more, "longer, than at common times: or in other words, that *a current overcomes the ebb tide a full hour:* not to mention how much it may accelerate the one, and retard the other, during the remainder of the time *.

Direction of the current from the shape of the shores of the Channel.

It is evident, that the direction of the current under consideration will be influenced by the form and position of the opposite shores, at the entrance of the Strait; and as these are materially different, so must the direction of the stream be, within the influence of each side, respectively. For instance, on the English side, the current having taken the direction of the shore, between Dungeness and the South Foreland, will set generally to the north-east, through that side of the Strait. But, on the French side, circumstances must be very different: for the shore of Boulogne, trending almost due north, will give the current a like direction, since it cannot turn sharp round the Point of Grisnez, to the north-eastward; but must preserve a great proportion of its northerly course, until it mixes with the waters of the North Sea. And it may be remarked, that the Britannia, when driven to the eastward of the Goodwin, would fall into this very line of current.

There

* It is also asserted, that in the mouth of the Channel, the extraordinary rise of tide, in stormy weather, is ten feet: that is, at common springs, twenty, and in storms thirty feet. *See pages* 28, 41, 70, and 133.

5

There is another circumstance to be taken into the account; which is, that the shore of Boulogne, presenting a direct obstacle to the water impelled by the westerly winds, will occsaion a higher level of the sea there than elsewhere; and of course a stronger line of current towards the Goodwin.

It must, therefore, be inferred, that a ship, passing the Strait of Dover, at the back of the Goodwin Sands, during the prevalence of strong W. or SW. winds, will be carried many miles to the northward of her reckoning; and if compelled to depend on it, may be subject to great hazard from the Goodwin.

Dangerous consequence of this.

It will be understood, of course, that although the stream of current has been considered here (in order to simplify the subject), yet that, in the application of these remarks, the regular tides must also be taken into the account. But from my ignorance of their detail, I can say no more than that I conceive, that the great body of the tide from the Channel must be subject to much the same laws, as the current itself. The opposite tide will donbtless occasion various inflexions of the current, as it blends itself with it; or may absolutely suspend it: and the subject can never be perfectly understood, without a particular attention to the velocity and direction of the tides in moderate weather, to serve as a ground-work *.

The regular tides subject to much the same laws as the current.

I am, with great respect,
Dear Sir,
Your faithful humble Servant,
J. RENNELL.

VIII.

On dead Lime, by MR. BUCHOLZ †.

IT has long ago been said, that under certain circumstances which are not yet well ascertained, and particularly after a violent

* Meffrs. Lawrie and Whittle's publication allows the tides in this quarter a velocity of one mile and a half per hour at the springs; half a mile at the neaps. The Britannia's accident happened at *dead* neaps.

† *Journal des Mines*, vol. xxii, p. 234.

Lime that will neither heat nor slack.

a violent and long continued fire, limestone may be convert into a kind of lime that does not heat with water, and do not slack; and this has been called *dead lime*. This sta of lime does not appear to be recognized by all chemis since it is not mentioned in elementary treatises on chemistr some however think, that clay combined with lime may gi it the property of hardening by great heat, and thus ca it to lose those of heating and slacking with water, givi rise to dead lime. Perhaps I may remove the uncertainti and reconcile the different opinions, to which this substan has given rise.

May be owing

Four cases may be supposed, to each of which lime m pass to this state.

to alumine,

1. When it contains a great deal of clay, and is heat so strongly as to become very hard. In this state it will n effervesce with acids, because all the carbonic acid is expelle

silex,

2. If it contain silex, and be heated strongly after t complete expulsion of the carbonic acid, it will not efferves with acids.

too hasty and strong a fire,

3. In the third case, lime being heated at once very vi lently, forms a mass perfectly similar to dead lime. It pass into a semifluid state, the possibility of which I have show in the Berlin Journal, and it requires to be heated graduall to expel the carbonic acid of large pieces in particula When the kiln is emptied, there will remain pieces hal fused, that will neither heat nor slack with water, but effe vesce with acids: these are carbonate of lime, fused hardened by the fire.

or fire too long continued.

4. Lastly on calcining carbonate of lime in a fire cont nued long after the expulsion of the carbonic acid, a tr dead lime is formed, which neither heats with water, n effervesces with acids. All the circumstances of its form tion are not yet well known.

Dead Lime from oyster-shells.

I saw some years ago this kind of lime formed by the c cination of chalk and of oyster-shells; but not being sati fied, that they contained neither silex nor alumine, I ascrib to these earths the peculiar properties of the lime obtaine Lately the same phenomena occurred to me, and as I w very certain, that the oyster-shells employed contained n earth but lime, no phosphate of lime, and no salt soluble i

wate

water, I can affirm, that the properties of the lime obtained were altogether independent on the presence of these circumstances: yet I obtained from the same shells, by a somewhat more gentle heat, common caustic lime easily slacked.

Its properties.

The dead lime obtained heated very strongly with muriatic acid diluted with a small quantity of water, without emitting the smallest bubble of carbonic acid. Pieces of it remained in water for twenty-four hours without falling to powder; and notwithstanding this common lime-water was formed, which is very remarkable. When the calcined oyster-shells were thrown into a boiling lixivium of carbonate of soda, the soda was completely decomposed, and a very fine pap was formed.

If this account be not sufficient to throw much light on the subject in question, it will serve at least to guide the reflections of men of science, and show how different opinions on the existence of dead lime may be reconciled.

IX.

On the Muriates of Barytes and of Silver; by BERTHIER, *Mine Engineer* *.

Proportion of muriate of silver mistaken.

IN a paper on the sulphate of barytes &c. † I took it for granted, that muriate of silver contained 20 per cent of muriatic acid, and hence I deduced the composition of the muriate of barytes, which I afterward employed in my inquiries concerning the sulphates. Recent experiments having taught me, that this supposition was not strictly accurate, I endeavoured to ascertain with more precision the proportions of the muriates of barytes and silver.

Muriate of barytes formed.

I put 10 gr. [154 grains] of barytes recently obtained from the calcined nitrate into a glass stoppled phial filled with water. 0·4 of a gr. of carbonate of barytes remained undissolved. The solution was supersaturated with some muriatic acid, and evaporated to dryness. The residuum, calcined in a platina crucible, weighed 12·75 gr. It contained 3·15 gr. of muriatic acid, since I had employed

* *Journal des Mines*, vol. xxii, p. 323.

† *See Journal*, vol. xxiii, p. 280.

9·60

9·60 gr. of caustic barytes. Calcined muriate of barytes therefore is composed of

Its component parts, when calcined,

barytes 0·753 muriatic acid 0·247

I formed anew some muriate of barytes from its component principles, and crystallized it. 10 gr. lost 1·5 gr. by calcination, consequently the crystallized salt contains

when crystallised.

barytes - - - -	0·64
muriatic acid - - -	0·21
water - - - -	0·15
	1·00

Muriate of silver,

Five gr. of artificial muriate of barytes calcined, containing 1·235 gr. of acid, were precipitated by nitrate of silver; and the result was 6·75 gr. of muriate of silver. This salt therefore contained

muriatic acid	0·183, and as it contained
silver - -	0·750, there remain
	0·067, which must represent the *oxigen*.
	1·000

Proust's proportions

Mr. Proust has found, that 100 parts of silver always produce 133 of muriate; and on precipitating a salt of silver by lime-water, he found, that the precipitate was composed of 0·905 silver, 0·01 lime, and 0·085 oxigen.

for oxide of silver,

Hence he concluded, that the oxide of silver contained

silver - - - -	0·909
oxigen - - - -	0·091
	1·000

and muriate of silver.

and the muriate of silver,

silver - - - -	0·751
acid - - - -	0·180
oxigen - - - -	0·069
	1·000

It is difficult in chemistry to obtain results agreeing more nicely.

Sulphate of barytes.

8·5 gr. of calcined muriate of barytes, which I had prepared, were precipitated by sulphate of soda, and produced 9·55 gr. of sulphate of barytes, a quantity differing very little from what I had before found; and whence it followed, that the sulphate contained 0·345 of acid *. The proportions of the muriate, which I have just ascertained, prove, that the sulphate of barytes cannot contain more than 0·33 of acid.

* This was the largest proportion shown by Mr. Berthier's analyses. C

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